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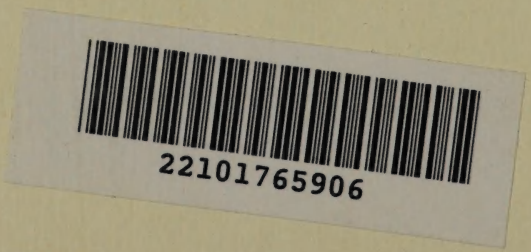

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OF

PRACTICAL MEDICINE AND SURGERY

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JOHN FORBES M.D. F.R.S. F.G.S.

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The Moral Treatment, Hygiene, and Education of Idiots, and of other Children backward or retarded in their development, agitated with involuntary Movements, Weak, Dumb, but not Deaf, Stammering, &c. By Edward Séguin.—*Paris*, 1846. 8vo, pp. 734.

2. *Remarks, Theoretical and Practical, on the Education of Idiots and Children of Weak Intellect.* By W. R. Scott, ph.d., Principal of the West of England Institution for the Education of the Deaf and Dumb. —*London and Exeter*, 1846. 8vo, pp. 46.

M. Séguin informs us in his introduction that he has studied idiotcy for ten years, that owing to a report by Pariset on his method, and to the encouragement and approbation of the Academy of Sciences, he was intrusted with the education of the young idiots in the Paris hospitals;—that his work is entirely new, for though encouraged by Itard and Esquirol, he was compelled to find his resources in himself;—that in his investigation he was forced to inquire into the plans of education, both intellectual and physical, most in vogue, and to weigh their merits by applying them to practice. We learn from the book itself, that M. Séguin is not a physician, but a teacher;—that, instigated by M. Itard, he has devoted himself wholly to the education of idiots; and that to effect this, the teacher must carry out his plans himself with the most laborious painstaking. The education of beings so imperfect in body as well as in mind, taught him in language not to be mistaken that the due performance of the healthy bodily functions is essential to the action of the mind, and that

every complete system of education embraces physical as well as intellectual and moral training. The rules of Hygiene must therefore be included in any complete treatise on Education. The relative importance of M. Séguin's views are in accordance with the attention he has bestowed on the various branches of the subject. The descriptions of the habits, and outward peculiarities of idiots, as well as their mental characteristics, are original and masterly; so are the rules for physical and moral training; the medical portion, and especially that which might be enlightened by pathological anatomy is meagre and unsatisfactory, whilst the hygiene and physiology are but common-place. Non omnia possumus omnes. To be a practical physician requires the exclusive devotion of a life, and we do not expect, from one who lays himself out for the work of education, anything more than a general acquaintance with subjects beyond his own province.

Dr. Scott's pamphlet, although creditable both to the head and heart of the author, contains nothing of importance not found in greater detail in M. Séguin's book. We shall not, therefore, take any special notice of it in the following pages. We had prepared an analysis of the description of idiotcy, its varieties, its physiological and psychological symptoms, its causes, diagnosis, and prognosis, and the rules for hygiene; but on finishing the whole, we found that the article was so bulky as to be beyond the space we could devote to the subject. The only mode of curtailing it seemed to be to omit this preliminary portion, and to insert only the analysis of the plan of education, physical, moral, and intellectual, for this plan is so dependent in its parts that it is impossible to omit any link without sacrificing its practical use. We regret omitting his descriptions of idiotcy, for they come from an observant and practised eye, guided by a thoughtful mind. Every page shows that the writer has lived with his subject, and has given to it all the devoted attention of an energetic nature. He writes from his own stock, not loose generalities, the result of reading. His style is not classical, but on the other hand it has a freshness better than mere tame correctness.

Before entering on the subject of education, there are, however, two or three points to be alluded to. The first is the definition of idiotcy.

What then is idiotcy? Idiotcy is a disordered state of the nervous system, owing to which there is no regular command of the will over all or part of the organs and faculties of the child, who is guided by his instincts, and cut off from the moral world. The type of an idiot (an ideal idiot?) is an individual who knows nothing, wishes nothing, and can do nothing, and every idiot approaches more or less to this *summum* of incapacity.

Psychological symptoms of idiotcy. Though confined in narrow limits the idiot still has sensations, sentiments, and limited perceptions, manifested by his wants, appetites, tastes, inclinations, desires, repugnances, fears, terrors, preferences, wishes, all expressed in his own way. He gives evidence that he remembers perceptions of his senses, that he can compare these past sensations with present ones, and that he can look forward and judge of the future from the past though this power is very circumscribed. That which is wanting in the idiot is not then distinct perception, as he can distinguish his food; nor internal nor external sensations,

nor attention, as he can fix it and his wishes on those things which please him; nor comparison nor judgment, as he can choose among many things that which pleases him most; nor understanding, when he submits to the moral influence of mild or severe language; nor to foresight of his wants and appetites, for though few, yet he will use great patience and ingenuity to gratify them; nor tastes, for although limited to the desire of tearing rags or licking a piece of crockery, they occupy the mind as completely as the taste for dancing or gaming occupies higher minds; nor personal affections or antipathies; nor even will itself in the narrow limits of willing to satisfy his instincts or to gratify his indolence. What then does he want intellectually to resemble the rest of the world? "Il ne lui manque," says M. Séguin, "aucune faculté intellectuelle; mais il n' a pas la liberté nécessaire pour appliquer ses facultés dites intellectuelles à l'ordre des phénomènes moraux et abstraits; il lui manque la synergie, la spontanéité d'où jaillit la volonté morale." By which we take M. Séguin to mean that the idiot has not that freedom or power of the will by which he can of himself originate intellectual or moral actions. The reader will perhaps more clearly comprehend the meaning when he has read the whole article.

Idiotcy is greatly aggravated by neglect. All those cases, without which, children well gifted could not have attained the dignity of manhood, are withheld: no kind forethought, no enlightened affection, no plan of education, no methodical treatment. Poor or rich, with affectionate or careless mothers, they are equally doomed to ignorance and inaction; with no assistance, no amusements, no excitements to thought, feeling, or action. Until left to their instincts, their inertness, their nervous disorders, and filthy and repulsive habits, they become incurable, and growing old prematurely die at thirty. Neglect makes their youth a season of decrepitude which leads them rapidly and painfully to their graves. We must refer to M. Séguin's own pages for his indignant remonstrances against those who thus condemn the ignorant to ignorance, the inert to inertia, the idiot to perpetual idiotcy.

The common opinion that idiotcy is incurable, is false, says M. Séguin. This opinion has arisen, 1st, from the incapacity of medical men to form a judgment on this subject from not having specially studied it, and from their hasty visits, a rapidity which prevents any attentive study, or any attempt at moral treatment by personal means. 2. The unwillingness of parents to vex the idiot by a change of habits which at first make him sad, vexed, or produce tears; for the idiot, though he cannot tell black from white, has always a marvellous intuition as to the degree of ascendancy he exercises over each person. 3. Others become after a time indifferent. 4. Religious prejudices, by which some people regard the idiot as incapable of sin.

But, notwithstanding these impediments, idiots have been made good, active, intelligent, and to a certain point useful to their relatives and to society; and it is impossible to say *à priori* what cases are incurable. The trial must first be made. Neither the smallness of the head, nor the hydrocephalic enlargement are signs on which any opinion as to the result can be formed. The most unpromising cases are those attended with general paralysis, or with hemiplegia, chorea, and epilepsy. But it is only after the failure of assiduous means that even these cases can be pronounced

incurable ; for proper treatment often ameliorates or removes these complications, and thus benefits the disease.

THIRD PART.—THE EDUCATION OF IDIOTS.

In the education of the blind, and of the deaf and dumb, one sense is substituted for another : the touch instead of the sight in the blind, and the sight instead of the ear in the deaf and dumb ; but idiots have senses although dormant. They hear, and do not understand ; see, and do not perceive. They are thus disconnected from the outer world, and the principle involved in their education is to awaken their attention. Their senses must be roused so as to convey impressions to their brain by a system of sensorial gymnastics, and their brain must be stimulated so as to receive and to react on these impressions by intellectual gymnastics, both being combined with hygienic regimen. The result is that an idiot inferior in capacity and intelligence to domestic animals, becomes if not restored to society, at least restored to his family, his bad habits are corrected, he is more obedient, more active, in better health, and affectionate to those who have given him their affection and support. whilst others have been enabled to read, write, speak, and occupy themselves readily in many manual occupations. If rude is the result (asks M. Séguin) of a system followed up for ten years only and by one man, with limited means, may not much be hoped for when it is perfected by the experience of many ?

Education consists not in improving the memory alone, but in developing all the functions of the body and the faculties of the mind. The education of the idiot should comprehend,—1. The active powers. 2. The intellect. 3. The will, that is the voluntary principle under the guidance of the enlightened conscience. (La spontanéité moralisée.) This is in order of their development. The infant moves and feels before he knows, and he knows before he has the consciousness of the morality of his acts and thoughts. The education of the active powers comprehends the training of the organs of motion and of sensation.

Education of the muscular system. The simplest apparatus is alone necessary. A table, a balance, a ladder, and dumb-bells, (or more simply, the two latter,) are alone required. Exercises demanding sudden and strong exertion are not required, but rather the constant exertion of force during a fixed time. There are no idiots who are well balanced,—all have either a preponderance of nervous excitability, or of muscular force, or are in complete atony. The education of the nervous system must therefore go hand in hand with that of the muscular, and be regulated according to the idiosyncrasy of the patient. Idiots are deficient in muscular sense. Prehension precedes standing in all children. When the idiot cannot use his hands he should be put on and in front of the ladder, held by his belt, and his hands and feet directed to mount and descend. If his hands refuse to hold the rounds, let him fall into the attendant's arms, and be again replaced. If this is not sufficient, M. Séguin makes the child mount the ladder behind, whilst he mounts in front holding its hands ; every step being made slowly and surely. In descending he disengages one foot which clasps the step, with his own foot with which he guides the child's foot to the next step. He disengages one hand which by an instinct of

preservation, rapidly clasps the step below; by these means the inert muscles contract energetically, and support a weight and shocks which no voluntary exercise can accomplish. The same exercise may be repeated on the reversed ladder. As soon as the muscles of the hands have been taught by this instinct of preservation to contract, they must be applied to taking food, to useful purposes, such as handling stones, bricks, spades, wheelbarrows. Dumb-bells can then be used, and the balance (which is a bar of wood with wooden knobs at each end) in order to make the child stand still, and walk steadily. When the abdominal muscles are so weak as to prevent standing, the child may sit with his feet on a spring board, to be moved up and down. Some cannot even sit, and must be taught at first to sit still without any support. Before any regular action of the limbs can be obtained, the child must learn to stand still. To do this he may at first amuse his hands with dumb-bells, and fix his feet either in boxes, or (which he often prefers) in a round or square space drawn with chalk on the floor. By whatever means necessary, this power of standing still must be obtained, as it is an essential step in rendering the motions obedient to the mind. When the powers of motion are much impaired neither the capability of standing, or of ascending or descending can be acquired without great practice, varied according to the individual. All should be habituated to those uses of the fingers necessary to daily life, to dress, to button their clothes, to tie, to fold, to carry, to arrange, to wash, and comb, &c., and subsequently to be able to cut, to saw, to plane, to draw, to sow, plant, water, &c. The aim is not to make muscular prodigies, but to employ the muscles usefully, and for this purpose a modest system of gymnastics, and a master patient in little things, are wanted. It must be remembered that there are three kinds of defects of motion which may be met with,—one in which the muscular power is deficient; the second in which the power of the will is wanting; the third, defects of structure, as contraction or retraction. For the first, purely muscular exertions with or without the will; for the second, voluntary efforts duly regulated; and for the third, an orthopedic treatment adapted to the maimed limb.

Imitation is a powerful means of instruction. It is *personal* when its object is to modify the individual's own acts and habits, *impersonal* when it relates to his action on outward things.

Personal imitation. Those who have the faculty and in whom it is not directed contract grimaces, and tricks of expression. Such must be got rid of. The first step being to enforce muscular repose. This is done by the double operation of imitation and authority.

"A. H. was (says M. Séguin) indomitably petulant; clambering like a cat, escaping like a monkey; he could not be kept standing still for three seconds. I put him in a chair sitting opposite him, holding his feet and knees between mine; fixing his hands on his knees with one of my hands and with the other bringing incessantly before me his moveable face. We remained thus *for five weeks*, meal and bed-times excepted, after which he began to stand, and almost immoveably."

When immobility is obtained the exercises of imitation may be begun. The first lessons are to teach the idiot by gestures and word the parts of his own body, and their common uses. Then the difference between right and left. The next exercises are to make him move his limbs at the will

of the teacher and by simple imitation, singly and then together; to close his fists, to open his fingers; to bend the index finger whilst the others are stretched out, &c. This personal imitation is more quickly taught in classes.

Impersonal imitation. There are three kinds of exercises.

1. The idiot is to repeat exactly the movements he sees his teacher execute who places objects in use in very different positions. The teacher takes a plate and places it on the table, indicating to, or telling the child to do the same with another. He then returns the plate, and the child imitates this: next places it vertically, &c., and puts it in numerous positions; and the same with a glass, a brush, a hat, and other objects in common use.

2. Place objects of no use in certain forms, such as wooden bricks as squares, triangles, &c., to be imitated by the child.

3. Make a vertical line on a board with chalk, which the child must imitate first by moving his arm in the direction and then marking. Next, a similar line from below upwards: then from right to left and from left to right. All idiots prefer their left hand.

EDUCATION OF THE NERVOUS SYSTEM AND OF THE SENSES.

The senses are to be educated in the following order. Touch, sight, hearing, taste and smell, that is in the order in which they are awakened in the healthy child.

Education of the touch. The most injurious effects of the disordered direction of this sense is in that bad habit to which idiots are much exposed, often by discovering themselves the gratification derived from it, and not by imitating others. Some idiots have no consciousness of the sensation produced by touch, whilst others have neither the consciousness nor the sensation. In the former it is only necessary to awake the consciousness, whilst in the latter the sensation itself must be provoked before attention of the mind can be given. In the first case it is important to associate hearing and sight with touch; in the last this is useless, but the touch must be roused by energetic and varied shocks, by agents whose actions are opposite, such as heat and cold.

To educate the touch of idiots, it is often sufficient to give him things to handle and to distinguish without using his other senses. The exercises are: 1, With hot and iced liquids; 2, astringent, emollient, oily fluids; 3, resisting or elastic bodies; 4, bodies which are rough, smooth, woolly, cottony, or silky; 5, heavy and light bodies; 6, bodies of the same form but of different size; 7, of varied form. They should be taught to distinguish slight differences in heat.

The education of the taste and smell. The children often abandon their senses to all the vagaries of their curiosity: nothing is too strong, or acrid, or fetid, or astringent, for their uncultivated tastes and smells. Although these senses have no direct connexion with the intellect, there can be no doubt that the harmony and due development of all functions is important. As a rule, stimulants should be avoided; but if necessary cayenne pepper, mint lozenges, or very small doses of colocynth may be used to stimulate the languid sense, and when it has produced its effect

it may be withdrawn. From these strong tastes the scale may be gradually descended to delicate ones. It is sometimes necessary to rouse the smell by long and frequent applications of ammonia or otto of roses, but in general the stronger smells are perceived.

The education of the ear. Very few are deaf, many however never listen, but in general the idiot loves and readily seizes rhythms, for the musical faculty is one of the gifts of idiots. M. Séguin has never seen idiots (except those absolutely paralysed) that did not express the liveliest pleasure at music, and he has known a great number who sang correctly though they spoke badly and with difficulty. One chanted all the vespers though he could not pronounce two words in succession. Another came out of a deep torpor during music. Another, generally torpid, experienced great pleasure in a concert; he smiled, his face became animated, his hair stood up, his fingers contracted themselves and were agitated, and his forehead and hands covered with sweat; after the last note he sank into his usual inanity. Another repeated difficult tunes after once hearing them, whose only articulate sound was papa, which he said with difficulty, imperfectly, and inappropriately.

Idiots are more alive to energetic, rapid and gay tunes, than to slow and grave ones; also to instrumental music than to vocal. The remedial influence of music has not yet been sufficiently studied by M. Séguin, to enable him to lay down rules for its application.

There are three kinds of exercises for the ear:

1. On sounds in general. The child must be taught to hear the sound of bodies falling, or of the contact of one sonorous body with another.

2. The gamut. He must be taught the differences in musical notes.

3. The voice as indicating the feelings. He should be made familiar with the expressions of joy, fear, and pain; in these exercises it may be necessary that the ear alone should be addressed, the action of the other senses being for the time suspended.

Education of speech. Some idiots are dumb from paralysis, perforated palate and deafness; but in general their muteness does not depend on want of voice, for the voice is strong; nor on the inability to articulate, as they can pronounce distinctly some words; but it consists in the few syllables or words they can pronounce not being spoken apropos, nor voluntarily, nor intentionally, and these accidental sounds having no sense. The idiot is said to be unable to speak, because he is unable to express his wants or sensations in words. This inability depends principally on two causes,—an incapacity to move at will and with ease his organs of speech, and that want of will, that repugnance to perform any spontaneous act which is the characteristic of idiotcy. The organs of voice should be carefully examined, as their structure is often anomalous,—such as defect of action of the muscles of the cheeks and lips, contraction or great width of jaws, atrophy, hypertrophy, or immobility of the tongue; great elevation or depression of the arch of the palate.

First exercise. When the child has been taught to imitate, his attention should be directed to his mouth. The index finger should be put across his lips, then horizontally, then between them, and then two or three fingers introduced into his mouth. Let him hold a ruler between his lips, if they do not contract increase its weight; if they are too far apart gradually diminish its size. Make him masticate hard food, next make him

simply imitate the motions of the lips in speaking. If there is a want of proportion between the tongue and the palate, as the latter cannot be improved, the tongue must be rendered more agile by a system of tongue gymnastics. He should be taught to move his tongue out of his mouth, up and down and sideways, keeping it some time in each position. If this cannot be done by imitation or direction, the tongue should be directed with a wooden knife or ivory spoon.

Second exercise. The next step is the development of the voice. The principle M. Séguin follows, is to observe the natural course of this development and to pursue the same. Thus children begin with sounds like pa, ma, dada, bo—bo—mi—mi; not with a vowel but with *a labial consonant followed by a vowel;* and they love to repeat the syllable. Such sounds, (as they are much easier, than vowels alone, or than consonants preceded by a vowel,) should form the first lessons. Even this plan must be modified according to circumstances. Some cannot make a long and pure vocal sound, and must be taught by imitation to do this before they can articulate; and in some whose lips do not contract it may be necessary to begin with linguals before labials, as the difficulty of the latter will discourage them. Two cases are given which show the method more clearly than mere rules.

A. T. a hydrocephalic child, with a high, deeply-grooved palate, talked constantly, but could not articulate. The letters he could pronounce were N, as non, which he repeated many thousand times a day, M, as ma, and P simply. He could remember and sing tunes. As he could pronounce labials, he was first made to practise them, to distinguish PA from PO, and MA from MU. From P and M he was led to B, through BO and BU. V and F were next attained by placing his under lip beneath his upper incisor, teeth, and this led to the sibilants S, CH, Z. J. By elevating the tongue anteriorly LA was pronounced, and next the labials T and D, and thence to the gutturals. He completely acquired speech.

The second instance is of a child of 13, with a small circular head, whose faculties were seriously impaired but whose voice was good. He could imitate any syllable of two letters only, but could not unite two unless they were the same repeated. Thus he could say mama, but not maman. M. Séguin first made him repeat A, following it with a syllable of two letters, as A-TA; next, A, O, which after some time he accomplished, and seizing a moment of excitement his teacher added T, so as to make it A, TO, a double articulation previously impossible, then A-DO, A-LO, &c., then the labials; next two syllables, keeping the first syllable and second vowel constant, and only changing the second vowel, as TA-LA, TA-BA, TA-RA. Next changing the second syllable wholly, as TA-BO, TA-VO; then changing the first only—then changing the vowels only. For syllables of three letters, which were next attempted, the two first were kept as constants, the last changed. The task was long and painful, but the child spoke distinctly at last.

As this part of education is the slowest and most difficult, it should not be begun until some power of imitation has been attained; and all the exercises must be practised a long time; analogous syllables must be first united, then different ones, then whole words, until those of the most difficult pronunciation are articulated. Stammering often hinders progress. Its remedy must depend on the circumstances of the case, and

on this adaptation to the individual case M. Séguin attributes the benefits which has followed his means.

The education of the sight requires great and methodical attention, from the difficulty of acting on the eye, which is an active organ so situated as not to admit of being mechanically directed. The first point is to fix the vacant wandering eye. Three exercises may be practised: 1, Shut the child in a dark room in which is a luminous point that can be moved so that the eye may follow it; 2, Throw the balance from one to another, so as to attract the eye to follow it; 3, Place the child before you, and follow his wandering eye with a firm and obstinate look which will provoke attention: the intelligent and animated eye follows the inactive eye, arrests it, fixes it, and then directs it. But this most important exercise is often very difficult to execute. The child avoids your look, and perhaps closes its eyes. M. Séguin attempted in vain for four months to attract the look of one child. The first time the child's eye met his own, it uttered a cry; the next day, instead of mechanically feeling him, it looked at him as something new; and day after day renewed this scrutiny with intelligence until its curiosity was satisfied and it saw without astonishment. By speaking to the child at the moment its sight was roused, it looked at him afterwards when spoken to, and thus its hearing improved. M. Séguin has found that the progress of the idiot is not so appreciable during the education of one faculty, as during the transition from one exercise to another.

Direction of the eye being obtained, even although imperfectly, it must be taught to distinguish colours and form. Square octagonal cards of different colours should be used, and square cards, of two colours (say orange and blue), being placed on the table, two octagonal cards of the same colours should be given to the child, who should be directed and commanded to place each on the square card of the same colour, and the colours should then be named. To give a notion of length and size, many pieces of wood of different lengths and sizes should be procured, and when placed together, the child should be made to point out the longest and shortest, and the biggest and least, beginning with those whose differences were very apparent. For forms, it is at first necessary to have holes corresponding in size to the circle, square, or triangular model, into which he may fit it; afterwards he must be taught to place one moveable shape on others merely painted or drawn on paper.

Arrangement may be taught by pieces of board or wooden bricks. Give one to the pupil, and taking another yourself place it in various positions, making him place his in the same; proceeding to two, three, four. Build bridges, houses, &c., with wooden bricks. To understand *plans* is an important step. The simplest forms, as the circle and square, should be begun with, and the child taught to carry its finger round their edges, and to put it, following the teacher's, on each of the angles. Next, the teacher, placing his finger on one square of a chessboard, must direct the pupil to place his on the corresponding square. The same may be done with figures of houses, animals, soldiers. Thus the child is taught to direct his hand to certain given points.

Images are useful, but they should be at first representations of simple and common objects; lines, figures, houses, carriages, animals, and

finally groups. As soon as an idiot takes pleasure in these representations, they should be multiplied; his taste cultivated by seeing good pictures and statues in museums, and he should be led to give an account of what he has seen afterwards. Those of M. Séguin's pupils who have made most progress are those whose intellects have been most trained by this examination of works of art.

Drawing. In order to draw, he must previously have been taught to distinguish the topography of a place, to know what is above, what is below, the left from the right, and to be able to place his hand or finger on any given point, and to direct it; for he is unable to do any of these things. To draw a line in a given direction is the first step in writing as well as drawing. Children acquire these notions generally by intuition; but every step must be carefully taught to an idiot. Having mastered these principles, the first step in drawing is to draw a vertical line between two points; but as the hands of some descend to the right, or left, or wander about the paper, it is often necessary to draw as guides two parallel lines, one to the left and the other to the right of the points, or besides this to fix two vertical rules so as to prevent the hand from deviating. Gradually these helps are to be withdrawn until the child can draw the line even without the points. The same method must be followed for horizontal lines. Curves are to be first drawn on these lines. Having made a vertical line, draw a curve, beginning at its summit and ending at its base; the child sitting at your left, and the curve drawn from left to right, by which means the hand does not hide the course of the line. A similar curve is to be drawn on a horizontal line, joining the lower end of the vertical. Four such curves form a circle. In uniting lines to form figures, M. Séguin has found that the idiot experienced great difficulty in drawing a square, but made a triangle more easily. He therefore begins with a right-angled triangle; joining his vertical and horizontal by an oblique line. Four such triangles make a square, the inner lines may be effaced, and afterwards the square drawn simply. Curves on each side make a circle. From these simple steps the idiot learns to make the most complicated figures, but the process is a laborious one for both parties. M. Séguin incidentally mentions a fortnight being required to teach one child to make a straight line. When the motion of the hand is irregular and jerking, it may be necessary to make him draw thousands of circles from right to left and left to right. Sometimes by drawing a white circle on a black board he follows the mark; or one circle within another, and between which he draws his own circle; in others, two projecting circles are necessary to guide his hand.

From drawing these geometrical figures, the idiot passes to forming letters. Thus D is only a half-circle resting on a vertical line, A two oblique lines united at their summits, and divided by a horizontal line. Having learned to draw, he can write. The letters must then be traced according to contrasts and analogies—O next to I, B to P, T to L, &c.

Reading. The exercises in plan, colour, arrangement, size, and form, are necessary steps to understanding letters, and the notions of which they are the signs. The next proceeding is to learn the names of the letters. A frame is constructed, in which are placed each letter of the alphabet painted on a card, to each of which a metal letter exactly corre-

sponds. Two or three metal letters are placed before the child, and he is directed to place one at a time on the corresponding letter in the frame, the teacher naming them. Subsequently, a letter being shown to him, he is told to name it: if at first he were told to name it as well as place it, he would be puzzled. He thus connects a name with a figure, and a figure with a name. The order in which the letters should be taught to be pronounced is,—labials, labio-dentals, dentals, linguals, gutturals. Letters being understood, syllables are to be mastered, or the relation between the sound and many signs; and also the relation of many signs with many successive articulations. Here the previous exercises assist. The idiot who has placed two bricks to form one figure, is led to comprehend how two letters can form one sound.

Words. The next step is from mechanical to intelligent reading. To connect words with objects and ideas. For this purpose the card letters forming the word (bread, knife) should be placed on the object; then the object should be given to the child, who should find its name among several names placed before it. The child at first should never read a word without understanding it, and therefore it should be the name of an object of sense: thus all the persons and things about him should be named, and alternately written and pronounced.

Notions and ideas. M. Séguin has carefully distinguished between these two words as the basis of his plan of education. He explains the different sense which he intends to convey by an example. A child when he can distinguish a key from a table or a hammer, has the *notion* of a key; but he has the *idea* of a key, only when he knows the relation between the key and the lock. The *idea* is the result of the mind's operation on two notions, key and lock. It embraces their relations, the reason of these, and their consequences. By the *notion* of the key the child distinguishes the key from other keys and other things; by the *idea* he understands its use. By *notion* he means to convey merely the knowledge gained by the senses; by *idea* the result of reflection on these impressions. The first appreciates the physical condition of bodies through the senses only; in the second their relations through the intellect. *Notion* is passive, *idea* active. *Notion* is acquired by the senses. *Idea* by the reason, through deduction and induction. It follows that education must commence by notions which embrace all phenomena which are objects of the senses. Once acquired there will be materials for *thought*, which, however, cannot be taught.—You may give the child notions, but his ideas must spring "proprio motu."

Practical grammar. The mode in which substantives are to be learned has been stated: the next step is the expression of their properties by *adjectives*. The child should be placed before a table, and taught that it is brown, round, a square, on two or four legs, so as to establish the differences between it and other tables. At first the object itself should always be shown to the child, and the words expressing its properties written. Subsequently names with their qualities (as a white horse, a sharp knife,) should be written, and the child point out the object among many, and so on. Action is expressed by the *verb*, which should be shown by sensible images. At first read or write the verb expressing some of the instinctive actions, as eating, and interrupt the act of eating to impress it: next,

having written "to strike the table," the master should point out the verb, and imitate the motion, and vice-versâ. As to the tenses, their order must depend on the language, and the rules for the French will not wholly apply to English. He begins with the infinitive, then the imperative, the conditional, the present and the future. The preposition comes next. Having written "put the bottle *upon* the table," the child does it, then "*under* the table," he obeys, &c.

Memory. In the education of idiots no words should be taught which they do not understand. The first exercises should relate to sensible things: the idiot should be requested to bring some object from a distance: then two, then three, then more which he should choose from many others: next he should receive the same orders for things in another room, and next for a time at some little distance, as for five minutes, half an hour, a day. Afterwards he should be ordered to perform personal acts, to wash his hands, at a certain hour before his meals; to remember the time of rising or going to bed; to say at noon what he has done in the morning; and in the evening what he has done during the day. From notions he may pass to ideas; but he should never be taught to repeat what he does not understand.

In those who have no memory the following plan may be tried. After the idiot has seen his food, remove it and ask him what it was; after some trials this will succeed. Afterwards let him name the food he has eaten. From thence pass to what he has seen, heard, &c. The real use of the memory should always be remembered. It is to furnish the mind with materials from the past to enlighten the future. Its final use is for foresight, and it therefore leads to a taste for useful labour, and to that positive knowledge which is of practical utility.

Provision and foresight. The most trifling occurrences of the day afford lessons in these important acts of life. The child wakes and he has no shoes, for he has forgotten over-night to order them, which was his fault: he has not got what he likes for breakfast, as he had not ordered it before; this is his fault: he has no money in his walk to buy refreshment, as he forgot to bring it. It is his fault, always his fault. This part of his education is most important; for without foresight he is the victim of slight circumstances and of all his wants.

Arithmetic. Numbers must be taught by objects; any will answer. M. Séguin makes use also of balls like billiard balls, numbered, with holes for them having similar numbers. To learn the numbers is an affair of practice and memory; some little help may be given by rules, a few of which are given. *Calculation* requires thought. M. Séguin takes *two* as the basis. He makes the child take off his shoes. How many shoes have you? Two. How many pairs is that? One; and so on with 2, 3, 4, and 5 and 10 pairs, and the odd numbers varying the objects at each lesson, and applying the numbers to quantities, weights, dimensions, size, distances, capacities, and at last to value; teaching by things and not by abstractions, and thus educating by means of the senses. In some idiots there is a natural faculty for calculation, but as this is useless alone, it should not be cultivated at the expense of the other and more useful powers. The principal application of calculation necessary for idiots, is to understand the use of money. They must be first taught the difference between

the different coins, their form, colour, weight, substance and name; next their use. This can only be taught by practice. They must see others pay for goods at shops, and be taught to purchase themselves, by first buying some sweetmeats. M. Séguin gives the details of his management in shops with his pupils, for this, which is a simple amusement for other children, is a study for an idiot. He has among his pupils, idiots whom he has taught to count, keep and spend their money, to go to market, to the butcher, &c., and for whom their mother's care becomes every day less necessary. For such he has attained his full object in teaching them calculation.

Natural history. The idiot should be taught the names, differences in form, colour, and the use of these animals, and also to take care of them, to feed them regularly, and to clean them. This gives him foresight, address, and neatness. Horses, sheep, cows, and such animals are all lessons for him; and he should also be taken to see wild animals. Vegetables are means of instruction: he should learn to distinguish one flower from another, by their colours, form, and smell. He should be taught to sow, plant, water, and gather. To teach him *cosmography* by maps would be impossible at first, but his teacher, with a rule always in his pocket, should constantly instruct him in distances, and in sizes, the longer and shorter, the less and the greater. He next should compare those which are separated: as the length of a room with its breadth, or the size of one piece of furniture with another; next the relative length of the garden and the court, and their relative position to the house, and the house to the street. One village is the end of a short walk, another of a long walk; in what direction are they? From this actual topography of the town and neighbourhood, the teacher passes to maps of the county, making the town the centre, and teaching the pupil to trace his journeys to certain places and back again. Their astronomy consists in learning that the sun rises before such a window and sets before another, &c. The great art in his education consisting in limiting him to positive and not speculative information; to facts such as can be presented to his senses, and not abstractions which to him are mere senseless words.

Education applied. The object of this training is not only to develop the faculties of the idiot, but to apply them to the common actions of life.

1. *Decency.* 2. *Bodily habits.* All tricks and unseemly bodily movements should be carefully eradicated. How necessary such training is in ordinary children! 3. *Attitudes.* The idiot lies in bed like a ball, or throws off any clothing, turns his head from side to side for hours; all such habits should be overcome, sometimes with mildness, sometimes with energy. He rarely sits well, and this becomes a cause of deformity. He should be made always to sit on a stool without a back, or arms, and the height of his knees. His bed should be hard; he should not lie long in it, nor sit much. Standing still is the cure for many of his ungraceful motions when erect. 4. *Walking.* He walks with the gait of a drunkard; and requires careful drilling. This unsteadiness prevents him from going up and down stairs with any ease. He is obliged to bring both feet on the same step before he moves one, and he always moves the same foot first, and with fear and trembling. Much practice on a light staircase with low

steps is necessary. The gymnastic belt furnished with two rings, one in front and the other behind assist the teacher. 5. *Dressing.* The idiot should first know every part of himself, and be taught next to name every article of his dress, and to show to what part of himself it belongs; next the order in which they are to be put on, and finally the manner of putting them on. These explanations require long efforts, and the complexity of the common operations of lacing stays, putting on braces, is seen by the difficulty. The teacher requires great skill as well as patience. 6. *Eating.* He should be taught to eat alone, and neatly, masticating well. When he has given signs of appetite, the attendant should eat before him slowly and deliberately, performing each step in the process, chewing vigorously, and placing his mouth so near as to show the food between the teeth. His attention being thus attracted the attendant should eat, sitting opposite to him at a narrow table, putting a spoon, or fork, or knife in his hand; if he is slow, be slow with him, making him take every spoonful, and mouthful, and draught in imitation. When his appetite is satisfied, play with him and induce him to speak and act freely, when he will often declare his tastes and habits. Hard and stale bread which he must masticate to swallow, is an exercise tending to strengthen his organs of speech. M. Séguin doubts whether he has sufficiently insisted on the importance of proper habits, as bad habits in idiots are so numerous from their idleness, and their infirmities. Patience and resources are necessary in the teacher, who should always seek the simplest and most natural remedies. Almost all manual exercises are useful. 8. *Useful tastes.* Constancy in quiet, agreeable, and useful tastes renders the idiot bearable to others. Such habits are the fruit of education alone. The kind of tastes must depend on the station of the child. The manners and gestures should be taught to approach as nearly as possible to those among whom the idiot lives, so as not to disgust; and to render their tastes useful, they should be taught to work. 9. *Useful work.* This is most essential to the poor, as the neglect and brutality with which they are treated is often from their expense and uselessness. But besides this, work is essential to health and morality. It is the fulfilment of duty. "Wherever I have gone (says M. Séguin) my object has been to organize work. In public institutions, field and labouring work, and joinery; at home, needlework and household work for girls; and trades for boys. Simple and useful work on which their health and morality depend."

FIFTH PART.—MORAL TREATMENT.

M. Séguin confines his observations to morality as embracing the relations of a human being to others and to himself, and not including those with his Maker, the higher department of the clergy. In many cases the teacher may assist, and M. Séguin shows his own religious feelings by giving two short prayers which he makes the idiots of the Bicêtre repeat before their meals and their exercises, praying for God's blessing on them. Morality then, as including the regulation of the actions, intellect, and passions, is essential to any education. The child's will must be subjugated to the authority of another's will as the first step.

Authority and obedience. Idiots are less deficient in the power of commanding others than in obeying them. Obedience irritates them, authority

fatigues them. Some employ the best devised artifices to bend another's will to theirs; others use ten times more skill in escaping an order, than it would have required intelligence to have obeyed it. Another would obey a man passively, though it would enslave a woman, highly intelligent but without authority.

"Miss ——, (says M. S.), at eight years old was intrusted to me; she had subjected all her family to her orders. Though an idiot and mute she saw at once that her relations were changed, and she obeyed. A year afterwards her mother visited her, and she began tormenting her, and disobeying me in her presence; I made the mother retire so as to see unobserved, the implicit obedience of the child to myself. Here the cure was not complete. Subsequently she became obedient to all her family."

An idiot, as well as children generally, perceives sooner than an adult the degree of authority to which they must yield, or which they can overcome; they do not struggle in vain, but know even by a gesture the will that can overcome them. In obedience to a well-regulated command their physical health depends; the child badly governed, sometimes with feebleness, at other times with force, or with indulgence, does not fear such versatile authority. He infringes, or eludes orders: becomes cunning, irritable and violent, and these mental irregularities bring indigestion, interrupted sleep, and nervous disorders.

That the idiot should obey, his teacher must know how to command, and be able to apply all means of authority from the most rigorous to the most insinuating. Obedience is not a simple affair; a child who for fifteen years has only learned to obey his masters who are with him, has often only learned to be disobedient the rest of his life. There are degrees to which he must be gradually submitted. 1. Obedience to his master when present. 2. Obedience to orders which may be immediately enforced by the presence of his master. 3. Obedience to an order, in the absence of his teacher which leads to obedience to a moral principle assented to by the reason. Or, in genuine English, obedience to a sense of duty, and we thank God heartily that we have in England in daily and in hourly use a term which does not occur in this whole volume, but which foreigners are forced to admit is the strong characteristic of the British character—*a sense of duty*.

At first *simple* obedience must be enforced. The idiot must be made to look, to touch, to act, to perceive, to compare at the will of his teacher. He must be at first passive: his progress, and the use of his own free will is the fruit at last. For the end of education is not passive obedience but liberty, and to be free there must be a will. *Liberty* and *will* are words only to be spoken by those who comprehend *obedience* and *authority*. No one can command but he who has learned to obey. The *will* regulated by the intellect and the moral sense is the secret of success in men of character. *This* will is the result of education. The idiot is destitute of it. *His* will is governed by his instincts, and by his inertness. Indulge his appetites and let him alone and he is content. But moral education consists in conquering these appetites, by breaking up his habits, and in subduing his indolence by varied and incessant activity. The instinctive and negative direction of the will having been subdued, the will must be morally trained by imitation, by authority, and by compres-

sion; it grows morally by the *resistance* given by a good education to the first instincts.

The master. Every one cannot teach idiots; the teacher must have a will, he must have the faculty of command; this is by no means in proportion to the intellectual power; the intellect and will are distinct faculties, and this is an important principle in M. Séguin's method. Often the labours of the study seem to deprive a man of the personal power of influencing others. A teacher should have no physical defects, but above all moral qualities he should be *calm*—whether exercising firmness, energetic decision, gentleness or insinuation, under all his aspects and in all moods, *calm*. Calm in command, calm at table, calm at play, of an unchangeable serenity of mind; if he has never had an occasion to exercise that self-control which produces this calm, no better opportunity of self-conquest than the education of an idiot can be afforded, for no other occupation demands more patience, more observation, more concentrated action, more calmness; but a man capable of dedicating his youth to the education of an idiot is found with difficulty, and one who gives his life to the work must daily call to mind the sacred words, "the good shepherd gives his life for his sheep."

But how is the idiot to be ruled? If he is capable of foresight he has been apprized of the new hands into which he is to be placed, and looks forward with some anxiety, so that the first interview is well calculated to impress him; but for this purpose the master should act as a master, not as an ordinary visitor; his address frank, his language and gesture plain, and his manner so decided that at first he is remarked, listened to, regarded, and recognised. If the first impression succeeds and has not been weakened by the comments of relatives and servants, the master has gained and will keep the magisterial position necessary to his success; but he must not think that time will increase his authority; he who looks to time has had little experience either of men or of idiots; if he is really the master he may in time relax his authority as the pupil's own will grows and is more regulated; but if he has not gained the ascendancy the child does not lose his time, but hour by hour, day by day, will steal from his master some concession, surprise him in some weakness, will profit by the lassitude of one day, and the indisposition of another, to command him in his manner; that is negatively, in doing or not doing, whatever suits his disordered instincts. The master therefore must at once take up a decided position, raising his authority above that of the family, which has been weakened by daily concessions to a delicate, suffering, or ungovernable child; the weakness of parents must be pitied, perhaps, rather than blamed, but the master must not for a moment hesitate as to how he is to act. If he cannot be master, he can do nothing.

The pupil. This necessity of commanding the idiot depends on his want of a moral will. Accustomed to do nothing, he will do nothing; used to manage his parents, he will attempt to manage his master, by force or address, sometimes by smiling and looking at him with that scrutinizing eye which sees into dispositions; sometimes by anger, or by crying, biting, rolling on the ground, or escaping from any constraint. From the energetic—no! no! incessantly repeated, to the most subtle pretences of fatigue or suffering, the master has often to be exposed to all

the various batteries of negation; he must expose to the family the nature of this resistance, for its energy proves the necessity of control, and that a will is present but not exerted in the right direction. The struggle between the two wills, the master's and the pupil's, may be long or short, ending by the conquest of one; the pupil will conquer if the master has not the faculty of command, and especially calmness: or if the parents take the child's part against the master.

Immediate command, or that of the master when present. Resistance varies, and rules must be modified: those given, suppose the maximum. To make an idiot will, it is necessary first to will for him, to will that he should will; the command given in the most imperative manner, with the words supported by gesture and look. If the master's voice is dull and insonorous, if his articulation is vicious, his words dissonant, nasal, long and monotonous, he labours under great disadvantages; he should unite the two qualities of a flexible voice and a perfect articulation; his commands should be in short, sonorous, and decisive sentences; in the explanatory part the expressions may be slow, detached, every syllable expressing its meaning aided by gesture and look. All the notes of intonation and articulation should be contributed, from the short, dry command, to the most sympathetic and fascinating intonations.

Gesture assists. It is a constant excitement to imitation and activity. To attain a facility the master should practise privately with another to express by signs only what he wishes; the master should also do himself what he wishes the idiot to do. The general rules are, that gestures to enforce stillness and attention are made by moving the hand from above downwards, and from without inwards; those from below upwards, and from within outwards, are used to excite activity. Again, the movements of the hand and arm should be in a straight and rapid line in imperious commands, and in a curved line produced by a slow and undulating movement in explanatory commands.

Expression. The eye in fixing attention becomes a powerful instrument; the idiot should feel that it is always on him, and that it expresses better than words whether his master is satisfied or not; it should command him at first, and afterwards guide him, animate his activities and sustain his exertions; for this purpose the master should not only have a good eye, but should have studiously practised its use. As his gestures and words should be more decisive than ordinary, so also should his expressions; he is not to stare and alarm by making faces, but simply to express by his eye and features what he himself feels at the sight of a child when obedient or disobedient, attentive or inattentive, patient or angry, and to make their expressions so marked that the child cannot mistake his meaning. For this purpose,

1. The physiognomy should be naturally moveable, expressive, and inclined to reflect the feelings. 2. It should always be in harmony with the mind. 3. The expression of satisfaction should neither be long, lively, nor noisy, so as to interrupt employment, especially if the idiot is vain. 4. The expression should be confined to the thing in hand, and never be interrupted by what is foreign to the immediate purpose.

Coercion. No other means but words, gestures and looks, should be employed. All children are born with a spirit of resistance, children are

all of the opposition; no, precedes yes by many months. This instinct of negation is most marked in idiots, and when joined to their extreme laziness, and confirmed into a habit, by finding it overcomes the reasonable wishes of the family, it must not be expected that the master can at once overcome it. No! he will be opposed in every way; but let him will with patience and perseverance, and the child will obey him. If, however, he has a pupil who has this habit of negation, and has ruled his parents, the master should never begin with an order which can be refused him. He must begin by negative orders, chiefly as—not to go there, not to touch that, or not to eat this, and by positive orders of which he can compel the execution, and afterwards proceed to those which demand a concurrence of the child's will; the transition requires an adaptation to circumstances which makes it a scientific art in the hands of a man gifted with that serenity of mind and charity of heart necessary to success.

In cases of extreme resistance the master must put himself in immediate proximity with the pupil. For instance, in commanding him to raise his dumb-bells he only raises that arm next to his master; he must therefore place himself before him beginning and continuing the action for him, and thus acting immediately by word, look and act on every part under command; this is hard and painful for both, especially for the master, but it is not necessary that both should be incessantly together: a milder authority, a half-will in the intermediate times may carry out the plans, a woman attendant, who knows how to resist the perverse will of the idiot without completely understanding how to enforce her own, is sufficient. The imperative command is necessary for each new step, but the child would repeat better the new lesson under a less rigid disciplinarian; the master passes from this imperative mood to a milder form or *mediate* command.

From subordinating the idiot's will to his own, the master next induces or excites him to act for himself. The favorable moment of transition must be watched and seized, it cannot be hastened; it is infallibly indicated by two signs usually simultaneous: the absence of all resistance to authority, and some spontaneous wish for an active and intelligent occupation. An instance is given of an idiot who would do nothing but keep in incessant motion; the usual steps of compelling him to be still and to use certain exercises were gone through; one day he seized a crossbow which had been used to direct his eye, and he shot the arrow through the eye of one of his ancestors in his sculptured frame. His governess was in despair. M. Séguin was enchanted; he procured painted lithographs of heads, stuck them along the garden wall, placed the crossbow at hand; the idiot seized it, discharged it at one of the figures, was encouraged, did it again, and subsequently the promise to repeat the play kept up his attention and patience in his exercises.

After the first spontaneous act of an intelligent kind the immediate imperative authority should be gradually discontinued, and the idiot should be induced and excited to obey, through the means of his tastes, preferences, ideas and feelings, which the master understands; gradually spiritualizing him; commanding him more by words than gestures—more by the sense of words than by the sound of the voice—more and more by hopes, reasons, and feelings. This is a very gradual process; it begins

by commanding the idiot to do a thing which he has done well, by an imperative order in a less imperative tone; diminishing little by little the rigour of the voice, the precision and decision of articulation, gesture and expression, until it is relaxed to the ordinary usages of society; but still the pupil must always feel that the old master can reappear—he must fear the claws beneath the velvet paw.

Stimulants of the tastes and desires are numerous. The idiot loves his parents and desires to see them; he likes the country and desires to walk; he likes flowers, cakes, pictures: all these may be promised as rewards. His antipathies may be used also as preventives of disobedience and idleness; but to be efficacious these promises must be facts—something positive and actual. He should see, or touch the object desired or feared; gradually the conditions should be adjourned, and thus his desires spiritualized by putting their satisfaction at a distance.

Games hold a high place in this education. M. Séguin mixes with his pupils as their playfellow, though he imperceptibly directs their games. A game is the most spontaneous act of infancy: it is more, it is the free and voluntary accomplishment of a bodily and mental function; an idiot who can play almost deserves another name. The choice of games lies with children, their variation and graduation with the master, who should take care that the game does not become a mere routine, but that there should be always something to learn; at first the games which please his tastes, afterwards those most useful. Thus when there is inability to direct the eye, the bow and arrow; for difficulty in motions of the hand or involuntary contractions, the battledore; for unsteady gait, the wheelbarrow, &c.

Negative command, or that which is not expressed in words but depends on such arrangements as shall lead the idiot to think and act for himself; the master has only to watch quietly and patiently after making his plans; the master has at first forced him to act, next has assisted him, now he places him in such circumstances as to force him to exercise his attention, comparison, judgment, reflection and moral will. He has arrived at that point in his education when he can execute all voluntary movements; he can read and write, whatever his master wills energetically he can do; but still he is an idiot, ἰδιος, 'solitarius,' alone! He cannot act on persons or things by his own free will; he cannot spontaneously will; before acting on persons he must begin on things, and as the prehension of food is one of the first voluntary acts, the master may begin to exercise the will with this. The cloth is laid; you sit down, so does he, but the dish is wanting; you take no notice; he calls the servant, "Where are the chops?" You wait, he goes to find the dish. At first it is placed on the sideboard, where he can also smell it; another day in the passage which he has passed through; finally, in the kitchen; the same with the bread, wine, &c. Next, the dish is there, but no plate, no fork, no spoon, and he is compelled in the same quiet way to exert his own thought and seek them. Thus (if his health permits) a meal may last for hours, but the time is not mis-spent if he learns to think. At a later period he must have nothing for dinner which he has not ordered in the morning, or purchased himself beforehand. Dress is another stimulus to thought,

especially as regards cleanliness; he must not walk unless he has ordered the washerwoman to send home his clean linen.

To establish his relations with persons he must have some want or wish that they can satisfy. When therefore he goes to find an object, a third person must detain it and exchange it for some other object which the child can comprehend, and which is not injurious. The simplest relations with others depend on the sentiments of property and of resistance; the inert, inoffensive idiot, incapable of defending himself, should be encouraged by innocent struggles which develop his strength and excite his confidence and courage. With regard to property, they should be taught that some things are common to all, as their water; some personal, as their clothes; some are both, as playthings; but as all have their equivalent in money they should be taught its use, among themselves; this will reveal indifference, prodigality, theft, and avarice, which should be corrected in time. The essential point is, that the idiot should cease to be isolated, that by his wants, tastes, habits, attractions, repulsions, he should establish voluntarily the most numerous relations possible with others.

The pupil has acted, perceived, and thought for some time, but he willed nothing spontaneously: he was motionless from a languor of the soul. Now his spontaneous will has been restored to him, and he has gained the energy that distinguishes man.

In conclusion, M. Séguin says that he does not regard his book as complete, but only as a commencement: two things are wanting to make the moral treatment not of idiots only, but of the insane as scientific and humane as it should be. The first is, the concurrence of men of *heart;* the second is, the solution of the great problem of education: this he thinks is at hand; for that society will no longer be content with mere education of the memory, or of the intellect, but will demand an education of the whole being, of all the functions of his body and the faculties of his mind, of his aptitudes, and of his artistical and moral sense; for an education of the functions such as Rabelais, Montaigne, and Rousseau indicated, does not yet exist.

On making the preliminary survey of the volume which we now close (consisting of 730 pages), there was no doubt as to the method of treating it. To be of any service to our readers it must be analysed. We have not criticised, from feeling that to criticise a plan of education which has produced such results, would be impertinence. We have therefore preferred the humbler task of clearly making out the author's meaning and giving it an English dress, and on this we have bestowed no little pains, and no little compression. Our analysis is the spirit of the book and not a literal translation.

The principle which guided M. Séguin, appears to us most sound and philosophical. His master was the celebrated Itard, by whom the idea of educating an idiot methodically was first put in practice, and to whom M. Séguin gives the credit for the first germ of his discovery. The well-known savage of Aveyron was the man whom Itard endeavoured to educate. It will be remembered that this was he whom the "philosophers" of Paris, during the revolution, regarded as a true type of an unsophisticated natural man. Disbelieving the Mosaic account they imagined man was

created a savage, and rose by civilization to such a philosophical height as Paris then furnished. This savage from the woods was just what they wanted to prove their theory. But Itard failed in educating him. He was an idiot. And why did he fail? From the influence (says M. Séguin) of the false metaphysical system then in vogue. He and the then fashionable school imagined that the mind consisted alone of impressions on the senses. They did not recognize the existence of the mind itself. Itard therefore thought that education consisted only in the education of the senses, and he failed in making the savage a man. Séguin is a disciple of a higher school of thought. He has lived in a better period, when French materialism has been modified by German and British spiritualism. The former school asserted that there was nothing in the intellect which was not previously in the senses; the deeper thinker admits this as a half-truth only, but adds, that besides this, there is the intellect itself. The difference between the two principles is practically immense. Educate the senses and you educate the mind, say the one. M. Séguin and his school say, you do no such thing; you must, it is true, educate the senses, but if you stop there you have done nothing, you must rouse the mind itself to act; as long as it is the mere passive organ for the reception and remembrance of impressions, its *human* power has not been called out: the individual is not educated. But how does it act? By means of the will under the influence of the reason and moral sense. This moral will is the distinctive attribute of man: it is this which the idiot has not the power of exerting, and this deficiency makes him an idiot. To educate not merely his senses, but to rouse his free will to activity is the end of all education. Guided by this principle, M. Séguin has framed a system of education, and has successfully put it into practice. He is modest in stating the results of his plans, but we quote the following from Dr. Conolly's admirable letters on the Lunatic Asylums of Paris, published two years since in this Journal, describing his visit to the Bicêtre.

"No fewer than forty of these patients were assembled in a moderate sized schoolroom, receiving various lessons, and performing various evolutions under the direction of a very able schoolmaster, M. Séguin endowed with that enthusiasm respecting his occupation before which difficulties vanish. In all these cases the crowning glory of the attempt is, that whilst the senses, the muscular powers, and the intellect have received some cultivation, the habits have been improved, the propensities regulated, and some play has been given to his affections; so that a wild, ungovernable animal, calculated to excite fear, aversion, or disgust, has been transformed into the likeness and manners of a man. It is difficult to avoid falling into the language of enthusiasm on beholding such an apparent miracle." (British and Foreign Medical Review. Vol. xix, p. 295.)

We take leave of M. Séguin with respect and admiration. His book increases in real interest as it proceeds, and he gets into that part of his subject where he is indeed the master, and he transfers to the reader some of that warmth which he himself feels. We have a great respect for enthusiasm and enthusiasts. Nothing great, nothing heroic, nothing largely unselfish, nothing which proves that man is indeed of a divine nature, has ever been done without it. Our own Harvey and Jenner were, equally with Howard and Clarkson, driven by that inward impulse towards their mighty objects, which their duller contemporaries regarded as akin

to madness. Many enthusiasts have, it is too true, been led away by a false and delusive light, but where, as in this instance, the path is one requiring unwearied, laborious, and self-sacrificing exertion, and the result not brilliant but silently humane, there can be no doubt that the guiding beacon is not the phosphorescent exhalation of a swamp, but a light from Heaven. We have aided in handing on the torch, and may it be the means of kindling a similar flame in the heart of even *one* earnest man, and of leading him to the amelioration of the sad condition of the numerous idiots in this country?

ART. II.

Beiträge zur Ohrenheilkunde von Dr. WILHELM KRAMER, Sanitäts-Rath. Nebst 19 statistischen Tabellen.—*Berlin*, 1845.

Contributions to Ear-Medicine. By Dr. WILLIAM KRAMER, &c. With 19 Statistic Tables.—*Berlin*, 1845. 8vo, pp. 314.

THE principal of these contributions, viz., the statistics of the diseases of the ear, the author tells us, are the result of fifteen years' careful observation, and are entirely practical; the utmost possible accuracy of diagnosis and simplification of treatment, the only true aims of the practitioner, having been alone kept in view. In these respects, indeed, the author wishes the present contributions to be considered as a necessary complement to his work on the Nature and Treatment of the Diseases of the Ear, published in 1836, and noticed in the number of this Review for Jan. 1837.

The materials for the statistics are 2000 cases of diseases of the ear, all of which Dr. Kramer tells us he explored, by means of the speculum auris, the Eustachian tube catheter, the air-douche, &c., and carefully registered in his journal the results thus obtained as well as all that relates to age, sex, and residence of the patient; the date of origin and causes of the disease; the existence or absence of tinnitus and other symptoms; the hearing distance for each ear; the treatment, if any, adopted before the patient's application to the author; the author's own treatment; duration, and event.

In a system of the diseases of the ear, it is, says Dr. Kramer, "my opinion that those diseases only should be admitted which, in consequence of the peculiar structure of the organ affected, and the peculiarity of its function as an organ of sense, are accompanied by such peculiar symptoms, that they are thereby distinguishable from all other diseases of the body."

In the classification of the diseases of the ear, Dr. Kramer says he has followed that laid down in his treatise above referred to, the more decidedly, as it is founded on the very simple and natural principle of anatomical difference of the structures composing the organ of hearing; the morbid states of which, moreover, present such definite characters, that in the course of fifteen years not a single case has occurred to him which does not find its place in his system, in a natural and unforced manner.

In the present work, for the sake of more fully carrying out his principle of classification, he goes on to say, he has changed the nomenclature

of the diseases of the auricle as follows. Instead of the names *erysipelatous inflammation, scirrhous degeneration* and *boils,* he employs respectively *inflammation—of the epidermis, of the dermis,* and of *the cellular tissue of the auricle.* For the same reason, *erysipelatous inflammation of the auditory passage* he now calls *inflammation of the epidermis of the auditory passage.*

It is, doubtless, a good principle in defining and classifying diseases to make particular reference to the structure affected, but it ought not to be pushed too far. Diseases are seldom entirely confined to a single structure, and a secondary disease is often very different in its nature and even seat from the primary which gave rise to it, and cannot, therefore, well be described under the head of the latter. Dr. Kramer, we must confess, in pushing the simplicity of his system so far as he does, loses sight of these considerations, and is thus often betrayed into the very hypothetical and speculative admissions which he takes so much pains to protest against.

In substituting the name of "inflammation of the epidermis" for "erysipelatous inflammation," Dr. Kramer certainly not only pushes his principle too far, but also makes hypothetical and speculative admissions not only uncalled for by, but positively opposed to, practical utility. Everybody knows what is meant by erysipelatous inflammation; but the name inflammation of the epidermis, even if we were for a moment disposed to admit it as correct, could be admitted only as a generic not as a specific name. The name "scirrhous degeneration" Dr. Kramer was right to drop, but we reject the name he would substitute for it, inflammation of the dermis of the auricle. There are various inflammations of the dermis. The disease Dr. Kramer refers to appears to be crustaceous herpes, or an affection resembling elephantiasis. Again, the substitution of the name of "inflammation of the cellular tissue" for boils, thus doing away with the distinction between boils and abscesses, is by no means warranted. What has just been said of the substitution of the name of inflammation of the epidermis for erysipelatous inflammation in the case of the auricle, is, of course, applicable to the erythema of the skin of the auditory passage, in which accumulation of wax has its origin.

The way in which Dr. Kramer describes and arranges this same accumulation in the auditory passage, under the head of "inflammation of the epidermis of the auditory passage," illustrates the absurdity to which principles carried out to an extreme will lead.

In articles on the ear in the Numbers of this Review for Jan. 1837, and July 1839, we took occasion to refer to an opinion of Mr. Swan's, that in a great proportion of the cases commonly put down as nervous deafness, the auditory nerves are not affected; but that the deafness is owing to a thickened state of the lining membrane of the cavity of the tympanum, involving the tympanic nerves, and that the effect of Dr. Kramer's treatment of nervous deafness by means of vapours of acetic ether was more explicable according to this view of the pathology, than according to that generally received and adopted by Dr. Kramer.

We, however, remarked that perhaps the thickened state of the lining membrane of the tympanum, which Mr. Swan had observed in his dissections of the ears of habitually deaf persons, and which has also been observed

in numerous other cases on record; and its consequent effect on the fenestræ, ossicles, and membrana tympani, is sufficient to explain the deafness without having recourse to Mr. Swan's supposition, that the deafness in such cases is owing to implication of the tympanic nerves.

This view of ours, which was founded on numerous well reported cases and dissections on record, has been pretty generally admitted in this country, and, in additional support of it, Mr. Toynbee has made known a very large number of dissections in which he found thickening of the lining membrane of the tympanum.

We, at the same time, did not deny that many cases of habitual deafness are actually nervous; that is, cases of disease of the auditory nerve or brain; and that many are also affections of other structures in the labyrinth.

In the pages before us, Dr. Kramer labours hard to refute this view and to maintain his own, but we must say his arguments are none of the soundest.

In the first place he is altogether mistaken in supposing that the thickened state of the mucous membrane lining the tympanum, which we consider to be the morbid condition in a large number of cases of habitual deafness, is necessarily at all a stage of catarrhal inflammation of the middle ear; and, therefore, his elaborate comparison of catarrhal inflammation of the middle ear, and his nervous deafness, at pp. 190-1, as regards their characters and attendant circumstances, has no bearing at all on the question.

"The lining membrane of the tympanic cavity being fibro-mucous, at once periosteum and mucous membrane—inflammation of it will vary in its nature and consequences. It may be at one time simply the catarrhal affection of a mucous membrane; at another, the violent purulent *otitis interna*, in which the surrounding osseous textures become so readily involved. The morbid state of the lining membrane of the middle ear I am attempting to illustrate, appears to be of the nature of periosteal swellings. On dissection, the membrane which is naturally very thin and transparent, has been found thickened and swollen, sometimes to such a degree as to bury the ossicles and overlap the fenestræ."*

He also betrays rather confined views as to the pathology of nervous deafness. Any disease entitled to the name of nervous deafness, ought to be analogous to amaurosis. Now, as the morbid conditions of the paralysis of the optic nervous apparatus in amaurosis may vary in their nature and have different seats, so also may the morbid conditions of the paralysis of the auditory nervous apparatus.

TABLE I.—*General view of the diseases of the ear.*

Of the 2000 cases, 5 or 1-400th were cases of disease of the auricle; 281 or 1-7th, cases of disease of the auditory passage; 442 or 1-4th and a half, of the membrana tympani; 198 or 1-10th, of the middle ear; 1028 or half, nervous deafness, as understood by Kramer; and 46 or 1-43d, deaf-dumbness.

Auricle. Of the five diseases of the auricle, 1 was erysipelas; 3 induration and thickening; 1 boil or abscess. Disease of the auricle, however,

* Cyclopædia of Practical Surgery, art. Ear and Hearing, Diseases of.

Kramer admits may be more frequent than is here represented, as they are not of a nature to have come under his especial notice.

External auditory passage. Of the 281 diseases of the external auditory passage, 213 were cases of concretions of wax from erythematous inflammation of the skin of the passage; 51 cases of catarrhal inflammation of the passage, or of what Dr. Kramer calls inflammation of the glandular skin; 9 were phlegmonous inflammation, and 8 periostitis and caries. The last three diseases of the auditory passage, viz., catarrhal inflammation, phlegmonous inflammation, and periostitis with caries, are accompanied by discharge from the ear; but the cases of discharge from the ear depending on them, in number 68, form but a small proportion, 1 in 7 of the whole number of 510 cases of discharge from the ear. In the remaining 6-7ths the discharge from the ear depends on inflammation of the membrana tympani.

Membrana tympani. The 442 cases of disease of the membrana tympani, were cases of inflammation and its consequences; and of these 45 were cases of acute inflammation, and 397 cases of chronic inflammation.

The diseases of the membrana tympani are twice as numerous as all the diseases of the auditory passage taken together, and as above stated the source of about 6-7ths of all discharges from the ear.

Nearly every fourth case of dullness of hearing has for its cause inflammation of the membrana tympani or its consequences.

In inflammation of the membrana tympani, the auditory passage in general remains unaffected.

Middle ear. Of the 198 cases of disease of the middle ear, 164 were catarrhal inflammation of the mucous membrane with muculent obstruction; 28 inflammation with stricture of the Eustachian tube; 2 obliteration of the Eustachian tube; and 4 inflammation and abscess of the cavity of the tympanum.

Catarrhal inflammation of the middle ear with muculent obstruction thus forms 1-12th of the whole 2000 cases of ear disease, and about 4-5ths of all the diseases of the middle ear.

This frequency of catarrhal inflammation of the middle ear is less than might have been expected from the proximity of the middle ear to the nose and throat, which are frequently the seat of catarrhal affections. It is, indeed, not uncommon for a slight degree of deafness to occur in ordinary nasal catarrh, but it in general subsides along with the latter; in such cases it is probable that the orifice of the Eustachian tube merely is affected. Very rarely is the cavity of the tympanum involved, but when it is, it will remain so long after the subsidence of the nasal and guttural affections.

Stricture of the Eustachian tube in a decided degree occurs rarely, 1 : 71 of the whole 2000 cases, and 1 : 7 of all the diseases of the middle ear.

Complete closure of the Eustachian tube is very rare.

Inflammation and abscess of the cavity of the tympanum. Dr. Kramer thinks that this does not occur as an idiopathic acute disease except when traumatic; but that it always supervenes on a chronic inflammation of the lining membrane of the tympanum, existing in connexion with chronic inflammation and perforation of the membrana tympani.

Internal ear. Of the 1074 cases of disease of the internal ear, 1028 or

rather more than half of the whole 2000, are put down as cases of nervous deafness; the remaining 46 were cases of deaf-dumbness.

Dr. Kramer concludes his commentary on Table i with the following observations on the exploration of the ear in order to a diagnosis:

"If we now take a glance at the diagnostical relations of the diseases of the ear which have been thus passed in review, it will be found that those which can be at once recognized on simple examination, viz. the diseases of the auricle, are the very fewest in number and the least important. All the other diseases of the ear require for their diagnosis special means of exploration. For the diseases of the external auditory passage and of the membrana tympani, the employment of the speculum auris in clear sunshine is necessary; but by means of them the diagnosis of the diseases in question may be made with as much certainty as the diagnosis of any superficial affection of the skin. Without these means of exploration, however, it is quite impossible to acquire, even in the most remote degree, a correct idea of the seat and peculiar nature of the diseases of the external auditory passage and of the membrana tympani, and just as little to strike out the right way to their successful treatment. According to the above table, the diseases of the external auditory passage and of the membrana tympani constitute about a third (728) of all diseases of the ear, and that of this number the diseases of the membrana tympani constitute two thirds (442). The importance of the membrana tympani to the integrity and function of the organ of hearing is especially evident from Tables v and xiii, even in the absence of more decisive proof.

"The remaining two thirds of all diseases of the ear (1272) have their seat in parts of the organ which lie behind the membrana tympani, and are thus completely withdrawn from ocular observation. The speculum auris here assists merely in the determination that no disease of the auditory passage or membrana tympani exists, and that therefore the seat of the disease must be looked for in the middle or internal ear. In this case the employment of the Eustachian tube catheter is the only means, in order, by blowing through it, or by sending in a stream of air from the air-press, or by the introduction of a catgut string, a whalebone or ivory probe, to get acquainted with the state of the Eustachian tube and cavity of the tympanum, and from thence to conclude as to the state of the auditory nerve. Much, indeed, here depends on an experienced, fine touch and hearing, especially on the latter, in judging of the sound produced by the air streaming into the tympanum from the Eustachian tube catheter. The hearing, it is to be admitted, does not afford us such trustworthy indications of certain morbid states as vision; but this is by no means any reason for rejecting the assistance which those means afford us in investigating deeply-seated diseases of the ear.

"From all this it will be clear to every unprejudiced person, that no disease of the ear, which has not its seat in the auricle, should be treated, until it has been most carefully explored by means of the speculum, and in the cases requiring it, by means of catheterism, &c., also, and its nature fully ascertained." (p. 26.)

Table ii.—*Of the relative frequency of diseases of the ear in the two sexes.*

Of the subjects of the whole 2000 cases 1274 were males and 726 females = $1\frac{5}{7}$ males to 1 female.

Auricle. Of the five diseases of the auricle, the subjects of the one case of erysipelas and the one case of boil were males, and the subjects of the three cases of eczematous disease were females.

External auditory passage. Of the subjects of the 281 diseases of the external auditory passage, 207 were males and 74 were females. Of the subjects of the 213 cases of accumulation of wax from erythematous inflammation of the skin of the auditory passage, 161 were males and 52

females = 3 : 1. Of the subjects of the 51 cases of inflammation of the tegumentary lining of the auditory passage and ceruminous glands, or as Dr. Kramer calls it, inflammation of the glandular skin, 34 were males and 17 females = 2 : 1. Of the subjects of the 9 cases of phlegmon 6 were males and 3 females = 2 : 1. Lastly, of the subjects of the 8 cases of periostitis and caries of the auditory passage 6 were males and 2 females = 3 : 1.

Membrana tympani. Of the subjects of the 442 cases of disease of the membrana tympani, 311 were males and 131 females = $2\frac{1}{3}$: 1. Of the subjects of the 45 cases of acute inflammation 33 were males, and 12 females = 3 : 1. And of the subjects of the 397 cases of chronic inflammation 278 were males, and 119 females = $2\frac{1}{3}$: 1.

Middle ear. Of the subjects of the 198 cases of the diseases of the middle ear admitted by Dr. Kramer, 141 were males, and 57 females. Of the subjects of the 164 cases of muculent obstruction of the middle ear, 117 were males, and 47 females = $2\frac{1}{2}$: 1. Of the subjects of the 28 cases of stricture of the Eustachian tube, 20 were males, and 8 females = $2\frac{1}{2}$: 1. Of the subjects of the 2 cases of obliteration of the Eustachian tube, one was a male, and the other a female. Of the subjects of the 4 cases of otitis interna 3 were males, and 1 a female.

Internal ear. Of the subjects of the 1028 cases of Dr. Kramer's nervous deafness, 581 were males, and 447 females = $1\frac{1}{3}$: 1. And of the subjects of the 44 cases of deaf-dumbness, 32 were males, and 14 females = 2 : 1.

TABLE III.—*Of unilateral and bilateral affections of the ears.*

In determining whether one ear only or both are at the same time affected, the patient's account alone must not be relied on, for very often the patient is under the impression that he hears quite well with one ear when he only hears *not so ill* as with the other;—or he complains of discharge from one ear alone, though there is suppuration in the other also, only the matter being thick and small in quantity does not run out. This shows the necessity in every case of testing both ears as to the distance at which they hear a watch, and of exploring their organic condition.

Of the 2000 cases, one ear only was affected in 361, and both ears in 1639 = 1 : $4\frac{1}{2}$. Of the 361 cases of unilateral disease, the left ear was affected in 167, and the right in 194.

Auricle. Of the 5 cases of disease of the auricle, the left ear was affected in 2, the right in 1, and both in 2.

External auditory passage. Of the 281 cases of disease of the external auditory passage, the left ear was affected in 54, the right in 62, and both in 165. Of the particular diseases of the external auditory passage, it is to be observed, that phlegmonous inflammation of the auditory passage occurred without exception on one or other side only, that inflammation of the tegumentary lining of the auditory passage and ceruminous glands (catarrhal inflammation), and that periostitis and caries of the auditory passage occurred oftener on one side than on both = 2 : 1, but still often enough on both.

Membrana tympani. Acute inflammation of the membrana tympani occurred 21 times in the left ear, 22 times in the right, and twice only in both at the same time.

In the diseases of all the other parts of the ear both sides were much

more frequently affected than one only. This was remarkably so in Dr. Kramer's nervous deafness, of the whole number of cases of which 44 only were unilateral, whilst 984 were bilateral = 1 : 22.

If we compare the extraordinary rarity of cases of acute inflammation of the membrana tympani on both sides, viz. 1 : 22 with the great frequency of chronic inflammation of both sides at the same time, viz. 2 : 1, it is evident that the latter inflammation can by no means frequently occur as a sequela of the former, but that the chronic inflammation most commonly comes on unpreceded by an acute stage. Indeed it is probable that acute inflammation has little tendency to pass into the chronic form.

Table iv.—*Of Tinnitus Aurium.*

Of the whole 2000 cases of disease of the ear, there was tinnitus in 1267, and none in 733. All the deaf and dumb were free from tinnitus.

In cases of accumulation of wax in the auditory passage, in acute inflammation of the membrana tympani, and in Kramer's nervous deafness—all essentially different diseases—tinnitus occurred on an average in 3 cases out of 4; whilst in catarrhal inflammation of the auditory passage, in phlegmonous inflammation of the same, in *otitis interna*, and in muculent obstruction of the middle ear, tinnitus was as often absent as present. In chronic inflammation of the membrana tympani, tinnitus was absent twice as often as it was present.

Next to dullness of hearing, tinnitus is the most common complaint of the ear patient. It is very rare that tinnitus occurs without some change in the power of hearing.

Tinnitus varies much in regard to the character of the sound, and in regard to its continuance or intermission, its early or late occurrence in the disease or its period of cessation. But all the varieties of tinnitus may accompany without distinction all the individual diseases of the ear; whilst, on the other hand, almost every disease of the ear may through its whole course remain free from it; and that without any probable reason for its presence in one case or its absence in another. Tinnitus thus appears not to have the slightest value pathognomonically, prognostically, nor even, unless Dr. Kramer's nervous deafness be in some degree excepted, therapeutically.

In accumulations of wax in the auditory passage tinnitus sometimes exists, sometimes not. When present, it subsides along with the deafness on the removal of the wax. The same is the case in muculent obstruction of the middle ear.

Under these circumstances, it is not to be wondered at that similar anomalies as to presence or absence of tinnitus in Kramer's nervous deafness should occur. Dr. Kramer has therefore found reason to give up the division of nervous deafness he formerly adopted, viz. with or without tinnitus, or erethitic and torpid, and to comprehend both under the head of simple nervous deafness.

Table v.—*States of the membrana tympani in chronic inflammation of it.*

Of the 397 cases of chronic inflammation of the membrana tympani, both ears were affected in 279, and one ear only in 118; there were thus 676 individual tympanic membranes in a state of chronic inflammation observed.

In 118 cases out of the 397, i. e. scarcely 1 in 3, the disease was unac-

companied by polypi or perforation, but in the remaining 279 cases, i. e. more than 2 in 3, the membrana tympani was either perforated (217 times), or beset with polypous growths (99 times), or in both states at the same time (37 times). Now as most discharges from the ear (6-7th) depend on chronic inflammation of the membrana tympani with perforation or polypus, the prognosis and treatment of the discharge resolve themselves into the prognosis and treatment of the diseased membrana tympani.

It is oftener the case that the membrana tympani is perforated (129 : 88) or beset with polypi (75 : 24) in one ear only than in both at the same time. When one ear only is thus affected, it is generally the right, much more rarely the left (77 : 52 and 44 : 31). This is in accordance with the fact above pointed out, that the right ear more frequently suffers alone than the left.

Polypi are very rarely observed along with perforation of the membrana tympani $37 : 397 = 1 : 10\frac{1}{2}$.

Perforations of the membrana tympani are very seldom so fine as a pinhole ($\frac{1}{6}$), more frequently they are of the size of a pea or bean ($\frac{2}{6}$), in which case a mere sickle-shaped stripe of the membrane remains. Most commonly the perforations are as large as lentils ($\frac{3}{6}$).

Of 305 cases of perforation, tinnitus was absent in 188—more than the half—and was present in only 117. Proportionally it occurred most frequently when the perforation was of the size of a bean (3 times in 5); somewhat less frequently when the perforation was as small as a pinhole (3 in 7), most rarely when the perforation was of the size of a lentil (3 in 9).

TABLE VI.—*Simultaneous occurrence of several diseases of the ear.*

Dr. Kramer found one and the same ear affected with more than one disease in 38 cases only out of the whole 2000, which is 1 : 52; and the two ears of the same person affected with two different diseases in 66 cases out of the whole 2000 which is 1 : 43.

The diseases most commonly found, the one in the one ear, the other in the other, were:—1. Accumulation of wax in the auditory passage on one side, and Dr. Kramer's nervous deafness on the other (5 times). 2. Chronic inflammation of the membrana tympani on one side, and muculent obstruction of the middle ear (10 times), or nervous deafness (18 times) on the other.

The diseases most commonly found in company in the same ear were as follows: 1. Accumulation of wax in the auditory passage and muculent obstruction of the middle ear occurred only twice in combination in 213 cases of the one, and 164 cases of the other. 2. Accumulation of wax with nervous deafness in one and the same ear occurred 16 times in 1028 cases of nervous deafness, which is 1 : 62. In nervous deafness, it is to be remembered that ordinarily the ceruminous secretion and also the mucous secretion of the middle ear are diminished. 3. Catarrhal inflammation of the auditory passage may be accompanied at the most by a slight degree of inflammation of the membrana tympani, never ending in suppuration, ulceration, or other change of structure.

In 305 cases of chronic inflammation of the membrana tympani with perforation, *otitis interna* occurred in six cases.

In inflammation of the membrana tympani without perforation, the

middle ear never was at the same time affected with muculent obstruction.

In erysipelas of the auricle the swelling closes the entrance of the auditory passage, but the inflammation extends no farther.

Accumulation of wax is not accompanied by any disease of the membrana tympani, however long it may have existed, except at the most some slight congestion, which disappears spontaneously. Indeed, reappearance of wax is a good indication of the subsidence of chronic inflammation of the membrana tympani.

In phlegmonous inflammation of the auditory passage, the swelling completely closes the passage, so that no examination of the membrana tympani can be made; but after the abscess has burst, the membrana tympani is found for a short time dim and impaired in transparency.

Inflammation of the mucous membrane of the tympanum with muculent accumulation, Dr. Kramer says, does not spread either to the membrana tympani or to the labyrinth. As soon as the mucus is evacuated the hearing, he says, is restored. He has satisfied himself in more than 164 cases of the kind under notice of the perfect soundness of the membrana tympani. As little is increased secretion succeeded by too great dryness.

Table vii—*Exhibits the situation and mode of life*, and Table viii, *the country and residence* of Dr. Kramer's 2000 patients, but nothing of any consequence admits of being elicited from them.

Table ix.—*Age at which Dr. Kramer's patients became affected.*

From this table it appears that of the 2000 cases of diseases of the ear 504 came on in the first ten years of life = 1 : 4. Of these 504 cases 241 or a half, were cases of chronic inflammation of the membrana tympani; of the whole number of cases of chronic inflammation of the membrana tympani, viz. 397 observed, 2-3ds had their origin in the first ten years of life.

In the second decennial period fewer diseases of the ear have their origin, but in the third decennium the number again increases. From this period the frequency of first attacks of disease of the ear rapidly diminishes, so that from 60 to 70 there is scarcely a single new attack.

The individual diseases made their first attack at the periods of life as follows:

Accumulation of wax in the auditory passage. This most frequently first occurred at the age of from 20 to 40; most rarely before the 10th year and after the 60th.

Catarrhal inflammation of the auditory passage. Most common below 40, less common after.

Phlegmonous inflammation of the auditory passage most common about 20.

Caries of the auditory passage in most of the cases occurred in the first decennium, and was dependent on the scrofulous diathesis.

Acute inflammation of the membrana tympani, which is quite independent of any diathesis, was most frequent from 20 to 40.

Chronic inflammation of the membrana tympani, always dependent on the scrofulous diathesis, occurred in the proportion of 2-3ds in the first decennium, and of this in the first two years—excited by the exanthemata. This thus does not generally arise from an acute attack, but comes on as chronic from the first.

Muculent obstruction of the cavity of tympanum comes on especially in the first decennium (2-5ths). In youth it occurs in the proportion of 1-5th, but diminishes after the 20th year. It is always from the first accompanied by great deafness; as both ears are generally at the same time affected, the deafness is very troublesome. By-and-by one ear becomes somewhat free, = 1 : 6, whilst the other remains more obstructed.

Inflammation of the mucous membrane of Eustachian tube with stricture most commonly affects one side only (1 : 2), and does not come on so quickly as muculent obstruction; therefore the precise time of origin is not always ascertained.

Nervous deafness (1 : 8) also comes on slowly, so that its precise time of origin is not certain; thus differing from catarrh of the middle ear. It would appear first to come on most commonly in the third decennium from violent cold, mental affections, &c. Within the first ten years and after the 60th year, the disease comes on for a first time very seldom (1 : 171). It first begins in one ear, proceeds slowly, and only after a long time perhaps, affects the other.

Table x.—*Age of the patients at the time they first consulted Dr. Kramer.*

It is only in the case of diseases attended either by severe pain, or by great and sudden deafness, that the age at the time of consultation corresponds with that at the time of attack. In the other cases a more or less considerable time had been allowed to elapse before the patients applied for assistance.

Table xi.—*Duration of the diseases at the time of consultation.*

From this table it is quite evident that by far the largest proportion of diseases of the ear are decidedly chronic in their nature. Only 164 patients out of the whole 2000, i. e. 1-12th, had, at the time of their applying to Dr. Kramer, laboured under the complaint for a period less than four weeks; 1650 had been already labouring under the complaint for a year and more.

Only three diseases of the ear can be considered acute, viz. erysipelas of the auricle, acute inflammation of the membrana tympani, and phlegmonous inflammation of the walls of the auditory passage; all the other diseases have either a decidedly chronic course, or have a tendency to become acute only under particular circumstances, such as catarrhal inflammation of the external auditory passage.

Table xii.—*This table shows the different distances at which Dr. Kramer's* 2000 *patients heard the ticking of a watch.*

It is of consequence to have some standard whereby to measure the power of hearing. The ticking of a watch is the most convenient for the purpose. By a sound ear a loud-ticking watch may be heard at the distance of thirty feet. Persons who do not hear the ticking of a watch at all, even when the watch is directly applied to the ear, hear what is said to them only when the speaker is quite close to them and speaks loud, and it is little better even when the watch is heard when directly applied to the ear. It is only when the hearing distance of the watch is as much as inches and feet that the power to follow conversation is at all possessed. Here it is to be remembered that the susceptibility of the ear to the human voice is not always in proportion to its susceptibility to the ticking of a watch. The

power to follow conversation is sometimes greater than might be supposed indicated by the shortness of the distance at which a watch is heard; but, on the other hand, it is also sometimes considerably less.

In regard to the diagnostic value of the distance at which a watch is heard, it is to be observed that of itself it is of little importance; more importance is to be attached to the nature of the disease than to the degree of accompanying deafness at the time.

Though the patient may complain of defective hearing on one side only, the surgeon ought not to omit to examine the other, for it will generally be found more or less affected also. See Table III.

In Dr. Kramer's 2000 patients both ears—therefore 4000 ears—were examined as to their hearing distance, but as in 361 cases one ear only was affected, the sound ears of those 361 cases must be deducted from the 4000, when there remain 3639 morbid ears.

From the table, it appears that in all diseases of the auditory passage, membrana tympani, middle ear, and internal ear, a great degree of deafness may occur, but it is in disease of the internal ear that it is most frequent. In the two cases of obliteration of the Eustachian tube, the watch was no longer heard.

Otitis interna, inasmuch as it occasions complete disorganization of the organ, was in 3 of the 4 cases most destructive to hearing.

Dr. Kramer's nervous deafness. Of the 1028 patients affected with nervous deafness, 214 no longer heard the watch with either ear. Of these 1 only was under 10 years of age, but having learned to speak before the deafness came on, he still retained the faculty.—151 patients no longer heard the watch with one ear, and of these it was the left ear in 77, and the right ear in 74. Those cases in which there is nothing to be hoped for from treatment amounted to 365, which is 1-4th of all the cases of nervous deafness, and of them one half was below 40 years of age.

Of the whole number of affected ears, viz. 3639, or of the whole number of ears affected with nervous deafness, viz. 2012, 217 ears (1 : 9) still heard the watch on immediate application, but in these cases the prospect of improvement is scarcely more favorable than in the former.

339 ears heard the watch at the distance of an inch, and 595 at the distance of a foot. In these degrees of nervous deafness, experience has taught Dr. Kramer that what may be considered as *a very good result of 3 to 6 months' treatment*, is an increase of the hearing distance from 1 inch to 1 foot in the one set of cases, and from 1 foot to 3 feet in the other.

In chronic inflammation of the membrana tympani, though it most generally occasions very considerable organic changes, the highest degree of deafness is only half as frequent as it is in nervous deafness. The lesser degrees of deafness, viz. 1 foot to 3 feet, are the most frequent, one half being about 1 foot and 1-6th about 3 feet. At the same time, however, it is to be observed that the remarkable fact comes out, that with apparently similar organic changes in the membrana tympani there is great difference in the accompanying deafness, and *vice versâ*, with very striking differences in the state of the affected membrana tympani, the same degree of moderate or great deafness may occur. This contradiction is especially frequent when there is perforation of the membrana tympani, and manifests itself here by the impossibility in which we find ourselves to recognize and determine the changes which, in addition to perforation of the membrana

tympani, may exist in the parts contained in the cavity of the tympanum.

In chronic inflammation of the membrana tympani in whatever degree, when the patient does not hear the watch, or even when he hears it on application or at a very short distance, there is little or no hope of restoration of hearing even though the inflammation be removed. The prognosis is more favorable only when the hearing distance is several inches, though the prospect of improvement is not so certain in this case, as the impossibility of improvement is in the former.

In periostitis and caries of the auditory passage and in stricture of the Eustachian tube, the highest degrees of deafness are very frequent and are always in exact proportion to the greatness of the organic change of the parts.

But in phlegmonous inflammation of the auditory passage; acute inflammation of the membrana tympani; catarrhal inflammation of the auditory passage; accumulation of wax in the auditory passage; and muculent obstruction of the middle ear, the highest degrees of deafness more rarely occur, notwithstanding very considerable changes of the parts. The highest degrees of deafness, when they do occur, have just as little influence on the prognosis as all the other degrees of impaired hearing. All the diseases under notice are not only decidedly and fully curable, but also the accompanying deafness in whatever degree; the hearing distance is of no consequence in assisting the prognosis, but its progressive increase indicates the progress of the cure.

Table xiii.—*In chronic inflammation of the membrana tympani the watch was heard as follows:*

a. Chronic inflammation with perforation. In 217 patients there were 305 membranes in this state; of these 305 affected ears, there was total deafness in 50, an inch of hearing distance in 80, a foot hearing distance in 113, three feet hearing distance in 50, above three feet hearing distance in 9, and undetermined hearing distance in 3.

b. Chronic inflammation without perforation. In 180 patients there were 359 ears affected; of these 359 ears there was total deafness in 42, one inch of hearing distance in 88, one foot of hearing distance in 148, three feet of hearing distance in 51, above three feet in 19, and undetermined in 11.

Total deafness occurred proportionally most frequently, viz. in 1-5th, among cases of small sized perforation of the membrana tympani, less frequently, viz. 1-6th, in cases of perforation the size of a lentil, most rarely, viz. 1-8th, when there was no perforation of the membrana tympani.

A hearing distance of an inch was on an average as frequent in the different degrees of perforation, as in non-perforation of the membrana tympani = 1 : 4; whilst the hearing distance of a foot was proportionally most frequent among the cases in which the membrane was not perforated = $\frac{1}{3} : \frac{1}{2}$.

A hearing distance of about three feet occurs most frequently in cases of considerable loss of substance of the membrana tympani = 1 : $4\frac{1}{2}$, less frequently in cases of small perforation or non-perforation = 1 : 7, least frequently in lentil sized perforation.

A hearing distance of more than three feet occurred most frequently in

cases in which the membrana tympani was not perforated = 1 : 18, and in cases in which it was perforated more frequently in those of the size of a lentil = 1 : 23.

From these observations it results, that in chronic inflammation of the membrana tympani, the hearing is more frequently less deteriorated when the membrane is still unperforated than when it is perforated; and, therefore, that there is little prospect of advantage from the operation of perforating the membrana tympani. If, however, the operation should in any case be had recourse to, as large an opening as possible should be made, as it appears that the hearing was more frequently less deteriorated in cases in which the perforation was large than when it was small. It is still at the same time to be remembered, that in cases of lentil-sized perforation of the membrana tympani, the highest degree of deafness occurred four times more frequently than the lesser degrees, so that the prospect of much benefit even from a large opening is very small.

Lastly, it appears from the table that in none of the cases of perforation was the hearing perfectly unimpared. In 9 cases only did the hearing distance exceed 3 feet (in one 12 feet, in another 10 to 30, but not normal). With such degrees of hearing, however, conversation is so readily carried on by the attentive and careful patient, that an unobservant person might be led to suppose that partial destruction of the membrana tympani is consistent with perfectly good hearing, which is not the case, for when the membrana tympani is perforated, the hearing is always more or less impaired.

TABLE XIV.—*The patients affected with nervous deafness to such a degree that they no longer heard the watch, were aged as follows:*

Of those who no longer heard the watch with either ear, 214 in number, more than a half (109), and of those who no longer heard the watch with one ear, in number 151, less than a half (71), were in the prime of life, below 40 years of age, and with few exceptions in the enjoyment of good general health. Of the persons above 40, many had laboured under their deafness from early life, and therefore may properly be referred to the preceding category. From these facts it would appear that Dr. Kramer's nervous deafness does not depend on old age or general exhaustion.

TABLE XV.—*The patients at the time of consultation laboured under other diseases besides that of the ear, as follows:*

More than 4-5ths of the patients were otherwise quite well and had hitherto been so, so that the affection of the ear was quite local and independent. Local treatment alone was employed, and indeed there was no indication for general treatment of any kind.

In the remaining fifth there were complications; the most frequent of which was general nervous weakness, which, however, almost exclusively occurred in accompaniment with nervous deafness, never in inflammation of the mucous membrane of the middle ear.

Inflammation of the mucous membrane of the middle ear, on its part, very frequently occurs in combination with scrofula; (there were 26 cases of this complication in the whole 164 cases of muculent obstruction of the middle ear, or 1-6th;) and with catarrh (there were 18 cases of this complication or 1-9th). Catarrh did occur along with nervous deafness but rarely.

In the cases of inflammation of the glands of the auditory passage, the scrofulous diathesis occurred in the proportion of 1 : 8. In chronic inflammation of the membrana tympani it occurred in the same proportion. Gout and rheumatism were rare.

Catarrh was most frequent in cases of muculent obstruction of the middle ear, and especially in stricture of the Eustachian tube. It did occur but rarely in nervous deafness.

Vertigo occurred in 1 case of accumulation of wax, in 3 cases of polypus of the membrana tympani, and in 7 cases of nervous deafness.

Hemorrhoids occurred in 7 cases of nervous deafness.

Phthisis in 1 case of caries of the middle ear, and in 4 cases of nervous deafness.

In the acute inflammations there was more or less inflammatory fever.

TABLE XVI.—*Causes of the diseases of the ear in Dr. Kramer's* 2000 *patients.*

The actual commencement of many of the diseases of the ear is frequently quite unobserved, pain seldom occurring; and as regards discharge, deafness, and tinnitus, they often occur only after the proper disease has already existed some time. Attention not being paid to this circumstance, and the discharge, deafness, or tinnitus being taken as a manifestation of the commencement of the disease, influences to which the patient may have been accidentally exposed at the time of the occurrence of those symptoms, are often put down as the cause of the disease. Hence the determination of the causes of diseases of the ear is often very difficult.

In more than one half of the whole 2000 cases, viz. in 1109 the cause remained undetected.

Hereditary predisposition to nervous deafness is not unfrequent, in the proportion of 1 : 6; but the same predisposition to muculent obstruction of the middle ear, and to chronic inflammation of the membrana tympani is comparatively rare : the former five times more rare, the latter nine times.

Cold appears to have been the most common cause of acute inflammation of the membrana tympani (2-3ds), and phlegmonous inflammation of the auditory passage (one half). It is also a frequent enough cause of the inflammation leading to muculent obstruction of the middle ear (1-3d), of the erythematous inflammation leading to accumulation of wax in the auditory passage (1-3d), and of the inflammation of the glands of the passage (1-3d). It much more rarely happens (1-10th) that cold can be with certainty determined to have been the cause of Dr. Kramer's nervous deafness, and of chronic inflammation of the membrana tympani (1-14th).

The exanthemata, and other diseases of the skin, are frequent causes of chronic inflammation of the membrana tympani, especially scarlet fever, this having been the cause of 1-4th of all Dr. Kramer's cases of chronic inflammation of the membrana tympani.

Only 1-68th of all Dr. Kramer's cases of nervous deafness appear to have had their origin in scarlet fever.

Nervous and gastric fevers sometimes though rarely call forth chronic inflammation of the membrana tympani and nervous deafness. Two cases of muculent obstruction of the middle ear appear to have been called forth by gastric fever; when thus excited these diseases were much developed and proved very obstinate.

Blows on the ear excited inflammation of the glands of the auditory passage in three cases. Chronic inflammation of the membrana tympani in 12, and nervous deafness in 24. Nervous deafness was also in some cases instantaneously produced in the greatest degree by injury of the head and spine.

Disease of the brain with convulsions in infants, was a common cause of deaf dumbness (1-3d).

Great noise, sorrow, severe toothache, exhausting hemorrhage, were sometimes the cause of nervous deafness.

TABLE XVII.—*Perforation of the membrana tympani occurred after scarlet fever, measles, cold, and smallpox*, thus:

Chronic inflammation of the membrana tympani, excited by cold or smallpox, most frequently in proportion led to perforation, viz. in the former case 22 : 29 = 3 : 4, in the latter 9 : 11 = 3 : 4, more rarely when it occurred after scarlet fever 49 : 89 = 1 : 2, or measles 15 : 28 = 1 : 2.

It is the converse of what has now been stated, when the question is, by what cause is perforation on both sides most frequently occasioned. Perforation on both sides occurred most frequently after measles, 14 times in 15 cases, somewhat more rarely after scarlet fever 32 : 49 = 2 : 3, still more rarely after smallpox 3 : 9 = 1 : 3, most rarely after cold 6 : 22 = 1 : $3\frac{1}{2}$.

As to the extent of injury of the membrana tympani from the inflammation excited by the different causes under notice, it was proportionally greatest after smallpox: of 12 cases the perforation was in 5 the size of peas; more rarely of this size after measles, 8 times in 29 cases; after scarlet fever, 20 times in 81 cases; and from simple cold, 7 times in 28 cases.

Smallpox is thus most destructive of the membrana tympani, as it is the most frequent cause of perforation, and that of the greatest extent. Measles again is the most frequent cause of perforation of the membrane on both sides at the same time. Scarlet fever and cold were somewhat less destructive, but still destructive enough.

TABLE XIX.—*Results of Dr. Kramer's treatment of his* 2000 *patients.*

From this table it is seen that the curability and improvability of the different diseases of the ear is very different.

That cure or improvement of the organic lesion has taken place admits of being determined, in the case of the auricle, by direct inspection,—in the case of the auditory passage and membrana tympani, with the assistance of the *speculum auris*,—and in the case of the cavity of the tympanum and Eustachian tube, by means of catheterism and the air-douche. That cure or improvement of the hearing has taken place, is of course determined by the hearing distance. In the nervous deafness of Dr. Kramer, this is the only point which admits of being determined.

If in order for the cure or improvement of a disease of the ear to be admitted, its permanency be required, this is, says Dr. Kramer, altogether *unreasonable;* for no treatment in the world can render any diseased organ of the body, and therefore the ear, unsusceptible to renewed attacks.

Dr. Kramer evidently makes these remarks to meet the objection that cure or improvement of disease of the ear is very often not permanent. However true the remarks may be, the objection is not practically removed; what a patient desires, is to have his disease permanently cured or relieved,

and in the hope of this he subjects himself to the trouble and expense of treatment. But what is the advantage gained, if after the cessation of the treatment he quickly relapses into his former state?

As regards curability, Dr. Kramer divides diseases of the ear into four groups, viz. 1, The unconditionally curable; 2, The conditionally curable; 3, Those conditionally capable of amelioration only; 4, The unconditionably incurable.

1. *The unconditionally curable, notwithstanding long continuance, neglect, and very considerable development of the disease.* Under this head, Dr. Kramer places erysipelas of the auricle, boils of the auricle, accumulation of wax in the auditory passage, catarrh of the auditory passage, phlegmonous inflammation of the auditory passage, acute inflammation of the membrana tympani, catarrh with muculent obstruction of the middle ear.

Erysipelas and boils of the auricle do not require any particular remark.

Accumulation of wax and exfoliated epidermis in the auditory passage. The erythema of the integument of the auditory passage in which this originates, quickly passes off and does not become the subject of treatment. The plug of wax and exfoliated epidermis are readily removed.*

On the removal of the accumulation of wax and epidermis, there is in general immediate and complete cure of the deafness, tinnitus, confusion of the head, &c., with which the patient had been troubled. Any redness of the skin of the auditory passage and membrana tympani which may exist, spontaneously disappear. In Dr. Kramer's table there are 213 cases of such immediate and complete cure after, in some of them, whole years' continuance of the complaint.

Sometimes, though not often (16 times in the 2000 cases), along with the accumulation of wax in the auditory passage there is nervous deafness. In such a case of course the removal of the accumulation effects relief of the deafness, tinnitus, &c., only so far as these depend on the accumulation. After the removal of the wax the case in respect of its curability comes under Dr. Kramer's second group.

Catarrh or inflammation of the skin and glands of the auditory passage, always yields to acetate-of-lead lotions, counter-irritation, and gentle purgatives, and in recent cases very quickly. The disease is not so tractable when there is a disposition to skin and glandular disease generally. Besides the local, the appropriate general treatment is necessary, and months may be required for the cure. Of 51 cases of this kind 49 patients were eventually cured, whilst in the 2 others the patients withdrew themselves from treatment before a cure was effected.

Phlegmonous inflammation of the auditory passage. If this is not resolved by the application of leeches, it comes in the course of a few days under the application of emollient cataplasms to suppuration; and when the abscess bursts the symptoms quickly subside. In consequence of the closure of the auditory passage by the swelling, hearing is disturbed for a time, but it is completely regained on the subsidence of the swelling.

* To this, which is all that Dr. Kramer says, we would add, that sometimes, in addition to the exfoliation from the walls of the auditory passage, there is exfoliation of epidermis from the membrana tympani in numerous layers, which, being tinged brown with unhealthy wax, looks like a plug of wax deep in the passage. It sometimes adheres closely to the membrana tympani, and does not admit of being readily removed, and no attempt should be made to remove it. By and by, it becomes loose and comes away.

Acute inflammation of the membrana tympani. This generally yielded in a few days to leeches and counter-irritation, and acetate-of-lead lotions. Repeated leeching was necessary in severe cases; 1 case only out of 45 was longer than eight days under cure.

Inflammation of the mucous membrane of the middle ear with muculent obstruction. All his cases of this complaint, Dr. Kramer tells us, were without exception perfectly cured "by dispersion of the mucus, and by bringing the secretion of it back to the natural standard." To this result the local treatment by catheterism and the air-douche contributed most. In recent cases blowing through the catheter merely, was sufficient without using the air-press.

The subjects of most of these cases, we would observe, were young, and there can be little doubt that the recent cases at least would have recovered without catheterism, merely by the patient making expiratory efforts with the nose and mouth closed.

When the muculent obstruction has lasted long, or when the accumulated mucus is so viscid that it admits of being only partially dispersed, Dr. Kramer found that the deafness was only partially relieved, and that the mucus collected again very soon with consequent aggravation of the deafness. In such cases Dr. Kramer found it necessary to use the air-press to disperse the mucus. At the same time he observes that the *cold air* gave tone to the morbid membrane!

In patients with the scrofulous diathesis, Dr. Kramer, besides using the local treatment just mentioned, ordered appropriate diet and medicines.

2. *Conditionally curable diseases.* The conditions on which the curability in these cases depends, are slight development, short continuance, &c., of the disease. Under this head, Dr. Kramer places eczema of the auricle, periostitis, and caries of the osseous auditory passage, chronic inflammation of the membrana tympani, stricture of the Eustachian tube, nervous deafness.

Passing over *eczema of the auricle* and *caries of the osseous part of the auditory passage*, both of which are stubborn complaints, and not in their nature and treatment peculiar to the ear, we come to:

Chronic inflammation of the membrana tympani. When this, as is often the case, is connected with a dyscrasic state of constitution, its curability depends much on the curability or improvability of the latter. Its curability, however, depends in a much greater degree on the extent of organic change which the membrane has undergone. When the membrane has once become much thickened or perforated, there is no hope of a restoration of its natural structure. Polypi, which for the most part have their origin in chronic inflammation of the membrana tympani, may indeed generally be removed, except when they are broad, flat, and sessile; but, as the membrana tympani, at the same time that it is the part where the polypus has its root, is the seat of the other changes above mentioned, viz. thickening or perforation; the removal of the polypus does not effect very material improvement.

If, therefore, says Dr. Kramer, it be taken into consideration, that in one half of the whole number of cases of chronic inflammation of the membrana tympani, there was perforation of the membrane, and that in one fourth of the whole number of cases there were polypous growths, it is not to be wondered at that I have succeeded in perfectly curing 1-14th only

of the whole number of cases; and in improving only more or less 11-14ths so far that the suppuration was moderated, seldom quite removed, and not unfrequently the hearing somewhat improved. Two fourteenths of all the cases remained uncured.

Stricture of the Eustachian tube. The figure of the curability of this affection is still more unfavorable than that of the curability of chronic inflammation of the membrana tympani.

Nervous deafness. "As the little success of treatment," says our author, "in chronic inflammation of the membrana tympani and in stricture of the Eustachian tube is essentially owing to the invariable obstinacy of the organic changes of the affected parts, so in nervous deafness the results of treatment have been equally unfavorable in consequence of the too far advanced state of depression of the vital energy of the auditory nerves. They, in fact, not only are affected with simple diminished susceptibility for sound, even to complete deafness, but at the same time with a morbid susceptibility for impressions of every kind, increasing with the deafness. The consequence of this is, that the mildest applications only can be borne. Indeed in cases of much developed nervous deafness, the impossibility of any improvement depends principally on the impossibility of finding remedies which will operate gently enough on the auditory nerves and not over-irritate them." (pp. 179-80.)

It will be seen that these therapeutical speculations of our author are dictated by the pathological views he entertains regarding the cases he calls cases of nervous deafness, but from which views we have above altogether dissented. Practically, however, we equally admit the hitherto little curability of the affections referred to.

Besides these extremely unfavorable circumstances, Dr. Kramer encountered still another, viz., that 3-5ths of his patients labouring under nervous deafness had already passed their 30th year before consulting him, and that in more than half, the complaint had already existed longer than 10 years; and lastly, that in 2-7ths the deafness had already attained the highest degree—viz., either total deafness or incapacity to hear the watch on application.

Dr. Kramer either did not at all undertake the treatment, or, having undertaken it, soon relinquished it as hopeless in 5-20th, i. e. 271 of his patients, in consequence of their not bearing the mildest applications, although the circumstances of the case were otherwise favorable, such as youth and a moderate degree of deafness.

On the other hand, he succeeded in benefiting, both in regard to hearing and tinnitus, 14-20ths of his patients, i. e. 703, though in very different degrees, according to the age of the patient, the length of time the disease had existed, the degree of its development, and to the length of the treatment.

In 1-20th, or 54 of the cases only, was a perfect cure effected, but the attending circumstances were of the most favorable kind, such as inconsiderable development of the disease and youth of the patient.

In regard to his mode of treating nervous deafness, Dr. Kramer tells us that it is still essentially the same as that described in his treatise on the Nature and Treatment of the Diseases of the Ear, viz. the introduction into the cavity of the tympanum of stimulating vapours. Instead of the vapour of acetic ether, however, which he formerly recommended, but which he has of late years found too stimulating, and therefore not so

well borne, he recommends the vapour of *aqua assafœtidæ simplex*, of musk, of *aqua amygd. amar. sine spiritu parata*, and the like.

General treatment, in addition to this local treatment, he had recourse to only for other attendant complaints, the occurrence of which was on the whole rare. But he remarks, that although such concomitant complaints be removed by the general treatment, the nervous deafness may be little or not at all benefited. It is, however, he continues, always quite proper, before the commencement of the local treatment of the auditory nerve, to bring the general health into as good a state as possible.

3. *Diseases incapable of cure, but under peculiarly favorable circumstances admitting of amelioration.* Under this head Dr. Kramer places *otitis interna*. In otitis interna resolution sometimes takes place, but seldom. In most cases, besides destruction of the organ, there is danger of life.

4. *Unconditionally incurable diseases.* Under this head Dr. Kramer places obliteration of the Eustachian tube and deaf-dumbness.

Besides the "Statistics," of which we have now given a very full abstract, the work before us contains three other contributions, viz. on the Acoustics of the human ear, on Electro-magnetism as a remedy for dullness of hearing and tinnitus aurium, and on Otorrhœa cerebralis. The first and the last of these we pass over, but pause to give the following results of the experience of Dr. Kramer and others as to the value of electro-magnetism as a remedy in deafness and tinnitus aurium.

1. The magneto-electric or electro-magnetic current acts decidedly as an excitant on the organ of hearing.

2. This action is strongest when there is an affection of one or both auditory nerves, and the magneto-electric current is conveyed from the mouth of the Eustachian tube to the external auditory passage of the same ear, instead of from one auditory passage to the other.

3. The exciting action of megneto-electricity is manifested by convulsive twitchings, pains in the ear, momentary increase of the hearing distance (which, however, very soon diminishes again, or, though it continues some time, it does not amount to any great degree), and by aggravation of the tinnitus, either at the moment or some time after the sitting.

4. Magneto-electricity or electro-magnetism does not thus appear to possess any peculiar strengthening action on the auditory nerves. On the contrary, great care is necessary in the employment of the remedy not to over-excite the affected auditory nerves.

5. Hitherto the results of experience as to the use of the rotation apparatus in diseases of the ear are not encouraging. If, however, for any reason it be had recourse to, nervous deafness is the complaint for which it is most adapted; but that the complaint is nervous deafness ought to be first determined by careful instrumental exploration of the outer and middle ear.

6. The application of magneto-electricity ought always to be made with the greatest precaution and in the mildest manner, and always given up whenever, notwithstanding every precaution, decided aggravation of tinnitus takes place, and improvement of the hearing distance is not effected, or, when effected, it is again lost.

ART. III.

Klinische Ergebnisse gesammelt in dem Königlichen Policlinischen Institut der Universität, von dessen Assistenarzte Dr. EDUARD HENOCH, und herausgegeben von Dr. M. H. ROMBERG, &c. &c.—*Berlin*, 1846.

Results of Clinical Observation in the Royal Polyclinical Institute of the University of Berlin. Recorded by Dr. E. HENOCH, and edited by Dr. ROMBERG.—*Berlin*, 1846. 8vo. pp. 282.

THE results of medical art, as delineated in the present volume, presents points of interest. The name of the editor is a guarantee for the accuracy of the details, and stamps a value upon the clinical and practical information imparted. A field of action, such as the policlinic of a large city like Berlin, must present ample scope for therapeutical experience and experiment, and the practised eye of Dr. Romberg would not fail to view it aright. Further, the preface of the editor contains proofs, that he rightly appreciates the importance of his position as clinical instructor and the value of dispensary practice as a means of instruction.

The cases treated in the dispensary are arranged methodically. First come diseases of the nervous system, according to the neurology adopted by Dr. Romberg, (vide our review of his work in vol. XVII of this Journal): then follow in succession, diseases of the blood, of the alimentary canal, of the sexual and urinary organs, of the thoracic viscera and of the skin.

Cardiac neuralgia, or *angina pectoris*, was observed only once; it occurred in a tailor aged 40, and was complicated with disease of the aortal valves: death occurred suddenly. The pain was much relieved by small doses of nitrate of silver.

Hemicrania was also successfully treated by the same remedy.

Cases of *chorea* St. Viti were frequent. Nine girls, one boy, and two women, aged 24 and 48 respectively, were treated. The left side only was affected in one case; the right side only in three; and in the eight remaining, both sides. The treatment of the cases, in which intestinal worms were the apparent cause of the disease, consisted in the administration of calomel and jalap: when no causal indication was manifest, the infusion of senna was given first to operate on the bowels, and then chalybeates. The hydrochlorate of iron was found to be the most successful, and was prescribed for children in three-grain doses thrice a day, combined with powdered rhubarb. The arseniate of potass was however found to be the most efficacious remedy: a case of eight years' duration in a girl aged 11 years, and another of 3 years, are reported as recovering under its use. Dr. Romberg began with a dose of two drops of the arsenical solution, gradually increasing to six drops.

Infantile meningitis was ushered in by various symptoms. Convulsions, not unlike eclampsia or epilepsy, was one of these. A boy, aged 4 years, had convulsive paroxysms for three days, with intervals of perfect health. On the fourth and fifth days, he did not suffer at all; on the sixth, the convulsions reappeared, accompanied by the usual symptoms of meningitis. A girl, aged 8 years, in perfect health, was attacked with such violent convulsions, that they were pronounced to be epileptic by the practitioner

called in; she recovered perfectly, but in twelve hours meningitis set in, and proved fatal in six and thirty hours. In another case, the convulsions preceded the attack of meningitis fourteen days. Dr. Romberg observes, that the condition of the nasal mucous membrane constitutes a good diagnostic sign in these cases. If the secretion be arrested, and the nose be dry, then meningitis is impending. Other signs of meningitis in infants are, the continual attempt to bury the occiput in the pillow, and a frequent change of colour in the cheeks, and especially in one cheek only. Vomiting was often observed, but with these peculiarities, that it occurred when the stomach was empty, without nausea, without straining, and was brought on by moving or shaking the head. The expression of the eyes was noticeable. Children usually follow with their eyes the eyes of their attendants; in meningitis, they cease to do this.

In the majority of cases, the disease was tuberculous meningitis, and was accompanied by deposit of tubercle in other organs, particularly the lungs, bronchial glands, liver, spleen, and mesentery. In the brain, the deposit usually occupied the sulcous folds of the pia mater.

The cases of meningitis infantum were treated after a rigorous antiphlogistic method. Leeches were applied behind the ears, and large doses of calomel given so as to act briskly on the bowels. Cold was applied to the head after the plan recommended by Heim. The child's head is wrapped round the temples with woollen, so that the water may be prevented running over the face, and then cold water is poured upon the head, from the height of one or two feet, for a quarter of an hour. After an interval, the process is repeated.

Several cases of *epilepsy* were treated. Some of these displayed remarkable periodic phenomena. A boy at the age of 9 years, had an attack of epilepsy brought on by fright, the fit recurring every evening at a certain hour. It disappeared without any treatment being adopted. In his twelfth year, he fell on the ice while skating, having a headache and other cerebral symptoms at the time; and immediately he was again subject to periodic epileptic fits, recurring every night at ten or eleven o'clock. In another case, a young woman, the fits recurred every evening about five o'clock. In another case, the paroxysm recurred every month, about the time of the new moon; the whole paroxysm or attack continued twenty-four hours, and was made up of twelve fits, having an interval of coma of two hours' duration between each. In a note Dr. Henoch mentions the case of a young gentleman who had a fit with the greatest regularity every seventh day, from eight to half-past eight o'clock in the morning.

Paraplegia. The suppression of the perspiration of the feet is mentioned by Romberg as a cause of this disease. We are inclined to doubt the accuracy of the etiological views given by him, as it seems more probable that the suppression of the perspiration was rather a coincident than an antecedent. A healthy man, aged 46, who had been troubled from his youth with profuse perspiration of the feet, had suffered for two years past from frequent pains in the limbs, which were worse towards night, were evidently of a rheumatic character, and had followed his getting cold and wet in the feet. The perspirations at last were suddenly suppressed, and immediately after the patient experienced a feeling of weakness and loss of sensibility in the lower extremities. At first, these phenomena

were confined to the leg, but extended subsequently to the thigh and lower abdomen, and were accompanied by eneuresis and constipation. Totally unable to walk, he was able to move about on crutches, dragging the lower half of his body after him; but when seated, he could move his legs in any direction. Perspiration was limited to the upper half of his body, ending just below the umbilicus. The lower half was dry, and desquamated continually. Sexual power was lost. The most careful exploration of the spinal column was instituted, but nothing could be discovered to account for the paraplegia. Dr. Romberg then conceived, that the suppression of the habitual perspiration from the feet at least indicated the therapeia in the case, and he prescribed the foot-bath, with half an ounce of "aqua regia" (nitro-muriatic acid) thrice a week. In a fortnight, the gait became steadier, the patient was able to stand, the feet hitherto cold and dry, became warm and moist. From gr. $\frac{1}{3}$ to gr. $\frac{1}{2}$ of the spirituous extract of nux vomica was then administered, and in fourteen days the urinary bladder recovered its tone, and sensation returned to the lower extremities. After a few months' perseverance in the treatment, the patient recovered perfectly, except a slight tottering in his gait.

Another case, somewhat similar, was observed in the person of a drunkard. In this, the bladder and rectum were not paralysed, nor was sensation lost, and the patient could move his legs when seated, as in the preceding case. Upon inquiry it was found, that a profuse habitual perspiration from the feet disappeared immediately previously to the accession of the paralysis. Foot-baths, thrice a week, medicated with caustic potass instead of acid, and continued for a month, effected a cure without the use of any other remedies.

A new form of atrophy of the face. This case is interesting, as an example of hemiplegic *atrophy* only. A woman, aged 28, had, at the age of 13, an attack of tertian ague. Three years afterwards, she made a journey on foot through a very heavy rain, and very soon after was attacked by what was probably scarlet fever. In particular, the mucous membrane of the pharynx was inflamed, the voice was hoarse, deglutition painful, the uvula enlarged, and an abscess formed in the left tonsil. Externally, the cervical glands were swollen. The general health was shortly re-established, but it was found, that the right side of the face appeared full, plump, and blooming, as it should be in a female of eight and twenty, while the left side was shrivelled and wrinkled like that of an old woman. The median line of the face constituted a sharp and distinct boundary between youth on the one side of the face and age on the other. On the wrinkled side, the hair was scanty, and the forehead less convex; but from the want of muscle and fat, the superciliary arch on that side was more strongly marked than on the right. Further, the eyebrow was nearly all gone, the eyelashes scanty, the caruncula small and pale, the eyelids large and thin, as compared with the right. The left side of the nose presented a curiously atrophied appearance, as also the left side of the mouth and chin; and the morbid change could be traced down the same side of the neck to about the middle. Dr. Romberg gives nine comparative measurements of the two sides: those on the left were less than the corresponding on the right, in proportions varying from a fourth to a tenth. The atrophy also implicated the left tonsil, left side of the uvula, &c.

Now it is remarkable, that there was no loss of motion or sensation on the left side, and the secretion of tears, saliva, and perspiration, was not at all affected or interrupted. The only difference was, that the pulsation of the left carotid was weaker than that of the right, and was accompanied by a dull bruit not perceptible in the latter. Dr. Romberg quotes a somewhat similar case (also in a young female) from "a collection of unpublished writings," by the late Dr. Parry: in this example, the atrophy of the left side came on with a hemiplegia of the whole left half. Dr. Romberg also relates a case communicated to him by a physician at Dantzig, resembling that observed at Berlin, in all essential particulars. Motion, sensation, and cutaneous secretion were all undisturbed.

These cases are interesting problems for the neurologist, and are not capable, we think, of a very satisfactory solution. They appear to point at a class of nerves differing from the motor, sensory, and organic, or at least that portion of the organic ministering to secretion.

Cases of *anæmia* were numerous; these were presented principally in young dressmakers, or persons engaged in sedentary employments. The anæmic condition of the blood was not, however, always indicated by pallid lips and surface: in one example, a dressmaker, aged 24, the face was so florid, that the cerebral and cardiac symptoms, observed in anæmia, were at first pronounced to be dependent on disease of the heart, and the patient was treated accordingly, without any good result. On further inquiry it was found that she had been bled seventy-five times! Steel and chalybeate mineral waters were then administered with the best success. Dr. Romberg remarks, that the characteristic bellows-sound was never absent from one or other carotid in cases of this kind: but of twelve examples, in nine it was heard in the right carotid only. A thrilling could also be felt with the finger. Gastrodynia was a usual complication, and also Pica, but the morbid appetite generally preferred sour and salt matters; in two cases only was chalk or earth selected. The treatment adopted was a course of aromatic bitters and chalybeates in different forms; in obstinate cases, chalybeate baths were prescribed; two oz. of muriated tincture of iron being added to a suitable proportion of water.

Cases of *Albuminuria* are related. The first is an example of the acute form of the disease. A girl, aged 11 years, of a scrofulous constitution, was attacked with an acute pain in the loins, particularly on the right side, œdema of the lower extremities and of the face, particularly the eyelids, slight difficulty in passing urine, and nausea. The urine was clear, frothed much when shaken, and precipitated albumen on being tested by nitric acid and heat. No exanthem preceded the attack. The patient was treated by bloodletting from the loins, mercurial inunction, calomel gr. $\frac{1}{2}$, and digitalis $\frac{1}{6}$, night and morning, together with confinement to bed, and spare diet. In eight days she was quite well.

Iodide of iron in albuminuria. In another example, that of a dram-drinker, aged 45, with febrile symptoms, headache, vertigo, &c. and *œdema* of the legs to the knee, and of the upper parts of the neck, the syrup of the iodide of iron was given with success, gradually from one third of a grain of the salt to a grain.

In another example in a man, aged 48, similarly and as successfully treated, the following were the results of the analysis of the blood and urine:—

The Urine in Albuminuria.		The Blood in Albuminuria.	
Specific Gravity	1·007	Water	816·8300
Water	981·51	Fibrine	6·1529
Fixed residues	16·49	Fat from the fibrin	0·0071
Composed of		Fat from the serum	2·7800
Salts	3·02	Colouring matter	57·9629
Uric acid	0·17	Albumen	62·3900
Urea	1·52	Extractive	50·5700
Albumen	4·08	Salts	3·9300
Extractive matters	9·70		
	1000·		1000·6729

Condition of the kidneys in diabetes mellitus. Only one case of this disease came under treatment. The urine was analysed by Simon, and found to contain two ounces and three drachms of sugar to each quart. Iodide of iron, with a purely animal diet, was the treatment adopted, and at first with apparent benefit. Ultimately, however, the case terminated fatally. On an examination of the body, both the pleuræ were found to contain serum; the upper lobe of both lungs contained crude tubercles, and in that of the right, there was a circumscribed cavity of the size of a hen's egg. The liver was very voluminous and congested: the spleen wasted, flaccid, and internally of a brown colour, with soft macerated portions. Both kidneys were hypertrophied, firm, congested: the ureters normal: the bladder dilated, and its coats thickened. On more minute investigation by Dr. Remak, the cortical substance of the kidneys was found hypertrophied in consequence of thickening of the tubuli uriniferi, and especially congestion of the malpighian glands, which were thus rendered visible to the naked eye. The tubuli had an opaque appearance, dependent on a deposit in the outer coat of fat-like granules. Free fat, in the form of large globules, was also strewn over the inner surface of the tubuli, and on the mucous lining of the pelvis of the kidney. The cellular tissue connecting the tubuli and malpighian glands was indurated. No trace of sugar could be detected.

Dr. Romberg very justly argues, that the changes in the kidneys and bladder were secondary, caused by the stimulus of the saccharine urine.

Treatment of the sequelæ of scarlatina. In simple anasarca uncomplicated with fever, the antiphlogistic diuretics, and particularly "tartarus depuratus" (bitartrate of potass) were administered. If there were febrile symptoms, and especially those of bronchitis, a small general bleeding was ordered, with infusion of digitalis, and the bitartrate of potass. In those cases in which thoracic effusion took place, this method was actively followed, conjoined with the inunction of sublimate ointment, of the value of which Dr. Romberg speaks very favorably. A drachm of sublimate is to be mixed with an ounce of lard, and the size of a hazel-nut, or of half a teaspoonful, is to be rubbed on a circumscribed spot on the chest about the size of a crown piece, every hour until the skin becomes red. Shortly bullæ, like those of pemphigus, will appear, and when these have dried up, a new spot is to be selected, and treated in a similar manner.

Treatment of acute rheumatism. In recent cases, general bleeding was first practised; the painful limbs enveloped in wadding or tow, to keep them at an equable temperature, and from seven to ten minims of the tincture of colchicum seeds given every two hours, combined with two scruples of sulphate of magnesia. In mild cases, this mixture was sufficient without venesection.

The remedy found by Dr. Romberg to be most efficient in shortening the duration of an acute attack, or in very obstinate chronic cases, especially those in which one joint only was affected, was the bichloride of mercury. To adults, it was administered twice or thrice a day, in doses varying from one sixth to one third of a grain; to children, the doses varied from one twelfth to one eighth. Counter-irritants were applied in conjunction with the remedy. Rheumatic periostitis was a frequent disease, and was best treated with the iodide of potassium.

Gangrene of the mouth in children, occurred in the Berlin Policlinic, under the two forms of Stomacace and Noma. The former was much less malignant than the latter. It began with disease of the alveolar periosteum, then the gums were affected, and lastly the teeth dropped out. Noma commenced in the cheeks, with an ashy gray speck that gave little annoyance, and was at first overlooked. Medical aid indeed was only called in, when the disease began to appear on the cheek externally. This first began with swelling, hardness, and a peculiar fatty appearance, and then a dark gangrenous spot appeared, the preliminary to perforation of the cheek. The disease would then proceed with the most frightful ravages on the face, and not sparing even the bones. Sometimes gangrene would attack other parts, as, for example, the labia pudendi of female orphan children in the orphan houses. It was usually complicated with gastric disturbance and profuse diarrhœa, but seldom with typhus fever. It usually attacked cachectic children recovering from an infantile exanthematous fever, as measles, or scarlet fever.

In the treatment of these cases of stomacace or stomatitis, an emetic was first given if the patient was not too feeble, but generally a stimulating diet with cinchona was prescribed, and locally the application of sulphate of copper (five grains to half an ounce of honey of roses) or sulphate of zinc (ten grains to half an ounce), or the hydrochloric acid combined with honey, as an application, or diluted and used as a gargle. We have found the nitrate of silver a very trustworthy remedy in stomatitis. The child should take the eighth or sixth of a grain, with two drachms of distilled water, and two or three minims of dilute nitric acid every four hours.

Scirrhus of the œsophagus was observed in two cases, both men, between forty and fifty years of age. The leading symptoms were, violent pains in the throat, vomiting immediately after and even during eating, acidity, and rapid marasmus. In the one case, the scirrhus was situate just above the cardia; in the other, there had been no pain felt for some months previously to death, but a most tormenting cough supervened in its stead, accompanied by the expectoration of a dark offensive matter. On a post-mortem investigation, the upper third of the thoracic portion of the œsophagus was found affected with scirrhus. There was a fistulous opening from the diseased part to the upper lobe of the right lung, to which, and also to the vertebral column, it was strongly adherent. The fistulous opening ended in a cavity in the lung about the size of a walnut, containing a carcinomatous ichor, and the whole lung seemed infiltrated. Contrary to expectation, a second scirrhous degeneration was found in the cardia, which was united to a tumour the size of a walnut, situate on the under surface of the small lobe of the liver. There was also a small tumour of stony hardness on the pancreas.

This case is interesting, as proving, that in scirrhus of the œsophagus,

other parts of the body may be implicated in the disease, contrary to the opinion of Rokitanski.

In cases in which *Gastritis*, or ulceration of the stomach, was presumed to exist, an exclusively milk diet (the milch-kur) was adopted.

Diarrhœa of infants was treated with the nitrate of silver, as according to the following formula:

℞. Argent. Nitr. cryst. gr. ss. Solve in Aq. destill. q. s.
Dec. Rad. Salep. ℥ iij. Syr. Diacodii ℥ ss. M. D. S. A dessert or teaspoonful to be taken four times daily.

We never find that the German physicians write elegant prescriptions: syr. diacodii might as well we think have been left out, and the dose of the nitrate is unnecessarily small. We have already given the formula which we prefer. We can, however, corroborate the statements of Dr. Romberg, as to the efficacy of the remedy in diarrhœa, and, we would add, in the gastric irritation that sometimes accompanies dentition.

Cases of *Chronic Peritonitis* in children, consequent on tuberculous disease of the mesentery, were common. Dr. Romberg speaks highly of the following combination (invented by Heim) in incipient tabes mesenterica:

℞. Potassæ Acetatis ʒ j ad ʒ ij; Extr. Conii macul. gr. iv.
Aquæ communis ℥ iij; Syr. Papaveris ℥ j. M. A small or child's spoonful to be taken four times a day.

With this were conjoined topical bleeding, and inunction with ointment compounded of the mercurial and hydriodate of potass ointments. In atrophia lactantium, and in marasmus from chronic diarrhœa, Dr. Romberg prescribed Tokay wine with the best effects. Infants took from five to ten drops thrice daily, and children from twenty to twenty-five drops. Other dietetic means of a strengthening kind were not however omitted.

Only five cases of *Croup* were treated. In one of these a membrane was found occupying the posterior surface of the right bronchus. The inner layer of that membrane, or, in other words, that portion resting against the mucous membrane, was ascertained to be made up of amorphous cystoblast cells, with somewhat round dark granules, in which two or three nuclei might be further observed. The membrane inclosing the granule was thin and dissolved readily in acetic acid. The outer and first-formed layer of the false membrane consisted of perfectly formed fibres, in which here and there a cell-granule could still be seen. Between the fibres, there were cells prolonged into a point, and united point to point. In the whole of the exuded matter, epithelium-cells were freely scattered about. Dr. Remak examined a false membrane expectorated during a fit of coughing, and found it to consist of finely organized fibrin, soluble in acetic acid, and infiltrated with mucous corpuscles.

Amongst the *diseases of the respiratory organs*, there were observed examples of *laryngismus stridulus*, from compression of the recurrent nerves by enlarged bronchial glands. *Pleuritis* was common, and was not always readily distinguished from a rheumatic affection of the thoracic muscles. *Empyema* was treated successfully with digitalis and supertartrate of potass, in combination; externally the affected side was pencilled over with tincture of iodine, the application of it being repeated so soon as desquamation took place. In some cases, the inunction of hydriodate of potass ointment was ordered.

In simple *hypertrophy of the heart* in young subjects, a seton in the region of the heart was found to be of great service. Dr. Romberg, however, cautions his readers against expecting too much from it at first; a persistence in its use for months, or even years, is necessary, especially in cardiac disease of rheumatic origin. We have occasionally prescribed this counter-irritant, and we believe with benefit, but in the first instance it has appeared to us to aggravate all the symptoms.

In *Psoriasis inveterata,* Dr. Romberg found the aqua picea, or aqua picis liquidæ, to effect a cure when all other means failed. The aqua picea was prepared by pouring a quart of cold water over a pound of pitch, and leaving it to stand for twenty-four hours in a cool place, and a "beer-glass" (about four ounces?) of the water filtered through paper, is to be taken every morning fasting, and the parts affected to be bathed with it twice or three times a day. Its use may be continued for months, the only apparent effect resulting, being slight diuresis. Six cases are related in proof of the great efficacy of this remedy in obstinate psoriasis. It is an old remedy in England, and has been found useful in ichthyosis. Its external application in the form of ointment is also well known. The facts stated by Dr. Romberg are however useful in reminding us amidst our desire for new remedies, that there is so safe and simple a remedy as this.

A very remarkable case of *Chronic Pemphigus* was treated at the Berlin dispensary. Dr. Simon had already published an analysis of the contents of the vesicles, or bullæ, describing it as pemphigus hystericum, the patient being a highly hysterical female. The attacks recurred at intervals during three years, and were doubtless periodic: this point, however, has not received Dr. Romberg's attention. The last recorded attack occurred in January, 1840, and commenced with febrile symptoms, and a sensation as if hot water was being poured over the legs. In four and twenty hours the burning sensation extended to the surface of the body: on the next day, the epigastrium was covered with red spots, and in twelve hours afterwards, bullæ appeared on the chest, generally of an oval form, and varying in size from that of a pea to a hen's egg, and even larger. One bulla, formed by the union of several minor ones, contained at least four ounces of fluid. The bullæ were developed from day to day on the body; on the face, within the month, and even in the vagina, old bullæ drying up, and fresh ones appearing. On the seventh day from the first eruption, when it was thought the disease had ceased, suddenly a new crop broke out on the face. On the tenth day, these began to dry up, but suddenly again on the fourteenth day, another eruption took place, but was principally confined to the mouth. When desquamation took place, the hair and eyebrows fell off. The analysis of the fluid in this case is given in 'Simon's Animal Chemistry,' Sydenham Society's edition, vol. ii. Tar-water appeared to be the remedy that gave the patient the most relief.

The volume concludes with a detailed history of a case of *Elephantiasis,* or *Lepra Arabum tuberculosa,* affecting the whole surface of the body, except the hands and feet. A coloured lithograph of the face of the patient is given. The disease terminated fatally with general anasarca, and effusion into the serous cavities. A dog and a horse were inoculated with blood from the patient, and with the glairy secretion from the tubercles on the skin, but without any result.

Art. IV.

1. *Researches into the Physical History of Mankind.* By James Cowles Prichard, M.D., F.R.S., M.R.I.A., Corresponding Member of the National Institute of France, &c. &c. Third edition.—*London,* 1836-47. Five volumes, 8vo, pp. 2547. With numerous Plates.
2. *The Natural History of Man; comprising Inquiries into the Modifying Influence of Physical and Moral Agencies on the different Tribes of the Human Family.* By James Cowles Prichard, M.D., F.R.S., M.R.I.A., Corresponding Member of the National Institute of France, &c. &c.—*London,* 1843. 8vo, pp. 556. With forty Plates and ninety Wood-Engravings.
3. *Appendix to the First Edition of the Natural History of Man.*—*London,* 1845. 8vo, pp. 40. With six coloured Plates.

If a person, whose knowledge of Mankind is restricted to the individuals of his own nation, and who is consequently unaware of the diversities in complexion, conformation, and habits, which exist among the several races of men, could suddenly be made a spectator of the various conditions which they assume in different parts of the earth, we cannot doubt that his curiosity would be strongly excited, as to the degree of relationship to himself which these races possess.

"If such a person," as Dr. Prichard has well remarked, "after surveying some brilliant ceremony or court-pageant in one of the splendid cities of Europe, were suddenly carried into a hamlet in Negroland, at the hour when the sable tribes recreate themselves with dancing and barbarous music;—or if he were transported into the saline plains, over which bald and tawny Mongolians roam, differing but little in hue from the yellow soil of their steppes, brightened by the saffron flowers of the iris or the tulip;—or if he were placed near the solitary dens of the Bushmen, where the lean and hungry savage crouches in silence, like a beast of prey, watching with fixed eyes the birds which enter his pit-fall, or the insects and reptiles which chance may bring within his grasp;—or if he were carried into the midst of an Australian forest, where the squalid companions of kangaroos may be seen crawling in procession, in imitation of quadrupeds;—would the spectator of such phenomena imagine the different groups which he had surveyed to be the offspring of one family? and if he were led to adopt that opinion, how would he attempt to account for the striking diversities in their aspect and manner of existence?"

This question is capable of being considered under a great variety of aspects. There are many very excellent persons, who think it quite sufficiently answered by the authority of the scriptural narrative; and if we make up our minds to receive with implicit confidence the declarations of the Bible in regard to the common origin of all the races of mankind, the question might *seem* to be decided without the necessity of entering upon any further inquiry. But it is not so in reality. There is a large class of persons at the present time, especially on the other side of the Atlantic, who profess the highest veneration for the Scriptures, and who would look with horror at any attempts to impugn their authority on any subjects regarding which their own prejudices and supposed interests lie in conformity with their teachings: and yet these very persons, to serve their own purposes, get rid of the historical testimony and the various declarations contained

in the Bible, in favour of the Unity of Origin of the Human Races,—after the following fashion. They maintain that the *Adamic* race does not include the uncivilized inhabitants of remote regions; and that Negroes, for example, Hottentots, Esquimaux, and Australians, are not in fact *men* in the full sense of the term, or beings endowed with mental faculties similar to our own. By writers of this school it is contended that these and other barbarous tribes are inferior in their original endowments to the proper human family which supplied Europe and Asia with inhabitants; and that, being organically different, they are separated by an "impassable barrier" from the race which displays in the highest degree all the attributes of humanity, and can never be raised to an equality with it. They maintain that the ultimate lot of the ruder tribes is a state of perpetual servitude; and that if, in some instances, they should continue to repel the attempts of the civilized nations to subdue them, they will at length be rooted out and exterminated from every country on the shores of which Europeans shall have set their feet. If the distinct origin of these tribes be admitted,—if we are to regard the Negro and the Australian, not as our fellow-men, brethren of the same great family with ourselves, but as beings of an inferior order,—and if duties towards them were not contemplated, as we may in that case presume them not to have been, in any of the positive commands on which the morality of the christian world is founded, our relations to those tribes will appear not to be very different from those which we might consider ourselves to hold towards the higher races of brutes. We can scarcely imagine a Grotius, or a Puffendorf, or any other great jurist, attempting to determine the *jus belli* or *pacis* between ourselves and a tribe of Orangs, who had just sense enough to pass for men and began to be suspected of the cheat;—which is nearly the true character of the Negroes, if those are right who maintain the doctrines to which we have alluded. And we may go a step farther and assert, that there is in such a case no moral principle which should prevent a hungry wanderer in Negroland or Australia from satisfying his appetite by killing and eating the first native he might happen to meet.

Thus, then, we see that the widest extremes of opinion, and the greatest diversities in those rules of conduct which are founded upon those opinions, may exist among those who profess the most implicit reverence for the Scriptural *dictum*, that "God hath made of one blood all nations of men, for to dwell on all the face of the earth;" for whilst some include under the term *men* all the individuals grouped together by the naturalist under the genus Homo, others assert that this genus includes several species, which form a gradation between the highest and most cultivated races of mankind, and those degraded tribes which seem to have more in common with the brutes; the former alone being really entitled to the appellation *Men*, whilst the latter should be designated by some other name indicative of their close affinity to Chimpanzees and Orangs.

But the question may be discussed without any reference to the supposed authority of the Scriptures on this head; and this, not merely by those who reject revelation altogether, but by those who consider the Bible as imparting truths of its own relating to the *moral* not the *physical* history of mankind, for which they entertain the highest veneration; and as not intended to enlighten us upon the present question, any more than upon astronomy or geology. The origin of mankind from a single stock,

or from a diversity of stocks, thus becomes a matter of purely scientific investigation; and as such we shall treat it,—not with an entire indifference as to its issue, for we consider it a matter of no trifling importance to arrive at a correct result,—but, we venture to affirm, with an earnest desire to arrive at the *truth*, wherever this may be to be found.

The question may be thus stated. In the *first* place, all the existing races of mankind may be supposed to be the offspring of *one pair;* and it becomes necessary then to account for the diversities which they now present, in physical conformation, in language, and in mental character and social condition. *Secondly*, the existing races may be supposed to have descended from *several distinct pairs*, which originally presented differences amongst themselves, nearly the same with those which now exist amongst the races that seem most remotely related to each other. The first of these suppositions requires that evidence should be given of a very considerable amount of *variability* from the original type—whatever that may have been—amongst the descendants from the common ancestry. The second is based upon the idea that the leading characters which now separate the different races are *permanent*, and must have been presented by their original progenitors. A *third* supposition is perhaps admissible; namely, that the existing races did not all proceed from one stock, but from several; but that these, although scattered over the globe, differed only in the adaptation of certain of their physical characters to the different circumstances of their several abodes, being all possessed of the same essential nature, both mental and corporeal, and all having a certain capacity for variation, so that the prolonged influence of climate, civilization, &c., might in a great degree obliterate the original differences. The moral relations between the several races would, on this last supposition, be as close as on the first, although the same physical affinity or blood-relationship would not exist. We cannot but agree with a recent reviewer of Dr. Prichard's work, in the remark that "the moral rights of men depend on their moral nature; while Africans have the hearts and consciences of human beings, it could never be right to treat them as domestic cattle or as wild fowl, if it were ever so abundantly demonstrated that their race was but an improved species of ape, and ours a degenerate kind of god."* It is in adopting the second supposition that those find their ground of defence, who assert that they are justified in treating as brute beasts certain races which have the misfortune to be dark in complexion and not to come up to their standard of beauty in physical conformation. If the characters of each race *are* permanent, and their respective boundaries fixed by an impassable barrier, the more elevated have a show of justice in claiming for themselves alone the designation of men, and in asserting their right to keep in subjection those degraded tribes which have no sufficient title to the name.

The question of the *permanence* or the *variability* of the races of men is therefore one of fundamental importance; since, if the former be established, not only is a strong argument afforded for original diversity of parentage, but (what is of far more importance in our apprehension) we are almost forbidden to hope that the races which we at present regard as inferior, can be in any way raised to our own level; whilst if the latter

* New Quarterly Review, No. xv, p. 131.

can be proved to be the case to a sufficient extent, the greatest difficulty in the way of the idea of the community of origin of mankind is at once removed, and we are encouraged in the belief that there is no race, however degraded at present, that may not be ultimately raised to the highest elevation of which humanity is capable. In the discussion of this question, a vast number of different topics are involved. The number of cases in which positive historical testimony can be brought to bear upon the question of the identity of origin of two or more races at present exhibiting characters of a very diverse nature, are comparatively few; and appeal is necessary to Philology, a branch of science with which Physiology would seem to have no direct relation, in order to obtain the requisite information. The inquiry into the nature and affinities of the several languages at present or in past times spoken in different parts of the globe, has been carried on of late years with great earnestness, especially by the *literati* of Germany; and notwithstanding discordances on minor points, the leading philologists of the day are fully agreed upon certain general principles, which we think it well here to quote, for the sake of exhibiting to our readers the *nature* of one of the sources of information from which Dr. Prichard draws a large proportion of the materials of his argument.

"On examining the relations of language which are said to display marks of resemblance or conformity, two very different series of phenomena are discovered, which lead to very different results. Languages of neighbouring nations, or of nations long and intimately connected by local proximity, by traffic and commercial intercourse, or by political bonds, exhibit marks of such connexion in their vocabulary, or in the possession of a great many words in common. Of this description is the extensive resemblance in words between the French and English languages.* In the languages of nations who may have come into a similar nearness of intercourse while in different degrees of social culture, when the one people possessed many arts and the knowledge of very many objects, of which the other were wholly destitute, it is evident that a much more extensive resemblance would take place than that which is discovered between the French and English. But this species of resemblance or partial identity in the vocabulary could never approach to what is termed a family relation of languages; that is, such a kind of connexion between them as affords proof of a common origin in the nations to which they belong, as in the instance of the English language compared with the German. The first and most important feature indicative of family relation between languages, is analogy in grammatical structure, and in the laws of combination, or, as we may so term it, the mechanism of speech.—Languages supposed to have been originally cognate have, in some instances, lost every other token of relationship except this. It generally happens, however, that grammatical affinity between languages is accompanied by a near resemblance in a certain part of the vocabulary. Occasionally this extends only to a comparatively small number of words; but they are words of a particular class, namely, such as serve to represent the ideas of a people in the most simple state of existence. Such are the terms expressive of family relations,—father, mother, brother, sister, daughter; names for the most striking objects of the visible universe; terms distinguishing different parts of the body, as head, feet, eyes, hands; nouns of number, up to 5, 10, or 20; verbs descriptive of the most common sensations and bodily acts, such as seeing, hearing, eating, drinking, sleeping. As no nation was ever found destitute of similar expressions, and as we know by the observation of facts even more than by the probability of the case, that tribes, however

* I have selected this example as the most familiar instance. It is liable to the objection that the French and English do not belong to originally distinct families of languages. The Anglo-Saxon and the Norman French were, however, so different, that in a practical point of view this instance answers my purpose as well as any other.

rude, do not exchange their own stock of primitive words for those of a foreign idiom, it may be inferred that dialects which correspond in these parts of their vocabulary were originally one speech, or the language of one people." (Natural History of Man, p. 182.)

We have in the case of the languages of the several races of which the great mass of the population of Europe is composed, an example of both these kinds of conformity; namely, in grammatical construction, and in what have been termed primary words. And this conformity not only links them to each other, but connects them with the more ancient languages of central Asia, the Sanscrit or original language of the Hindoos, and the Zend or earliest idiom of the Medes, Persians, and Bactrians; and these two are so intimately related to each other as to leave no room for doubt as to their common origin. On the other hand, the languages of America are entirely different from those of the Old World in both particulars. Their grammatical structure is distinguished by a very striking feature; namely, the peculiar manner of forming compound words, which has been called "agglutination." This is very different from the ordinary composition of words in the languages with which we are most familiar; the process being to make up new compounds from a number of *small fragments* of simple words, and then to treat these compounds as if they were simple vocables, mutilating or contracting them to form other aggregate words. Thus when a Delaware woman is caressing or playing with a little dog or cat, or some other young animal, she will often say to it *kuligatschis*, meaning, "give me your pretty little paw." The word is thus compounded:—*k* is the inseparable pronoun of the second person, representing *ki*, and meaning either "thou" or "thy;"—*uli* is part of the word *wulit*, meaning "handsome" or "pretty;" *gat* is a part of the word *wichgat*, meaning "leg" or "paw";—and *schis* is a diminutive termination. This peculiarity, especially consisting in the introduction of very small portions of words into the compound term, and in the large capacity of the latter, distinguishes them from all known languages of the Old Continent, except perhaps the Euskarian; and even in this the agglutinating process is rarely carried to so great an extent as it habitually is in the American idioms. "The only Greek compound words," says Dr. Prichard, "which are comparable with those of the Lenape, are the long fantastical epithets used in burlesque by Aristophanes in some of his comedies. If we could imagine the main stock of words belonging to a language to be made up of such materials, and adapted not to poetry either serious or ludicrous, but to the purposes of ordinary conversation, we should perhaps have an idea of the nature of the words which furnish names for objects in the American idioms. The following is another striking feature in the physiognomy, so to speak, of the American language.

"These languages, like the idioms of perhaps all nations in a similar state of society, may be said to be rather more subjective than objective. The prevailing impulse in a rude people of hunters and warriors is, not to discriminate the qualities of external objects, but to give vent to the internal feelings, passions, and desires of their own minds; their egoism, or personal volition and action, is ever uppermost, and the predominant movement of their mental life. Circumstances and externals are secondary, and draw but little of their attention. Hence verbs or words expressive of emotion, will, and action, are the principal words in these languages, and are developed in the greatest variety of forms. There is a constant tendency to involve in the expression of the verb, and to denote in one

word as much as possible, the circumstances and conditions of the agent, and the external relations of the act he has performed, or is perhaps about to perform. The alterations in the forms of the verbs thus induced go further than the changes of mood, tense, and conjugation, which are common in different degrees to most languages. They are contrived to express, without the use of distinct words, as many varieties and shades of meaning as possible. Even physical objects, especially where pronouns are used, are pointed out without being expressed, through some slight alteration in the forms of verbs." (Physical History of Mankind, vol. v, p. 312.)

Some very extraordinary compounds are thus elaborated to express abstract ideas, which in other languages are designated by simple terms. Thus *wulamœwagan*, which means "the truth," is derived from the verb *wulamœ*, "he has spoken the truth" and the formative *wagan*, which resembles the English *ness;* and *wulamœ* itself is compounded of three words, a pronoun, a portion of the term meaning "good words," and another which implies the act of speaking. By the insertion of a portion of the verb *glistam*, which means to "hear" or "listen," we get the compound *wulistamœwagan*, which means belief in what is said by another.—These examples will suffice, we think, to convey to our readers some idea of the peculiar character of the American languages, so far as regards their construction. This feature serves not merely to distinguish them from the languages of the Old World, to such a degree as to show that the two families of nations must have been very early separated; but it also serves to connect the different American tribes, from Greenland to Cape Horn, by a common bond of relationship, in spite of a very considerable diversity in their primary words and in the roots of their vocabulary. This diversity, —extending in some cases, it has been thought, to total dissimilarity,— might appear to forbid that idea of a common origin which has been founded on similarity of grammatical structure. But as Dr. Prichard has well pointed out, there are peculiarities in the very nature of the American languages, which are likely to produce great varieties in words, and to obliterate in a comparatively short period the traces of verbal resemblance. We quote his remarks on this subject nearly in full, because we wish to point out to our readers the vast number of the considerations that enter into the inquiry on which we are engaged; and because we shall not again revert to the philological principles which are especially involved in it.

"1. The great length of words in the American languages is unfavorable to the preservation of resemblance between vocabularies of separated tribes..... It is impossible that the memory can be so tenacious of long polysyllabic words, as of shorter ones; and if the habit prevails of truncating words at their beginnings and endings without rule or limit, it is very obvious that any resemblance that existed between them originally may soon be lost. In the languages of Asia and Europe, roots are either monosyllables, or at most dissyllables; and these are not so much lengthened out by additional syllables as the American languages. Hence they are better retained in memory. It is in fact principally in words of not more than two syllables, that those resemblances exist, which are so striking when the different members of the same family, as of the Indo-European or the Syro-Arabian group of languages, are compared.

"2. It is easy to perceive that the agglutinative system of formation, which is the principle of structure belonging to the American languages, must tend effectually to destroy resemblance. We have seen that it is the custom in conversing in the Delaware language to aggregate some parts merely of as many words as may occur to the mind of the speaker, in order to construct for temporary

use a compound word that shall comprehend all the circumstances of any action. .....It must soon happen among people who have the habit of thus expressing themselves, that only those persons who live near to each other and have some acquaintance and frequent intercourse, can readily make themselves mutually intelligible. They must apparently know something of each other's thoughts and habits of mind before they can understand such casual and arbitrary enunciations of meaning, in which words newly coined for the occasion are of perpetual occurrence.....

"3. Another cause which has been observed to render obscure and difficult of detection, the original connexions between American languages, and which must always have a tendency to increase or create differences in the vocabulary of idioms really cognate in their origin, is the imaginative and rhetorical disposition of the native people of the New World. In a barbarous state of society, and principally in one of early and imperfect but growing refinement of mind, the imagination has more influence in the formation of language than in a more advanced stage. When scientific accuracy and precision of thought and expression are required, and it becomes the habit of men to aim at such acquirements, the exercise of the imagination is restrained within very narrow limits. Observation and discrimination are employed. But figures and metaphors abound in the discourse of simple people; and hence the eloquence for which the American nations are celebrated. As a matter of fact it is the habit of the American tribes to substitute for words epithets which become conventionally established as ordinary terms. Dr. Scouler has observed that in the languages of the north-west coast in particular, the names even of simple and familiar objects, such as sun, moon, day, night, &c., are not simple nouns or names, but not unfrequently compound words or epithets. In this case he suggests, unless we possess an intimate knowledge of the influences of the verbs, and the nature of the indeclinable particles, we might mistake two idioms nearly allied for primarily separate languages." (Physical History, vol. v, p. 320.)

Thus we are informed by Dr. Scouler that among the tribes of the north-west coast, the names given to articles introduced among them by European traders, are not, as is common among rude nations, derived from the foreign designations of the same objects; but they are descriptive epithets, formed in their own languages, and consequently different in the idiom of each particular tribe. Thus, the name of a gimlet among the Chemesyans is a compound formed from the noun which means "a hole" or "aperture," and a verb signifying "to make." The compound name means "a hole-maker."

It is evident throughout his work, that Dr. Prichard sets a primary value upon fundamental affinities of language as indicative of family relationship amongst groups of nations; and that he assigns to it a vast superiority over the indications afforded by physical characters. The impossibility of drawing any correct inferences from the latter alone, will become apparent, we think, when our readers shall have followed us through our review of Dr. Prichard's investigations in regard to their variability. But we have looked in vain through his work for any corresponding general examination of the kind of evidence derivable from language; and his readers are obliged to take for granted the philological principles on which he has conducted the details of his inquiry. Now as very few of them are sufficiently versed in the science of glottology, to be able to judge for themselves with regard to the value of these principles, without some further elucidation of them than has been afforded in either of the works before us, we feel that the cogency of Dr. Prichard's arguments is considerably diminished by the numerous exceptional cases, in which

from conquest, migration, and other causes, an *entire* change in the language of a nation has taken place, without any considerable intermixture with another race. Thus to take a very familiar case, that of the Jews;—their dispersion through almost every country in the world has caused each individual to learn and to speak familiarly the language of the nation in the midst of which he has been brought up; yet their national descent has been preserved more purely than that of any race not isolated by local separation. The preservation of their original language is an accidental circumstance, arising out of their religious peculiarities; and it is easy to foresee that, if the Jewish nation were converted to Christianity, the Hebrew tongue might soon be reckoned among the languages now extinct and only known to the learned. If we were to rely solely upon the existing languages of Britain, France, and Spain, and had no guidance from history with regard to the mode in which they have been formed, we could scarcely avoid falling into errors of the greatest importance as to the original stocks of the nations at present inhabiting those countries. Consequently, in cases where we are totally destitute of any trustworthy history, and have either vague tradition alone to direct us, or have no historical data whatever, it appears to us that great caution must be exercised in drawing inferences from analogies or diversities in the languages of different nations with respect to their degree of family relationship. Doubtless there are collateral circumstances, which may afford us great assistance in the inquiry; and which may give to inferences founded upon fundamental community of language, between certain groups of nations, a degree of weight which they do not elsewhere possess. And we believe that we shall be able to show that this is especially the case in some of the instances in which there is a wide diversity in physical characters; so that, when the variability of the latter within certain limits is admitted,—as we conceive it *must* be, if the question be candidly and philosophically investigated,—the *general* superiority of the glottological over the physical test must be freely conceded, even although the results of the application of the former may be doubtful in particular cases. The relative character of the two may, as it appears to us, be stated somewhat as follows.

We shall suppose a family or group of nations to spread from a common centre; carrying with them the language, habits of thought and expression, traditions, religious observances, &c., derived from their common parentage; and dispersing themselves through countries that differ considerably in climate and in those other conditions which affect the social existence of the human race. Now all experience shows that amongst nations thus separated considerable diversities soon present themselves, and that language begins to undergo modification sooner than anything else, unless it have been previously fixed by a national literature. Dialects soon spring up, distinguished by a different method of pronouncing the same words, which subsequently gives rise to a fixed diversity in the mode of spelling them; and new words are created to express new requirements, which words will obviously be peculiar to the nation or tribe amongst which they have originated. As some of these diverging tribes, however, come into contact with others which have spread from a different centre, and intercourse is established between them, their language receives an admixture of elements altogether foreign; and thus a still

greater diversity is created amongst the several dialects. By a long continuance of such changes this diversity may become so great, as to cause the languages of nations which have diverged from a common centre and have really had the same ancestral origin, to appear to a superficial observer to have nothing in common. But the experienced philologist will look for similarity of *roots* amidst diversity of *inflexions;* for correspondence in "primary words," amidst dissimilarity of terms expressive of less necessary ideas; and above all for a fundamental accordance in the grammatical construction or plan of mechanism of the language, in the midst of great variety in the mode in which the details of the plan are worked out. Where no greater change has been effected in any group of nations, than that which results from contact and intercourse with others, we believe that clear evidence of their original relationship may be derived from these points of accordance. The evidence is of course less clear in proportion to the deficiency of *verbal* coincidence; but it seems to lose but little of its force, where, as in the case of the American languages, the peculiarity of grammatical construction is marked with extraordinary force, and where (as is pointed out in the quotation which we have made from Dr. Prichard's remarks upon them) the very nature of this construction and of the genius of the language is such as to produce a speedy verbal discordance.

On the other hand, although the physical characters of the several nations thus diverging from a common centre, may not for centuries present any decided change, we think that we shall be able to draw from Dr. Prichard's researches, and from other sources, clear evidence that the lapse of centuries *may* alter them in a degree so considerable, as altogether to break down any distinction that may have once existed between their common original and the original of some other group of nations. Of course, the greater the change of climate and other external conditions encountered by particular tribes during their divergence from each other, the greater would be the modifications we should be entitled to expect, if these modifications are really induced by external causes: and such, we think, will prove to be the case; some nations presenting, as nearly as can be ascertained, the physical characters of the original stock; whilst others have widely departed from it. Hence, if the tendency of such variations be to bring the different races of men that cover the globe into conformity with its varying physical conditions, we should expect to find that the bodily characters of the different races have a much closer relation to the circumstances in which they now live, than they have to the characters of their supposed parents. There is an ample accumulation of evidence to this effect; with regard at least to some of those characters on which the greatest stress has been laid as means of determining the affinities of races,—namely, the colour of the skin, and the form of the cranium.

We have here taken for granted, however, the results of those physiological inquiries to which a considerable portion of Dr. Prichard's treatises is devoted; but as we are particularly desirous that our readers should be well informed upon this department of the subject, we shall now proceed to examine into the evidence which has led us to assert with so much confidence, that the distinctive characters of the human races are *not* fixed and invariable, but are liable to undergo modification through the agency of various external conditions, as well as to become altered from time to

time by variations, which, in the absence of any ostensible modifying influence, are considered as *spontaneous*.

In following out any inquiry of this nature, it is desirable to commence with a comprehensive survey of the phenomena which have relation to it amongst other tribes of animals. We do not say that it *might* not be satisfactorily pursued with reference to the human races alone; but there will be more probability of a decisive result, when that result is not capable of being endangered by arguments derived from collateral sources; and we consider the analogical investigation into which Dr. Prichard has entered, with such completeness as nearly to exhaust the subject, as one of the most valuable portions of his treatise, although it might at first sight appear too digressive from its main purpose. We have more than once had occasion to point out, that amongst the different species of plants and animals, there is a very wide diversity in regard to their respective *capacities for variation;* the several individuals among some being all very closely conformable to one type; whilst in others, individuals that are known to have the same parentage exhibit differences in external characters, and even in internal structure, which would have been regarded, if they had occurred amongst races known to have less capacity for variation, as unequivocally indicative of diversity of origin. In the species which have least capacity for variation, we find (as a necessary consequence) the *least* adaptiveness to external conditions; they are limited, therefore, in their distribution to the parts of the globe which supply those conditions, any decided change of these being fatal to them. On the other hand, in those which have *most* capacity for variation, that capacity manifests itself in the peculiar adaptation which their physical constitution undergoes to circumstances as they change; and also in the spontaneous origination of peculiarities that cannot be traced to the influence of those circumstances. Thus, when animals that are naturally inhabitants of cold or temperate climates, and have a thick protective covering of wool, exchange this for a thin covering of crisp hair, when they have been transported to a tropical region, we at once see that the variation is influenced by the external change of temperature. On the other hand, when a new race arises with a number of fingers, toes, or vertebræ, different from that of its parent, we must set down the occurrence as another manifestation of the same capacity for variation, only that we cannot here trace its connexion with any external influences. It will generally, if not always be found, that the races which show the greatest disposition to be affected by external changes, are precisely those that show most tendency to that divergence from a common type, which must (for the present at least) be regarded as spontaneous. Such is the case, for example, with our domesticated animals; which, like plants improved by cultivation, derive their capability in usefulness to man, in part from the degree in which they may be modified by external influences, and in part from their disposition to pass into spontaneous variations, of which those that seem likely to become useful to him are speedily perpetuated by his interference.

Of the greater number of our domesticated races, the origin is lost in antiquity so remote, as to prevent the possibility of our obtaining direct historical testimony as to their ancestry. There are few cases in which we can point to any existing *really wild* race, that can with any certainty be considered as the representative of the parental type, still

uninfluenced by domestication. We know not what has become of such;—whether they have been entirely subjugated by man, so that we at present only know the species in their altered forms, or whether the wild representatives of the original species are really so different from their reclaimed descendants, as to be unhesitatingly separated from them by the naturalist, although really possessing a common ancestry. Thus it is still an undecided question, whether the primeval dog was anything else than a wolf; the wolf being the original wild form, and the various breeds of dogs the more or less modified types of the same species. There are indeed *wild* oxen, sheep, goats, and horses; but most, if not all of these, are tribes which appear to have returned in some degree to their original state after having been more or less completely domesticated; and we are ignorant of the time and circumstances under which most of these races became wild, and of the particular breeds from which they have descended.

Still the phenomena which have taken place within the historic period, and of which there is indisputable proof, afford ample evidence of the wide range of variation, through which certain species of animals are capable of passing, in the course of even a few generations; and show that although the tendency to spontaneous variation may seem to have nearly exausted itself heretofore, in the production of the most divergent forms, there still remains enough to originate new races, distinguished by well-marked peculiarities of conformation, even under our own eyes. Some of our most valuable information upon this subject is derived from the changes which have taken place in the races of domesticated animals, introduced into the West Indies and South America, by the Spaniards, three centuries since. Many of these races have multiplied extremely on a soil and in a climate congenial to their nature; and several of them have run wild in the vast forests of America, and have lost all the most obvious appearances of domestication. The wild tribes are found to differ physically from the domesticated breeds, from which they are known to have originated; and there is good reason to regard this change as a partial restoration of the primitive characteristics of the wild stocks, from which the tame animals originally descended. The comparison of these wild races with our domesticated breeds affords much information which is important to our present argument; and we shall now proceed to give a summary of the facts, which are drawn by Dr. Prichard from the valuable memoir of M. Roulin, whose researches relate to New Grenada and Venezuela, and from the well-known and justly-esteemed work of Don Felix de Azara on the Natural History of Paraguay.

We commence with the hog; which was introduced into St. Domingo by Columbus on his first discovery of that island in 1493, and which was speedily carried into the continent of South America; multiplying with extraordinary rapidity in both situations.

"These animals, wandering at large in the vast forests of the New World, and feeding on wild fruits, have resumed the manner of existence which belonged to the original stock; and their appearance nearly resembles that of the wild boar. Their ears have become erect; their heads are larger, and the foreheads vaulted at the upper part; their colour has lost the variety found in the domestic breeds, the wild hogs of the American forests being uniformly black. The hog which inhabits the high mountains of Paramos, bears a striking resemblance to the wild

boar of France. His skin is covered with thick fur, often somewhat crisp, beneath which is found in some individuals a species of wool. From excessive cold, and defect of nourishment, the hog of that region is of small and stunted figure. In some warm parts of America, the swine are not uniformly black, as above described; but red, like the young pecari. At Melgara and other places, there are some which are not entirely black, but have a white band under the belly reaching up to the back; they are termed *cinchados*." (Natural History of Man, p. 30.)

Now these few facts might alone serve as the groundwork of a very important argument. It was long ago pointed out by Blumenbach, that the difference between the cranium of the wild boar and that of our ordinary swine, is at least equal to that which exists between the cranium of the Negro and the European; yet we see that the change from one form to the other may take place in the course of a few generations. The difference between the thin and sparse bristles of the ordinary swine, and the thick fur with subjacent wool, with which the hogs of the mountains of Paramos are covered, is at least equal to that which exists between these tegumentary appendages in any races of men. And the restoration of an uniform black colour throughout the inhabitants of extensive districts, is an equally significant proof of the influence of external circumstances in modifying this character. It has also been pointed out by Blumenbach, that the varieties of swine in other countries, all referable to a common stock, exceed in divergence the widest departures from any one type of human conformation. Thus swine with solid hoofs were known to the ancients, and large breeds of them are found in Hungary and Sweden, as also in some parts of England. In like manner, the European swine, first carried by the Spaniards in 1509 to the island of Cubagua, have degenerated into a monstrous race, the toes of which are half a span in length; and in some other breeds the hoof is divided into five portions.

Horned cattle were introduced into St. Domingo in the second voyage of Columbus; and thence into the Spanish settlements on the mainland, whence they have spread over the South American continent. The herds of wild cattle, which have descended from these, are distinguished, like the hogs, by their uniformity of colour; the upper parts being of a brown red, and the rest of the body black. That climate alone does not produce this effect, is evident from the fact that the herds of domesticated cattle in the same country exhibit the usual variety of hue.

"In some of the hot provinces of South America, M. Roulin informs us that a variety of ox has been noted for an extremely rare and fine fur. These oxen are termed 'pelones.' The variety is reproduced or descends in the stock; but is not cultivated, because the pelones are too delicate in constitution to bear the cold of the Cordillera, to which the cattle are driven for the provision of the towns there situated. The pelones evidently constitute a variety adapted to a particular climate. Oxen of other breeds often perish when driven into the same provinces, and are with difficulty assimilated to the climate, or *acclimatised*. In the same hot countries, a variety is sometimes produced with an entirely naked skin, like that of the dogs without hair found at Calongo on the coast of Guinea. These cattle are called Calongos; they are very delicate and weak. This variety never makes its appearance in cold districts.—Another remarkable fact relative to the oxen of South America is recorded by M. Roulin. In Columbia, the practice of milking cows was laid aside owing to the great extent of farms and other circumstances. In a few generations, the natural structure of the parts, and withal the natural state of the function, has been restored. The secretion of milk in the cows of this country is only an occasional phenomenon, and contemporary with

the actual presence of the calf. If the calf dies, the milk ceases to flow ; and it is only by keeping him with his dam by day, that an opportunity of obtaining milk from the cow by night can be found. This testimony is important, by the proof it affords, that the permanent production of milk in the European breeds of cows is a modified function in the animal economy, produced by an artificial habit continued through several generations." (Natural History of Man, p. 34.)

In the year 1770, as we learn from Azara, a hornless bull was produced in Paraguay, which has been the progenitor of a race of hornless cattle that has since multiplied extensively in that country. We need not refer to the remarkable differences in the form, size, and direction of the horns which exist amongst the domesticated breeds of our own country ; since with these our reader must be familiar. But we may add to the foregoing statements, a notice of the Zebu or Brahmin ox, which inhabits the East Indies, and is found also in Madagascar and the eastern coast of Africa. It is chiefly remarkable for its large fatty hump ; the size of which, however, varies with the degree of repose and the amount of food which the animal enjoys. This animal is distinguished by a docility fully equal to that of the European ox ; and it is thought by many naturalists to be of the same species. Whether or not this be the case, it has the same tendency to pass into varieties ; for numerous races are to be met with, differing in size from that of our largest cattle to that of a young calf, and having variations also in form and aspect. Further, it now seems to be the general opinion, that our domesticated breeds are the descendants of a wild species, which formerly inhabited the forests of central Europe, and which was described by Cæsar and other ancient authors under the name of *Urus*.* This appears, from historical evidence, as well as from the remains preserved to us, to have been a much more powerful animal than our existing races ; being especially distinguished by the size and direction of its horns. In a specimen found at Melksham, the distance between the ends of the bony cores was four feet ; and the space between the horns at their greatest distance must have been much more. The horns bent forwards and downwards. The difference between the ancient urus and our domesticated breeds was not greater, however, in this respect at least than that which exists among the different breeds of the half domesticated buffaloes. Thus the arnee of India, which is referred by the most competent authorities to the same stock with the ordinary buffalo, has horns which often measure from four to six feet in length, and ten feet between the tips.

The sheep which were transported into America by the Spaniards, have not multiplied so extensively as the oxen and swine ; and in many parts they are scarcely to be met with. The following fact, however, regarding the sheep of one of the valleys of the Cordillera, shows the marked influence of climate in altering their woolly covering.

"Wool grows on the young lambs nearly as in temperate climates ; if shorn, it sprouts again, and the fleece is formed as usual. If neglected, however, it forms itself into a large tufted mass, which breaks off in shaggy portions. When it comes off, there is found beneath, not fresh wool, nor a naked and diseased skin, but a short fine hair, shining and smooth, like that of the goat in his best state ; and this remains permanent, the wool never reappearing." (Natural History of Man, p. 37.)

* The animal now known under the name of urus or aurochs, is of a different species.

We have ourselves observed the same kind of modification in the covering of the sheep inhabiting our West India colonies; and we believe that we are correct in stating, that a residence of two or three years is sufficient to produce it in sheep transported from temperate climates. So completely similar is the hairy covering of the sheep and the goat in those climates, that the two animals cannot be distinguished in any other way than by their general conformation; and this, too, from the general absence of any considerable accumulation of fat in the bodies of the sheep, is not nearly so distinctive as it is with us. The goat in South America has become more agile, of more slender make, with the head better formed, and bearing smaller horns than in Europe. The most marked sign of domesticity in our European goats, viz. the large size of the teats, has completely disappeared in the goats, as in the cows, of South America. We are informed by Azara, that sheep and goats bear twice in the year in South America, and produce three lambs or kids annually.

The sheep is one of the most anciently domesticated animals, and it is one in which great varieties display themselves. We have in our own country numerous breeds, differing as to their stature, the texture of their wool, the presence or absence of horns, &c. But these are by no means so unlike each other, as are the various breeds of Asia and Africa, which are regarded by naturalists as having a common specific character. Thus we find in Spain a breed of sheep distinguished by the length and straightness of the hair, and by their long spiral horns. In the breeds spread through Persia, Tartary, and China, the tail seems replaced by a double spherical mass of fat, which forms a most awkward excrescence on the rump, and is nearly destitute of hair. These sheep, when transferred to the cold dry pastures of Siberia, lose their accumulations of fat after a few generations. The sheep of Syria and Barbary, on the other hand, have an accumulation of fat in the tail itself, which is long, and sometimes attains a weight of from 70 to 100 lbs.

"Now breeds of sheep are frequently formed in different countries in which particular qualities predominate, according to the preference of the breeders. This is done, partly by crossing or intermixing races already constituted and well known, but in great part also by selecting individuals from the stock, in which the particular qualities are more marked than in the generality of the same breed. In these instances, the natural or congenital variety, which the individual animal displays perhaps for the first time, becomes perpetuated by the hereditary transmission of such characters, which is the law of the animal economy. A striking instance of this fact is to be found in the origination of the new breed of sheep in the state of Massachusetts, which has been noticed by many writers in connexion with this subject. In the year 1791, a ewe on the farm of Seth Wright gave birth to a male lamb, which, without any known cause, had a longer body and shorter legs than the rest of the breed. The joints are said to have been longer, and the fore-legs crooked. The shape of this animal rendering it unable to leap over fences, it was determined to propagate its peculiarities; and the experiment proved successful; a new race of sheep was produced, which, from the form of the body, has been termed the otter breed. It seems to be uniformly the fact, that when both parents are of the otter breed, the lambs that are produced inherit the peculiar form." (Natural History of Man, p. 46.)

The history of the propagation of this breed offers some instructive facts. In the first year that this peculiar race was used for breeding, only two lambs were obtained with the same peculiarities. More were obtained

in subsequent years; but it was only when they became capable of breeding with each other, that the new race became permanent, the variety having previously a tendency to merge into the ordinary type. Now in the human race, it is not uncommon to meet with families, distinguished by some peculiar feature, or by a well-marked departure from the ordinary conformation, such as the possession of six fingers on each hand, and six toes on each foot. If such were to intermarry exclusively with one another, there can be no reasonable doubt, that the children would invariably exhibit the same peculiarity; and the six-fingered race, which now tends, whenever it is originated, to merge in the prevailing five-fingered type, would then become permanent. When it is considered that the influence of a scanty population, in the early ages of the world, by isolating different families from each other, and causing intermarriages amongst even very near relatives, would have been precisely the same with that which is now exercised by the breeders of animals, we can understand why the varieties which *then* arose should have had a much greater tendency to become permanent, than most of those which *now* present themselves.

We need not advert to evidence of a parallel kind, furnished by the known variations of several other species of domesticated animals; but we must say a few words in regard to the dog, which, of all known species, seems to be the one most capable of passing into widely-divergent varieties. These varieties differ extremely, not merely in their stature, the form and proportions of their body and limbs, the nature and colour of their hairy covering, &c.; but also in a point which elsewhere seems more unalterable, namely, the conformation of the cranium, the size of the brain, and the development of the organs of the senses. Thus, to advert to a few breeds only, we find in the Dingo of Australia a cranium, differing but little from that of the wolf.

"In both the head is very flat, and the cavity which contains the brain has comparatively very little space. This arises from the flattening of the temporal and parietal bones; which, from their outer and lower margins, pass almost in a level plane towards the median line, where they join the opposite bones with very little elevation, thus forming a flattened roof for the cerebral cavity. The Danish dog, and the mastiff, resemble the Australian in the shape of their heads, and they display as little development of intellect or sagacity. The terrier and the hound differ from the preceding breeds, in having the parietal bones much more arched, and allowing a larger space for the brain. The greyhound has a larger muzzle and smaller frontal sinuses than the hound; and the sense of smell is proportionably deficient in this breed. The shepherd's dog, which displays much greater sagacity than the hunting dogs just mentioned, and which Buffon very erroneously considered as the least modified by domestication, has a very considerable capacity of the cranium. The temporal bone in the head of the shepherd's dog is not flattened from the inferior margin, or rounded off with a trifling degree of arching or elevation towards the opposite side. In the shepherd's dog, the bones rise perpendicularly to one half their vertical extent, and then become arched over the space occupied by the brain. The wolf-dog resembles the shepherd's dog. Again, in the spaniel and water-dog, the capacity of the cranium is much greater than in the shepherd's dog; and these races, in all their varieties, are remarkable for a great development of the frontal sinus, which is so considerable, as to give the outline of the forehead a direction almost perpendicular to that of the nasal bones; the lower jaw is very much bent. The head of the bull-dog differs remarkably from all the preceding varieties; the posterior parts of the system of facial bones are situated higher than the muzzle, and the

jaws have a curved direction; the muzzle is shortened, and its breadth greater as four to three. The cranium of the bull-dog is much less capacious than that of the shepherd's dog; and the parietal bones, instead of being arched, bend towards each other almost at right angles. The docility of these races evidently bears a due proportion to the capacity of their skulls. The wolf-dog, and the spaniel and water-dog, display wonderful intelligence, and seem to understand the voice of man." (Natural History of Man, p. 57.)

It has been asserted by some naturalists, that the chief varieties of the dog ought to rank as distinct species; but there are several objections to such a view. And this question is one of peculiar interest to the anthropologist, since the very same mode of inquiry ought to be pursued in both instances; and that which is freely admitted to be the right method of arriving at a decision in the one case, must be the legitimate ground for determining the other. In the first place, then, it is extremely difficult, if not quite impossible, to draw definite lines between the several breeds of dogs; owing to their strong tendency to mutual approximation. The best-marked or typical forms of each kind may be distinct enough; and yet, even without any crossing of breeds, it is found on a close examination that intermediate forms not unfrequently present themselves, possessing the distinctive characters of their race in a modified degree, and even blending them with those of some other. Moreover, if we once begin to form distinct *species* out of the different breeds of dog, we cannot stop short with five or six, but must go on, as Frederic Cuvier showed, to at least fifty. Again, the different races all breed freely with each other; they have the same period of utero-gestation; and, when they escape from the dominion and influence of man, and go back to their wild unreclaimed state, they all return to a common form, lose their variety of colour and marking, and assume new habits adapted to the circumstances in which they live. It is a curious circumstance that they generally lose the power of barking, which appear to be an hereditary accomplishment, first acquired under the influence of man. The proper wild dogs, such as the Dingo of Australia, do not bark; they only howl. The dogs introduced by the Spaniards, which have run wild in South America, have lost the habit of barking. The progeny of the dogs, left by the Spaniards in the island of Juan Fernandez, before Lord Anson's time, were never known to bark. Two dogs brought to England by Mackenzie from the western parts of America, could never bark, and continued to utter their habitual howl; but a whelp, bred from them in England, learned to bark. It is a curious observation of M. Roulin, that the cats also in South America have in like manner lost those *miaulemens incommodes*, which so often disturb the nocturnal repose of the inhabitants of Europe. A distinguishing feature of all the races of dog, varying, however, with their degree of intelligence, is their disposition to attach themselves to man. This, however, seems to return to a dormant condition in all the races that have run wild; but to remain capable of being aroused in the course of a generation or two. For these and other reasons, it appears to be a conclusion from which there is no legitimate means of escape, that all the breeds or races of dog are varieties of one common specific type. The question is, what is that type?

There is no race of wild dogs at present existing, which can be regarded with any certainty as having kept up the original stock, without having

been at any time altered by the influence of man. Even the Australian dingo, which seems the least humanised (if we may use the expression), is probably the descendant of a once domesticated race; since there are many considerations which induce the zoologist to believe that it is not properly a native of New Holland, like the marsupials and monotremes which there represent the mammalian type, but that it was introduced there by the agency of man. Now it is a fact that seems common to the Australian dingo, the dhole of India, and other partially wild races of dogs, that their general conformation approaches that of the wolf. "In proportion, as they are more wild, they exhibit the lank and gaunt form, the length of limbs, the long and slender muzzle, and the great comparative strength, which characterize the wolf; and the tail of the Australian dog, which may be considered as the most remote from the state of domestication, assumes the slightly bushy form of that animal." (Bell's British Quadrupeds, p. 197.) The uniform dull-brown hue, moreover, which all the wild races of dogs present, much resembles that of the common wolf. It has been objected that the wolf does not exhibit that character, which is so remarkable in all the races of dogs,—a disposition to attach itself to man. Even the wild breeds of dogs are easily brought under subjection, and are made useful to him in various ways, which could not be the case if they had the same savage disposition as the common wolf. But it has been proved that the wolf is much more capable of domestication than is commonly supposed, if taken young from its wild state and brought under the influence of man; and that it then displays as much attachment to its master, and remembrance of kindness shown it, as any dog could do. So that there is no difficulty in understanding how, by a continuance of this influence through successive generations, the character of the race may become so permanently changed that the traces of former domestication may not be altogether lost, even in breeds which have returned to their wild state for centuries. There is no reason why the *psychical* character of a peculiar aptitude for domestication should not manifest itself as the special feature of a variety, like any *physical* character, such as the remarkable conformation of the otter breed of sheep; or why, to put the matter in a still more definite form, the first manifestation of the variability of the species of wolf, should not be the production of a race disposed to attach itself to man, from which race, under the continued influence of domestication, an almost infinite variety of new breeds has arisen, differing in the number of vertebræ in their tails, in that of their toes, and even in that of their molar teeth. These last departures from the ordinary type exist, not merely in particular races most remote from it, but also within the limits of single varieties or breeds, just like the occasional appearance of six-fingered races among various nations of men. They cannot, therefore, be admitted as specific differences; and they serve to prove how *very* wide are the limits of variation in this species.

The question of the specific identity of the dog and the wolf is at present undecided; and it is generally admitted that the determination must be founded chiefly upon the characters supplied by the generative function. However strange it may seem, the precise period of utero-gestation in the wolf has not yet been ascertained. An erroneous statement of it was adopted by John Hunter, who employed this as an argu-

ment *against* the specific identity of the two races. But so far as is at present known the period is the same in the wolf as in the dog, namely, 63 days. If this should prove to be the case, it will be a powerful argument for their specific identity; whilst, on the other hand, if the periods should be shown to differ, the specific diversity must be admitted. The results of hybridism, too, have yet to be properly tested. It is well known that the dog will breed with the wolf, and that the offspring will breed again with either of the parent races; but it is yet to be ascertained whether the offspring of a dog and wolf will breed with another hybrid of the same kind. Should the results of this test accord with that of the preceding, no reasonable doubt, we think, will then remain on this interesting question.

Thus it will be observed that, in adopting the conclusion of the specific identity of the different races of dog with each other, the zoologist is guided by the following considerations. 1. That, notwithstanding the wide differences between the typical forms of each variety, their distinctive marks want that constancy, which is essential to a specific character. 2. That all the races breed freely with each other; the hybrid offspring breeding with one of its own kind, as readily as with one of either of the parent stocks. 3. That the period of utero-gestation is the same in all the varieties. 4. That when any of the breeds run wild for several generations, they return to a common form, losing altogether their previous distinctive peculiarities. 5. That, notwithstanding the peculiarities of habit and instinct, which the several breeds have acquired, they all possess the instinct of attachment to man; this being retained, even after the race has lived in a wild state for many generations. Upon such grounds as these, most naturalists recognise without hesitation the specifie indentity,—that is, the *common*, or at least the *similar* origin,—of all known breeds of dog; notwithstanding that the differences between them amount to well-marked changes in the form of the skull, the size of the brain, the development of the organs of sense, the form and relative size of the body, limbs, and tail, the number of vertebræ in the latter, the thickness, length, colour, and texture of the hairy covering, and peculiarities of instinct which are distinctly hereditary. And further, the approximation of the form and characters of the wild dog to those of the wolf, would be regarded, if borne out by the characters derived from the generative function, as sufficient to establish the specific identity of these two races: the wolf being the permanently wild form, the domesticated dog the humanised form, and the dingo, ghole, and other so-called *wild* races of dog, being the half-reclaimed form, of one and the same species.

For the *cause* of such variations from the common or original type, we must look in part to the original constitution of the species, and in part to the influence of external conditions. As already pointed out, there is a marked difference among various species of animals (even those nearly allied, such as the domestic cat and the tiger), in regard to their respective *capacities for variation*. It is from this circumstance, that we find some species (of plants as well as animals) restricted to particular conditions in regard to climate, food, &c., because their constitutions cannot adapt themselves to any other; whilst others are more widely dispersed, simply because their constitutions are more capable of modification in accordance with a change of circumstances. A change to which the latter can readily

accomodate themselves, would be fatal to the former. Now this accommodation is effected by a change in the organism itself, of which the marks soon become evident, as we have already seen in the alteration of the hairy covering of the sheep transported to hot climates; and the continued action of the same circumstances for a few generations gives increased permanence to the new characters of the breed. It cannot be questioned, if the history of the domesticated races of animals be fairly considered, that changes in external conditions are capable of exerting a very decided influence upon the physical form, the habits and instincts, and the various functions of life, in species possessing this adaptiveness. The variations thus induced extend to considerable modifications in the external aspect, such as the colour, the texture, and the thickness of the external covering; to the structure of limbs, and proportional size of parts; to the relative development of the organs of the senses and of the psychical powers, involving changes in the form of the cranium; and to acquired propensities, which, within certain limits (depending, it would appear, on their connexion with the natural habits of the species) may become hereditary. As already remarked, wherever we find this *adaptive* capacity the greatest, we also observe the greatest tendency to what we must call *spontaneous* variation; that is, to variations of which we cannot trace any obvious cause in external conditions. Such are the varieties of features, complexion, general conformation, and mental character, amongst the offspring of the same parents, born under (so far as we can trace) the very same circumstances; and the peculiarities of family character, which are frequently thus propagated through many generations,—sometimes reappearing in the grandchildren, when they have lain dormant in the children. Such, too, are the development of additional fingers or toes, the alteration in the number of vertebræ in the tail, the unusual consolidation or separation of the toes, &c. &c.; of which we cannot find any explanation in the agency of external conditions. And we may take it as a general principle, that the tendency to *adaptive* and to *spontaneous* variation always go together.

We now come to apply the same method of investigation to the human races; and shall give a general summary of the evidence, by which, as it seems to us, the specific identity of all of them is proved as unequivocally as that of the different breeds of sheep, oxen, or pigs, whose common parentage no one at all versed in the subject ever dreams of questioning. We would remark, in the first place, that a very great diversity might naturally be looked for, amongst the races of men inhabiting different parts of the globe; since the very circumstance that individuals of any one race can sustain their existence in any part of the world where they can obtain food, shows a constitutional *adaptiveness* to external conditions, the effect of which must in the course of generations become evident in the physical conformation. And again, the variations which present themselves within the limits of any one breed or race,—that is to say, the differences which mark the several individuals, families, or nations, of whose common ancestry there is no doubt,—are greater than those which we meet with in the case of any other species. There is evidently, therefore, a very strong tendency in the human constitution to *spontaneous* variation; and it is easy to conceive that this tendency might have shown itself very early in the history of the species, so that mem-

bers of the same families might have exhibited a much greater diversity than they now usually present; and, by the breeding in-and-in, which (as already remarked) would be the necessary result of a scanty and scattered population, the peculiarities thus arising would be increased and rendered permanent.

The characters which are most relied on for the discrimination of the several races of mankind, are the colour of the skin, the nature of the hair, and the conformation of the skull and other parts of the skeleton. Each of these will require to be separately considered.

The colour of the skin, it is now well known, exists in the epidermis only; and depends upon the admixture of "pigment-cells" with the ordinary epidermic cells. These pigment-cells are distinguished by their power of drawing from the blood, and elaborating in their own cavities, colouring matter of various hues; and all the varied shades of colour, presented by the different races of men, are due to the relative amount of these cells, and the particular tint of the pigment which they form. What was formerly known as the "rete mucosum," and described as a distinct colouring layer beneath the epidermis, is now known to be nothing else than the newest and softest layer of epidermis. There is no structure in the skin of the dark races that is at all peculiar to it; the very same dark cells being found in parts of the fairest skins. Still it might be affirmed, that the strongly-marked distinctions which present themselves between the typical hues of certain races, ought to be accepted as valid specific distinctions; since particular spots or patches of colour are admitted to be so in other instances. Thus, for example, the fair and ruddy Saxon, the jet-black Negro, the olive Mongolian, and the copper-coloured North American, would seem positively separated from each other by this character; propagated, as it seems to be, with little or no perceptible change from generation to generation. But although such might appear to be the clear and obvious result of comparisons of this kind, yet a more extended survey tends to break down any such distinction. For, on tracing this character through the entire family of man, we find the isolated specimens just noticed to be connected by such a series of links, and the transition from one to the other to be so very gradual, that it is impossible to say where the line should be drawn. There is nothing which at all approaches to the fixed and definite characters, which the zoologist admits as specific distinctions amongst other tribes of animals. On the other hand, we find such a constant relation between the climate and the colour of the skin, that it is impossible not to perceive the connexion between them. The parts of the globe included between the tropics or closely bordering upon them, form the exclusive seat of the native black races; whilst the colder temperate regions are the residence of the fair races; and the intermediate countries are inhabited by people of an intermediate complexion. On passing from northern Europe to central Africa, we meet with a most regular progression of this kind. Further, elevation above the level of the sea is found to have the same uniform relation to the human complexion, that it has to vegetation; for as we find the plants of temperate or even arctic regions on the sides of intertropical mountains, so do we notice that high mountains and countries of great elevation are almost uniformly inhabited by people of lighter hue than those of the surrounding country; whilst low

and level countries, especially those which border on the sea, are generally tenanted by people of unusually dark colour. These distinctions are particularly well seen in Africa and in India. The deepest hue among the African races is to be found among the Negroes of the swampy plains of the Guinea coast; and there are several instances in which nations residing at no great distance from these, but at a higher level, are comparatively light, although their ancestry is undoubtedly the same. In India, there is distinct historical testimony, that races which are now found inhabiting high elevations on the Himalaya and other lofty mountain ranges, and which have complexions of almost European fairness, are the descendants of dark Hindoos, which migrated thither from the low countries.

If we take any one of those groups of nations, which are usually regarded as altogether constituting a race, such as the (so-called) Caucasian, the African, the American, or the Polynesian, we find that the greatest diversities of complexion present themselves within its limits. Thus we presume that no one who possesses even a smattering of philology will now question the eastern origin of the bulk of the European nations; yet from the very same stock that produced the fair Saxon and Celtic nations, the jet-black Hindoos of Lower India have sprung. In the same manner, upon philological evidence, the Arab tribes bordering the great desert of Sahara, some of which are as black as the darkest Negroes, are referred to the same stock with the red-haired and blue-eyed inhabitants of the mountainous regions of Yemen. Of the Jewish nation again, some which are dispersed through the colder countries of Europe, and have been acclimatized by a residence of many centuries, have come to assume the fair complexions and red or brown hair of the nations among which they live; whilst others, inhabiting Southern Europe, or still clinging to their ancient home, seem to present us with the original hue of the nation; and others, long settled in India, have become as dark as the black population around them. This last case is one of an extremely interesting character, and peculiarly satisfactory in the testimony it affords to the effect of a prolonged exposure to climatic influences; since it is well known that, in consequence of their national and religious peculiarities, the Jews maintain a complete isolation from the people among whom they may happen to dwell. It is probable that, in nations as in individuals, a pre-existing tendency to a swarthy complexion will cause the effect of long-continued exposure to a tropical climate to be more decided in blackening the hue of the skin, than it is in cases, where the fairness of the complexion shows an indisposition to the secretion of dark pigment by the epidermic cells; and that the Jews are thus more readily blackened than Saxons or Celts would be. The same holds good with regard to other nations than Jews; for the descendants of the early Portuguese settlers in India have become in many instances as dark as the Hindoos around them. No doubt, this change has originated in part from intermixture of races; but still the complete merging of the original complexion, whilst other characters of the European stock are retained, shows that such an intermixture by no means fully accounts for the change.

Various phenomena, presented by individuals of all the principal races, tend still further to break down any fancied barrier which it might be attempted to erect, on the ground of colour. For among all the chief subdivisions, *albinoism*, or the absence of pigment-cells, occasionally pre-

sents itself; so that a fair skin, nearly resembling that of the native of Northern Europe, may present itself in the offspring of the Negro or of the Red Man. On the other hand, instances are by no means rare of the unusual development of pigment-cells, in individuals of the fair-skinned races. Thus we frequently meet with persons of a swarthy complexion, whose descent appears to be quite free from the slightest infusion of the blood of the sable races, and who have never been themselves exposed to the darkening influence of hot climates. Or there may be a special development of pigment-cells in particular spots. Thus the summer freckle, the influence of sunlight in producing which cannot be questioned, is formed by an aggregation of red or brown pigment-cells. And the areola around the nipple, which in some women becomes very dark during pregnancy, derives its colour from the same source. Instances are not unfrequent, in which large patches of the surface of the body or limbs become discoloured, even almost blackened, during pregnancy; and this in females of remarkably fair complexion. The influence of light in favouring the development of the black pigment-cells is further shown by the circumstance, that the infants of dark races do not acquire their characteristic hue until some days after birth; and by the marked difference in complexion, which exists in many Polynesian nations, between the chiefs and females who pass the greater part of the day within doors or under the shelter of the umbrageous foliage, and the common people whose avocations require them to be freely exposed to the light and heat of the tropical sun; the former being often no darker than the inhabitants of Central or Southern Europe, whilst the latter are quite swarthy and sometimes even of Negro blackness. It is not improbable that such differences are partly congenital, being fixed in the respective *castes* by the operation of similar agencies through a long succession of generations; but there is no reason whatever for regarding them as indicative of original diversity of parentage. Dr. Prichard has collected a large number of cases which show that the *xanthous* variety,—distinguished by fair ruddy complexion and auburn, light brown, or red hair,—may spring up out of every melancomous or swarthy dark-haired race. In some of these cases, we find that the character of a whole tribe has changed in course of time, as may be proved by historical testimony, without the admixture of any other element, and without any obvious change in external conditions. In others, the change is confined to individuals; and this sporadic character is for the most part observable in the appearance, from time to time, of xanthous offspring from perfectly black parents. Dr. Prichard has shown that many of the so-called "white negroes" have not been *albinoes*, that is, destitute of all pigmentary matter in the skin, eyes, and hair; but have been truly *xanthous*, that is, have possessed the light colouring matter which is contained in the skin, eyes, and hair of the European. There are also cases in which fair races have become dark, without any considerable change in external conditions; thus we find that the Germanic nations, which were unanimously described by ancient authors as exceedingly fair, possessing yellow or red hair and blue or gray eyes, have become much darker since that time, so that these peculiarities are far from being common amongst them, and must now be rather looked for in Sweden. That an amelioration of the climate of Central Europe has taken place during the same period, cannot be questioned; but the

climatic change scarcely seems decided enough to account for such an alteration in the physical characters of the population. In whatever way we explain the fact, however, it stands in evidence as a clear proof of the variability of particular races of men; since the purity of descent of the Germanic nations at once negatives the supposition that their change of complexion can be attributed to any admixture of a foreign element.

The characters derived from the texture and distribution of the hair are intimately connected with those furnished by its colour and by the hue of the skin; but they seem at first sight to be more permanent, and therefore more distinctive. Thus the negro is usually characterized by his "woolly" hair; whilst the Northern Asiatics, commonly referred to the Mongolian type, are equally remarkable for the scantiness and tufted arrangement of their pilous covering, which has been compared to the mode in which the bristles are set in a scrubbing-brush. But, however decided these characters may seem to be in extreme cases, their value as indications of original difference of race altogether disappears when we examine closely into them. For, in the first place, Dr. Prichard has clearly shown, by microscopic examination, that the hair of the Negro is not really wool, and that it differs in regard to its intimate structure from that of the fairer races, solely in the greater quantity of pigmentary matter which it contains in its interior; and, in this respect, it is similar to the jet-black hair not unfrequently to be met with in our own country. The crisp and twisted growth of the Negro hair is the only character by which it can be really separated from the straight, flowing hair common amongst European nations; and this alone cannot be relied upon for a moment as an indication of original difference, since these national variations do not exceed those which present themselves within the limits of any one race. Thus we occasionally meet with Europeans whose hair is nearly as "woolly" as that of most Negroes; whilst, on the other hand, if we take the entire mass of the black native races of Africa into comparison, we shall find tribes among them, which, though similar in complexion and in most other physical peculiarities, yet differ in regard to their hair, and present every possible gradation from a completely crisp or so-called "woolly" hair, to merely curled and even to flowing hair. A similar statement may be made respecting the inhabitants of the islands of the great Southern Ocean; some individuals having crisp hair, whilst others of the same race have it merely curled. Even if, as Dr. Prichard justly remarks, the hair of the Negro were really analogous to wool, it would by no means prove the Negro to be a peculiar and separate stock, unless the peculiarity were *constantly* presented by all the nations of Negro descent, and were restricted to them alone; for there are breeds of domesticated animals which bear wool, whilst others of the same species, under different climatic influences, are covered with hair.

By most writers on the diversities of mankind, the varieties which are observable in the form and structure of the bony skeleton, and particularly in the cranium and pelvis, have been thought to furnish more important characters for the separation of the races, than those derived from the colour of the skin or the texture of the hair,—a certain capability of alteration in which, under climatic influence, can scarcely be gainsaid. It might certainly be supposed *à priori* that strongly-marked peculiarities in the configuration of the skull, in the proportion of the

parts of the body, and in the development of the brain, would be less likely to undergo modification from changes in external conditions or from spontaneous variation, than are the outward or superficial marks furnished by the tegumentary covering and its appendages. Since the time of Camper and Blumenbach, various attempts have been made by anatomists to divide mankind into groups, by taking the shape of the skull as the chief ground of distinction; but scarcely any two writers have agreed as to the number of primary divisions that should be established; and in nearly all such attempts it has been assumed as a guiding principle, that conformity in physical characters must be taken as the best indication of family relationship amongst nations,—a position which later philological investigations seem to have overthrown. It would be easy to select a European, an African, a Mongolian, a Malay, and an American skull, which, when strongly contrasted, might afford sufficiently distinctive characters; but the question would then arise, whether these characters are common to the entire races which they have been selected to represent, and whether they have the degree of permanency and invariability which is requisite to enable us to regard them as a ground of specific distinction. Now it will appear from the investigations, of which we shall subsequently give an outline, that no such typical forms can really be invested with any such character of constancy; for that they are far from being invariably presented in the several nations of one race, or even in the several individuals of one nation; and that they, too, are liable to undergo alteration under the influence of change in external conditions. It does not appear, however, that such alterations are liable to take place under the mere agency of climate; but the general habits of life, the supply of food, and the mode and degree of exercise of the mental faculties,—every influence, in fact, which enters into the terms barbarism and civilization,—have a much greater tendency to bring them about. And consequently we find that in every race there may be an approximation, both in the form of the skull and in the general configuration of the body, to the characters which we are accustomed to regard as distinctive of the highest; whilst the highest may undergo such degradation as may bring it into approximation with some of those usually ranked as inferiors. The number of leading types of configuration of the skull is reduced by Dr. Prichard to *three;* and he considers that there is sufficient evidence for connecting these with different habits of life.

"Amongst the rudest tribes of men, hunters and savage inhabitants of forests, dependent for their supply of food on the accidental produce of the soil or on the chase, among whom are the most degraded of the African nations and the Australian savages, a form of head is prevalent which is most aptly distinguished by the term *prognathous*, indicating a prolongation or extension forward of the jaws; and with this characteristic other traits are connected which will be described in the succeeding pages.

"A second shape of the head, very different from the last mentioned, belongs principally to the nomadic races, who wander with their herds and flocks over vast plains, and to the tribes which creep along the shores of the Icy Sea, and live partly by fishing and in part on the flesh of their reindeer. These nations have broad and lozenge-formed faces, and what I have termed *pyramidal* skulls. The Esquimaux, the Laplanders, Samoides, and Kamtschatkans, belong to this department, as well as the Tartar nations, meaning the Mongolians, Tungusians, and nomadic races of Turks. In South Africa, the Hottentots, formerly a nomadic people who

wandered about with herds of cattle over the extensive plains of Kafirland, resembling in their manner of life the Tungusians and the Mongoles, have also broad-faced, pyramidal skulls, and in many particulars of their organization resemble the Northern Asiatics. Other tribes in South Africa approximate to the same character, as do many of the native races of the New World.

"The most civilized races, those who live by agriculture and the arts of cultivated life, all the most intellectually improved nations of Europe and Asia, have a shape of the head which differs from both the forms above mentioned. The characteristic form of the skull among these nations may be termed *oval* or *elliptical*." (Natural History of Man, p. 108.)

Now that these typical forms are *not* permanent, but may be changed under the influences of civilization,—just as we have seen the form of the cranium to be changed by domestication in the hog, to say nothing of the greater changes in the canine races,—appears to us to be demonstrated by adequate evidence. Although it is not infrequent to cite the Negro race as an example of *permanence* under different conditions, yet nothing can be more fallacious. We have the testimony of a most intelligent observer, Dr. Hancock of Guiana, to the fact that it is not uncommon to meet with Negroes in that colony, the descendants of those first introduced into it, who now exhibit a decidedly European cast of countenance, although their descent has been perfectly free from admixture of European blood; and he even goes further and asserts that, among those especially who have been employed as house-servants, and have therefore been brought into closer relation with their European masters, it is not at all difficult to distinguish a Negro belonging to the Dutch portion of the colony from another belonging to the British settlements, by the correspondence in the features and expression of each with those which are characteristic of his respective masters. We have been informed by Mr. Lyell that, during his recent tours in the United States, he made numerous inquiries on this head, especially from medical men residing in the slave states; and that the testimony of all those who had paid attention to the subject was unanimously to the same effect,—the approximation in general configuration of head and body to the European model being more and more evident in each successive generation of Negroes, especially in such as were brought into closest and most habitual relation with the whites, without any actual intermixture of races. There is no nation in which we have the power of tracing historically the elevation of the skull from the prognathous type to the perfect oval or elliptical, under the influence of civilization prolonged through many successive generations; but as the most elevated physical characters are found amongst those of the African nations, which have, in some degree, emerged from their original barbarism, chiefly in consequence of the superior influence of the Mahommedan religion which they have embraced, it does not seem unfair to presume that their elevation has been the result of their refinement. On the other hand, the most strongly-marked examples of the prognathous type are to be found amongst the most degraded Negroes of the Guinea Coast, and among the Australians, who may be ranked with these as very near the bottom of the scale of humanity.

The variability of the *pyramidal* type of skull, and its convertibility into the more symmetrical type, characteristic of the so-called Caucasian race, seems clearly demonstrated by the change which a portion of the Turkish nation has undergone. The present aspect of the cranium and features of the

Turks of Europe and Western Asia is, on the one hand, so closely accordant with that which we encounter in the great bulk of European nations, and on the other, so dissimilar to that which is presented by the Turks of Central Asia, that some of those writers, who look upon physical characters as the chief test of family relationship amongst nations, have actually maintained the Western or Ottoman Turks to be descendants of the Caucasian stock, and to have no real affinity with the Eastern or Mongolian race. All the most learned inquirers, however, are now agreed that there is clear historical evidence that the Ottoman Turks are a branch of the same stem with the Turks of Central Asia. The latter continue in the original nomadic habits of the nation; and continue to exhibit, probably with little or no alteration, the original configuration of the skull and characteristic physiognomy of the race. The former, however, having conquered the countries which they now inhabit eight centuries since, have settled down to the fixed and regular habits of the Indo-European nations, and have made corresponding advances in civilization; and the national type of cranium has been completely changed during this period, from the *pyramidal* to the *elliptical*. Some of those who have been forced to admit the fact of this change, have endeavoured to avoid what seems the obvious inference from it,—that the change has been the result of civilization and social improvement,—by attributing it to an intermixture of the Turkish race with that of the countries which they conquered, or to the introduction of Georgian or Circassian slaves into their harems. But there is every probability that the difference of religion and manners kept them as separate at first, as it has since done, from the inhabitants of the countries which they conquered in the course of their migration; and the improvement effected by the introduction of Georgian and Circassian slaves must have been confined to the higher classes who alone can afford to purchase them. In either case, the cause assigned, even if admitted to the full extent claimed, would have been insufficient to produce the entire substitution of the new type for the original one, which observation demonstrates to have taken place. There is one other attempt at explanation which requires notice, although it has not the least pretence to be so received. There are those who maintain that the entire Turkish race is of Caucasian origin; the type of conformation now presented by the Ottoman Turks being the original; whilst the tribes of Central and Eastern Asia, which now present the pyramidal skull, have acquired that type by admixture with the Mongolian races. Here, again, the explanation would be insufficient, even if the facts were most fully admitted, to account for the *complete* substitution in the Tartar races of the pyramidal type for the elliptical form, which, according to this hypothesis, they originally possessed. But the idea is totally destitute of foundation. All historical testimony goes to prove that the race primarily sprang from the remote East, and that it was entirely disconnected from the Indo-European races until the westward migration of a portion of it brought it into collision with them. It is utterly absurd to imagine that its original character should have been preserved better by those parts of the race which have undergone a complete change in their habits of life, than amongst those which continue in their primitive semi-barbarous condition. And the supposition that any extensive change could have been produced by an

admixture of the Mongolian element, is at once negatived by the fact that the Mongols form but a small tribe in comparison with the widely-extended nations of the Turkish race; whilst the purity of descent of the latter is further evidenced by the small amount of variation in the languages even of those most removed from each other,—their conformity being so close that, according to M. Jaubert, a native of Constantinople might travel eastwards as far as the frontier of the Chinese empire, and be understood all the way.

Another instance of the same modification is to be found in a different group of the nations originally characterized by pyramidal skulls. The North and East of Europe still contains the descendants of tribes, which seem to have been in possession of it before the appearance of the races of Indian descent in that quarter of the globe. Some of these are well known under the name of Lapps and Finns, whose similarity of origin is indubitable, although their physical characters now present important differences; the Lapps retaining, in a well-marked degree, the pyramidal skull, whilst the form of the cranium, in the modern Finn, is far more oval. But there is another group not commonly referred to the same stock, which appears from Dr. Prichard's researches to be fairly referable to it, and to have undergone a still greater departure from the original type; this is the race of Magyars, of which the Hungarian nobility is composed, a race which is not inferior in physical or in mental characters to any in Europe. Now we have in the Lapps, Finns, and Magyars, three distinct gradations in the form of the cranium, between the pyramidal and the elliptical type; and the amount of change, in each tribe, presents a most striking conformity to its degree of advance in civilization. Thus the Lapps, retaining the original nomadic habits of the race, retain also the pyramidal type more decidedly than either of the others; whilst the Finns, who are nothing else than Lapps half civilized by a settled agriculture, show a decided approximation to the ordinary European physiognomy; and in the Magyars, who have made the greatest advances in refinement, that which may fairly be presumed to have been the original type has been entirely superseded by the more elevated one.

On the other hand, that the continued operation of degrading causes tends to produce a degeneration in the form of the cranium, and in the physiognomy of the more elevated races, as well as in their general configuration, must be apparent, we think, to those who have had opportunities of studying the physical characters of the worst specimens of our own population. Dr. Prichard quotes from an Irish writer a very striking testimony to the truth of this position; and we quote it in full, as being of peculiar importance at the present time, when public attention is being so strongly directed to the social evils of that unhappy country, which threaten, if continued, to reduce the great mass of the population to the same fearful state as that here so graphically portrayed.

"On the plantation of Ulster, and afterwards on the successes of the British against the rebels of 1641 and 1689, great multitudes of the native Irish were driven from Armagh and the south of Down, into the mountainous tract extending from the barony of Flews eastward to the sea;—on the other side of the kingdom, the same race were expelled into Leitrim, Sligo, and Mayo. Here they have been almost ever since, exposed to the worst effects of hunger and ignorance, the two great brutalizers of the human race. The descendants of

these exiles are still readily distinguishable from their kindred in Meath and in other districts where they are not in a state of physical degradation; being remarkable for *open projecting mouths, with prominent teeth and exposed gums; their advancing cheek-bones and depressed noses bearing barbarism on their very front.* In Sligo and northern Mayo, the consequences of two centuries of degradation and hardship exhibit themselves in the whole physical condition of the people; affecting not only the features but the frame, and giving such an example of human deterioration from known causes, as almost compensates, by its value to future ages, for the suffering and debasement which past generations have endured in perfecting its appalling lesson. Five feet two inches upon an average, pot-bellied, bow-legged, abortively-featured, their clothing a wisp of rags,—these spectres of a people, that were once well-grown, able-bodied, and comely, stalk abroad into the daylight of civilization, the annual apparitions of Irish ugliness and Irish want. In other parts of the island, where the population has never undergone the influence of the same causes of physical degradation, it is well known that the same race furnishes the most perfect specimens of human beauty and vigour, both mental and bodily." (Dublin University Magazine, No. 48.)

The characters here assigned to a certain section of the Irish people are so precisely those which, in degrees more or less exaggerated, are presented by the natives of Australia, usually considered to be amongst the lowest in the scale of humanity; that it is not possible to avoid the inference that the latter owe their physical inferiority, in great part, to the extreme degradation of their mode of existence. We were particularly struck with the resemblance, on happening to see not long since the representation of a group of the natives of Australia, which is among the plates illustrating the Voyage de l'Astrolabe. This group would have passed very well, in all but their clothing and decorations, for an assemblage of such Irish as have been just described, specimens of which not unfrequently present themselves in our large towns.

There is another race at present existing under circumstances of the most depressing kind, and exhibiting the utmost degradation in its physical characters; in which the progress of the descent from a much higher physical and social condition can be historically traced. Writers on the history of mankind seem to be nearly agreed in considering the Bushmen or Bosjesmen, of South Africa, as the most degraded and miserable of all nations, excepting, perhaps, the Australian savages. They are described by M. Bory de St. Vincent, as differing most widely from what he terms the Japetic species of men, and as forming the transition from the genus Homo to the genera of Orangs and Gibbons; he even finds analogies between them and the Macacos. He speaks of them as even too brutish to be reduced to slavery; as having a language consisting only of a few guttural tones, and incapable of expressing any but a very limited range of ideas; and as scarcely having a capacity for reasoning. Their present mode of life is thus described by Dr. Prichard:

"No picture of human degradation and wretchedness can be drawn, which exceeds the real abasement and misery of the Bushmen, as we find it displayed by the most accurate writers who describe this people. Without houses, or even huts, living in caves and holes in the earth, these naked and half-starved savages wander through forests, in small companies or separate families, hardly supporting their comfortless existence by collecting wild roots, by a toilsome search for the eggs of ants, and by devouring, whenever they can catch them, lizards, snakes, and the most loathsome insects." (Natural History of Man, p. 515.)

There is nothing, however, in the form of their skulls or in their phy-

siognomy, which departs more widely from that of the Hottentots—to whom, as we shall presently see, they are nearly related,—than the physical characters of the most degraded tribes of the Irish nation differ from those of the superior part of the same nation; and there is not, as we are assured by unprejudiced observers, anything like that deficiency in their moral and intellectual faculties, which has been represented to exist by those who are searching for approximations between mankind and the inferior species of animals. Mr. Burchell, who sought and obtained opportunities of conversing with them, and of observing their manner of existence, though he found them in the most destitute and miserable state, yet discovered among them traits of kind and social feeling, and all the essential attributes of humanity. But whatever may be their *present* amount of degradation, the general argument in favour of the influence of external conditions is rather strengthened than weakened by those who depict it in its strongest colours; since there is clear proof that the Bushmen do not constitute a distinct race, but are nothing else than degenerated Hottentots.

"A careful comparison of their language with that of the Korah and other Hottentots, convinced Professor Vater that there is an essential affinity between them; and in recent times this conclusion has been fully established by local inquiries, and no diversity of opinion at present exists upon the subject. We are assured by one of the latest and best writers on South Africa, that the Bushmen are the remains of Hottentot hordes, who subsisted originally, like all the tribes of Southern Africa, chiefly by rearing sheep and cattle; but who have been driven by the gradual encroachments of European colonists, and by internal wars with other tribes, to seek for refuge among the inaccessible deserts and rocks of the interior. 'Most of the hordes,' says the same writer, 'known by the name of Bushmen, are entirely destitute of flocks and herds, and subsist partly by hunting, partly on the wild roots of the wilderness, on reptiles, locusts, and the larvæ of ants, or by plundering their hereditary oppressors, the colonists of the frontier. Having descended from the pastoral to the state of robbers and hunters, the Bushmen, as we are assured, have necessarily acquired, with their increased perils and privations, a more resolute and ferocious character: from a mild, confiding, and unenterprising race of shepherds, they have been gradually transformed into wandering hordes of fierce, suspicious, and vindictive savages; by their fellow-men they have been treated as wild beasts, until they have become assimilated to wild beasts in their habits and dispositions.'

"Difficult as it may be to imagine a change from the state of herdsmen to that of the miserable Bushmen, the transition has been actually observed and described. Among the Hottentot tribes, the Koranas are well known to be the most advanced in all the possessions and improvements which belong to the pastoral life. A late traveller in Africa, whose narrative is replete with good sense and the marks of accurate knowledge, has traced from observation the process by which hordes even of the Korah race have been reduced from the life of peaceful herdsmen to the condition of hunters and predatory savages. The Koranas, as visited by Mr. Thomson on the Hartebeest river, had actually undergone this transition; having been plundered by their neighbours and driven out into the wilderness to subsist on wild fruits, they adopted the habits of the Bushmen, and had become assimilated in every essential particular to that miserable tribe." (Natural History of Man, p. 517.)

We cannot suppose that any properly civilized race could undergo such a degeneration, in the course of the one or two generations which seem sufficient to convert Hottentots into Bushmen. But it is to be borne in mind, that the first steps in the progress by which a nation emerges from

barbarism are not towards those attainments in literature and art which, when once acquired, can never entirely lose their influence; but towards improvements in the simplest arts of life, the cultivation of the ground and the rearing of cattle, the construction of habitations, and the manufacture of implements. If deprived of these, they are at once depressed into a condition worse perhaps than their primitive rudeness; for they have not yet attained the intellectual development which will enable them to rise superior to circumstances and to triumph over difficulties apparently the most insuperable. That the moral and intellectual nature of the Hottentots is not different from our own, and though at present inferior is capable of unlimited elevation, will sufficiently appear, we think, from considerations hereafter to be stated; and every shadow of a reason, therefore, for regarding the Bushmen as one of those distinct races which are separated from our own by an impassable barrier, disappears under the light of a careful scrutiny of the facts. And if this be the case with regard to the wretched Bushmen, how much less ground is there for entertaining such a notion regarding the Negroes; whole nations amongst whom have attained a considerable degree of civilization; whilst individuals have every now and then proved by their attainments in the literature, art, and science of Europe, that they possess every faculty boasted by the so-called superior races, and that too in no inferior degree. The comparative rarity of such individuals must be imputed to two causes: first the want of opportunity for improvement, to which the great bulk of the race is at present condemned; and secondly, an inherent inferiority. This inferiority may be freely admitted by the warmest advocates of the equal rights of nations; for the admission involves no contradiction or abatement of their principles. That the Bushman and the Negro are at present on the average inferior in intellectual and moral capacity to the average of Englishmen, Frenchmen, or Yankees, only shows, in our apprehension, that they have been longer subjected to the influence of degrading causes; whilst the elevation occasionally attained by individuals clearly demonstrates that the capacities of these races have not been permanently and hopelessly repressed, but are capable of being again developed.

We have digressed in some degree from our direct line of inquiry; but differences in the form of the cranium have such an intimate connexion with the general capabilities of the several races which present them, and have been so much dwelt upon as establishing valid specific distinctions, that we have thought it necessary to enter at some length into the proof that these differences may be produced, or at least greatly modified, by the influence of external agencies prolonged through a sufficient period. We shall now advert to certain differences in the form of the pelvis, which have been thought to be characteristic of particular races or groups of nations. According to Dr. Vrolik of Amsterdam, the difference between the male and female pelvis is much more strongly marked in the Negro than in the European. The pelvis of the male Negro resembles, in the strength and density of its substance and of its component bones, the pelvis of a wild beast; but the pelvis of the female in the same race combines lightness of substance and delicacy of form and structure, whilst it presents a conformation which approximates in his estimation towards the narrow elongated pelvis of the chimpanzee or orang. The structure of the pelvis in the Hottentot race is only known from the skeleton of the

female who died in Paris in 1815; the shape of which indicated, in Dr. Vrolik's opinion, a still greater animality than is found in the pelvis of the Negro. In no other individual, except from deformity, have the ilia been observed to assume so vertical a direction; and they are remarkable, likewise, for their great height in proportion to their breadth, which helps to give the pelvis its character of peculiar elongation. Dr. Vrolik further states that the pelvis of the Javanese is distinguished by its peculiar lightness of substance, the smallness of its size, and the circular form of its upper opening. A comparison of the same portion of the skeleton in different nations has been carried out, however, upon a more extended and philosophical plan by Professor Weber of Bonn. Bringing together all the pelves he can collect, he classifies them according to their shape; and finds that they fall into four principal divisions, as follows:

"1. The oval form—*die ovale ur-becken-form.* An oval pelvis is one in which the upper opening presents an egg-shaped figure in such wise that this aperture at the anterior part, namely, at the symphysis pubis, is narrow; but towards the middle of the same aperture and the junction of the ilia with the os sacrum, becomes gradually and proportionally widened, and again becomes somewhat narrower in passing backwards towards the promontorium, when it ends in an obtuse point.

"2. The round form of the pelvis. A round pelvis is one in which the upper opening is round. The circumference, particularly at the symphysis and horizontal branches of the pubis, is more spread out than in the round oval form, whereas the conjugate has nearly the same extent as the transverse diameter.

"3. The square or four-sided form is the shape of a pelvis of which the sides, especially that formed by the os pubis, are flat and broad, so that the upper opening forms nearly a perfect square: the transverse diameter is greater than the conjugate.

"4. The wedge-shape—*keil-formige ur-becken-form*—belongs to the pelvis which appears on both sides compressed, so as to be narrower from side to side than from front to back. The ossa pubis unite under an acute angle, and the horizontal branches run backwards in a straighter direction than in the oval form; the conjugate is lengthened, and the upper opening is oblong rather than oval." (Natural History of Man, p. 127.)

Now if an attempt be made to assign one of these forms of pelvis to any particular nation or group of nations, as a constant distinctive character, it is found to fail *in toto;* specimens of each kind being found in the same races, although particular types are more common than others in particular races. Thus the oval type is more common in the European, where it is in accordance with the oval form of the skull; but round, square, and wedge-shaped pelves also occur amongst individuals of that race; whilst the oval form presents itself in the pelvis of a Botacudo, a people reputed to be the most savage of all the American nations. The round type is most frequent among the American nations; but it has been observed by Weber not merely in the European, but in a Negress, a female Hottentot, and a Javanese. The square form seems most common among the nations with pyramidal skulls, commonly ranked as Mongolian; but it occurs also in Europeans and in the mixed race of Mestizos. The oblong or wedge-shaped pelvis is most common amongst the African races, and is conformable to the elongated shape usually possessed by their crania; but it has been observed also among Europeans and Botocudos.—Thus even from the very imperfect materials yet collected, it may be deduced with

certainty that the form of the pelvis affords no character of sufficient constancy to establish well-marked distinctions amongst the races of mankind.

Other characters which have been at different times adduced, as showing that the Negro and other degraded races are really to be considered as forming a distinct group, intermediate between the higher specimens of humanity and the superior apes, are found on close examination to be equally insufficient. Well-marked differences undoubtedly present themselves when we compare the average of one race with the average of another; but *individuals* are found in every tribe, who in all the particulars in question *pass* the intermediate line, and agree with the tribe which is distinguished from the majority of their own kindred. Thus, in the Negro, the bones of the leg are much more convex in front than in Europeans, so as to give the limb that peculiar shape which is commonly known as the cucumber-shin; and the foot is much less arched, being broad and flat. But the same conformation not unfrequently presents itself in individual Europeans; whilst individuals are occasionally found among the Negroes, whose persons might be taken as models of symmetry and vigour; witness the celebrated athlete, a cast of whose body is conspicuously displayed in the Museum of the College of Surgeons. Again, it was stated by White, and has been generally believed, that the length of the forearm in the Negro is so much greater than in the European, as to constitute a real approximation to the character of the Apes. But an extended comparison proves that only a very slight difference exists between the *average* of each race; and that this difference is by no means greater than those which may be observed on comparing the individuals, of which any single race or nation is composed. On the other hand, a constant and decided difference exists in this particular between Man and the highest Apes; for whilst the hands of man do not hang, when he is erect, below the middle of the thighs, (except in cases of absolute deformity, as from curvature of the spine), those of the chimpanzee extend below the knee-joint, and those of the orang as far as the ankle. So, again, it has been a general opinion since the time of Soemmering, that the head of the Negro is placed so much further back on the vertebral column, as to occasion a material difference in the figure of the whole body, causing an approximation to the posture of the lower animals, in which the foramen magnum is placed considerably behind the centre of gravity. The posterior situation of the foramen magnum, however, in the Negro skull, as compared with that of the European, is more apparent than real; being chiefly dependent upon the projection of the upper jaw, which adds to the portion of the cranium in front of the aperture, whilst its position in reference to the cranial cavity has undergone no change. And we meet with a most decided alteration in the site of this aperture, when we examine it in the adult orang or chimpanzee; although in the young of these Apes it is not so apparent.

We have now brought under examination every one of the characters which have been relied upon by those naturalists, who would divide mankind into a certain number of races, having had distinct origins, and continuing to be separated by well-marked physical characters. We have shown that neither the colour of the skin, the colour or texture of the hair, the shape of the skull, the conformation of the features, or the

general configuration of the body, whether considered singly or in combination, afford characters of sufficient fixity to be employed, according to the canons of Natural History, as valid specific distinctions. We have seen that they are all liable to be affected by the influence of external conditions, especially if these influences are prolonged through successive generations; and that whatever is not capable of being accounted for in this manner, may be legitimately set down to that tendency to spontaneous variation in the human race, of which almost any family of half a dozen children will afford abundant illustration. We consider that the investigation, of which we have thus given a sketch, leaves those upholders of the diverse origin of the human races, who contend for the fixity of the characters at present possessed by them, without the least foundation for their argument; and affords strong *negative* testimony in favour of the community, or at least of the similarity of their parentage. For the *positive* argument on the same side, together with a brief account of the principal groups into which the human family is distributed by Dr. Prichard, we must refer our readers to our next Number; in which, too, we shall say something of the distinctive characters of the two works at the head of our article, and shall endeavour to express our sense of the vast obligations under which we conceive that both science and philanthropy have been laid by the persevering devotion manifested by Dr. Prichard through his entire professional life, to this one great object; than which nothing can well be conceived to be less remunerative, either directly or indirectly, when weighed in that commercial balance by which we are too much accustomed to estimate the merit of our pursuits.

ART. V.

Recherches de Pathologie Comparée. Par CH. F. HEUSINGER.—*Cassel*, 1845.

Researches in Comparative Pathology. By C. F. HEUSINGER.—*Cassel*, 1845. 4to, pp. 155, DXLIX.

IT affords us much pleasure to observe the advance that is being made in the study of comparative pathology. There can be but one opinion as to the importance of the subject, and the advantage to be derived by human pathology from a careful study of the diseases of animals; and when we see such men as Andral and Rayer turning their attention to it in good earnest, we have reason to expect that medical science will profit by their labours. The important researches of M. Andral on the blood of animals are already known to our readers. M. Rayer is at present publishing a Journal of Comparative Pathology: we notice occasionally, especially in the foreign medical journals, excellent articles on the same subject; and now it is our pleasing duty to introduce to the notice of our readers M. Heusinger's very valuable contribution to comparative pathology. This is, indeed, a remarkable production, the fruit of long and laborious literary research and of considerable practical observation and experience in the diseases of the lower animals. The title of the work but faintly indicates the nature and value of the contents; for it is not simply a treatise on those maladies which are common to man and the

domestic animals, but contains in addition, an elaborate account of veterinary medicine from the earliest period down to the present; a history of the great epidemics of Europe, with valuable remarks; and a memoir on the diseases of birds: showing, as the author justly observes in his preface, that the monograph contains matter of interest not only to the physician and veterinarian, but also to the historian and student of natural history.

The physical life of man, says M. Heusinger, having the same conditions and exhibiting the same general phenomena as that of animals, must also present the same resemblance in the state of disease. We may, therefore, very naturally ask, what influence has the medicine of animals upon that of man; and what influence ought it to have? Such is the text with which the author sets out, and all his subsequent observations have for their object the elucidation of the interesting questions involved in it. Although it is the special object of one division of the work, ("Sketch of comparative nosography of man and of domestic animals,") to prove that very nearly the same diseases occur in veterinary as in human medicine; and that, therefore, the study of the one is calculated to throw much light upon that of the other; the author keeps the same object steadily in view in those parts of his memoir not so immediately connected with human medicine, and especially in his historical details; thus imparting a character of unity and closeness to the work which renders the subject easy to the student.

M. Heusinger proposes to discuss his subjects in the following order:

1. By comparing the development and progress of human and of veterinary medicine.

2. By comparing the diseases of man and those to which animals are subject.

3. By comparing the conditions and phenomena which accompany the development of disease in man and animals.

It would be impossible to embrace within the limits of an article suited to the space of this journal, an analysis of the different subjects here enumerated; we shall therefore, for the present, confine ourselves to one, namely, the "comparative nosography of man and animals," which we select as being the most immediately connected with human pathology.

The author requests the readers to bear in mind, while perusing this section of his work, that as yet we have no comparative nosography, and that we cannot form a complete one while the diseases of animals are so imperfectly understood and so ill-defined, even by veterinary writers. His object in drawing up the following "sketch" is to encourage others to pursue the inquiry further, and to throw out a few hints, as he moderately calls his valuable researches, which may be of use to his successors in their inquiries into the same interesting and unexplored field of science.

DIGESTIVE APPARATUS. *Diseases of the mouth.* The domestic animals are subject to several varieties of aphtha, more especially to an epizootic form, called the aphthongular disease. This malady is very common amongst the ruminants, but rarely attacks dogs, horses, and swine; it is contagious, and sometimes attacks the human species. A disease similar to stomatitis gangrenosa in man frequently attacks the lower animals, especially hogs. The tongue is the part principally involved, and when the disease occurs in an epizootic form, as it often does, it is very fatal. It is called by the Italian

writers, "*Mul pinzanese, Cancro volante,*" &c. A disease to which birds are very subject, called anthrax of the gullet and windpipe, closely resembles angina gangrenosa in man. All the varieties of angina are frequently met with in the domestic animals. Cancer of the mouth, lips, or tongue is a very rare disease in animals, the horse alone being subject to it, and even he but seldom.

Diseases of the stomach. Pneumatosis or *tympanitis* is a common and very dangerous complaint amongst oxen, sheep, and goats. It is produced by eating freely of young grass and clover, the result of which is the formation of an immense quantity of gas in the first stomach, containing besides carbonic acid so much hydrogen and carburetted hydrogen, that it ignites instantly when brought in contact with a lighted taper.*

Vomiting, which may be regarded as a physiological act in the dog and in swine, is an indication of some serious disease of the stomach in ruminants, and in the horse it is almost always a mortal sign. Hæmatemesis is also observed in the same classes of animals, but less frequently in the two latter than the former. Ructitation is a common affection in horses, and, as in man, is generally a symptom of indigestion, or of organic disease of the stomach or intestinal canal. *Gastritis* is a very common disease amongst all domestic animals. Dogs are very subject to *gastritis mucosa* and often die of it. Horses, and especially ruminants, are frequently attacked by a dangerous form of *gastro-enteritis mucosa*, which is supposed to be produced by the use of wild or forest pasturage. The Germans call it wood-ill—"*waldkrankheit.*"

Scirrhus and cancer of the stomach are often met with in the horse, and sometimes in the ox. Perforation of the stomach by ulceration and gangrene occurs in domestic animals as well as in man; but rupture of the stomach, by mechanical causes, and without preceding disease, is a lesion almost confined to the horse and the ass. It is, however, sometimes the sequel of tympanitis in the ruminants. Calculi of considerable size have been found in the stomach of the dog, and even of the horse: their structure is not described. Intestinal worms are often found in the stomach of the lower animals, and sheep in particular suffer much from this accident.

Diseases of the small intestines. The different forms of enteritis are commonly met with in all the domestic animals, and they are very often complicated with gastritis. These animals are more subject to worms than man. In the small intestines of the horse they are not unfrequently found in immense numbers.

Diseases of the large intestines. The young of animals are subject to diarrhœa during the process of dentition precisely as children are. Dysentery is very common amongst domestic animals, both in an enzootic and epizootic form. In warm countries it is very fatal to animals as well as to man. Torsion of the intestines is very common and very fatal in some of the lower animals; it generally occurs in the cæcum and colon. Rupture of the large intestines, produced by constipation or other obstruc-

* We have seen several cases of this disease in the cow, in which it was supposed to be produced by the immoderate use of raw potatoes and turnips. The beast was immensely swollen, and, on the side being punctured, a stream of gas issued from the aperture, which readily ignited, and burned with a blue flame for a considerable time.—REV.

tion of the bowels, is by no means an unfrequent occurrence in the horse and ox. Prolapsus and fistula ani are almost as commonly met with in the dog and horse as in man. The large as well as the small intestines are very subject to worms, and they exist in enormous quantities.

LESIONS OF THE LYMPHATIC SYSTEM. M. Heusinger's observations lead him to the conclusion that young animals are as liable to disease of the lymphatic system as those of the human species,—especially the horse, in which this morbid condition easily degenerates into a general cachectic state of the system, in the event of which a simple cold or superficial ulceration will quickly merge into glanders or farcy. M. Heusinger justly remarks, that many obscure points in human pathology might be materially cleared up by the study and comparison of these morbid states in the two races.

Diseases of the liver and bile. Dogs are often attacked by chronic hepatitis, and sheep and oxen by an acute form of the disease, with ramollissement of the liver, which is a dangerous if not fatal malady. The same parasites which are found in the human liver are also commonly met with in the hepatic ducts of animals. The echinococcus and cysticercus are frequently found in the liver of ruminants and in swine. The disease called cirrhosis has not been observed, although M. Heusinger has no doubt of its existence.

Inflammation of the biliary ducts—a common disease amongst the ruminants—frequently terminates in a remarkable kind of ossification of these vessels. Jaundice is a common disease amongst dogs and sheep; the horse is rarely attacked. Biliary calculi are not so frequently found in animals as in man; in oxen and swine they are sometimes met with.

VASCULAR SYSTEM. Traumatic phlebitis, and that variety which is the result of the absorption of pus, are often discovered. Rupture of the veins, especially those near the surface, and ossification, also occur in the domestic animals; the arteries are seldom attacked by inflammation. Ossification is a rare occurrence; and aneurism is confined to the large trunks, particularly to those in the vicinity of the heart. Diseases of the heart are by no means uncommon in animals. Pericarditis is often met with: and there is a peculiar form of this disease, caused by pins or nails which the animal has swallowed, passing from the stomach through the diaphragm into the heart; this is generally mortal. Hydrops pericardii is a common affection amongst sheep and dogs. Endocarditis occurs in animals in connexion with articular rheumatism. Softening of the heart, and that condition commonly observed after typhoid fever in man are often found in our domestic animals. Hydatids, and even worms, cysticercus and strongylus, have been found in the heart of animals.

RESPIRATORY ORGANS. In all acute diseases of dogs the respiratory organs are sure to be attacked. Pleuritis, hydrops pleuræ, and pseudomorphosis, are very common, particularly in dogs and sheep. Acute and subacute pneumonia and pleuropneumonia, emphysema, and œdema pulmonum, are constant diseases. Tubercles of the lungs are common to all domestic animals, (but the author does not say whether the structure and composition of these degenerations are similar to those observed in phthisis pulmonalis in man). Gangrene of the lungs is much more common in animals than in man. Hydatids and parasites are constantly found in the lungs and air-passages of the domestic animals.

The catarrhal affections of animals present a striking analogy to those diseases in the human subject. They assume, however, in each species of animals certain peculiar and characteristic features. In the horse, the lymphatic ganglions are almost immediately inflamed and subsequently engorged, in which state the disease rapidly passes into glanders. In oxen, common catarrh is very liable to terminate in an ulcerated and gangrenous state of the parts, which is exceedingly dangerous. In sheep, this complaint often assumes a cachectic and even a putrid form, which the Germans call glanders of sheep. When catarrh attacks dogs, it has great tendency to become complicated with a peculiar nervous and atoxic state, which resembles the disease in the sheep. Cats are often similarly attacked.

The catarrhal affections in animals have a great tendency to become contagious, to pass from one species of animals to another, and to assume an epizootic form. For example, horses are frequently attacked by a disease very similar to the influenza in man, when that disease exists in an epidemic form amongst the human race.

THE SKIN. The great distinction which exists between the structure of the skin of animals and that of man, must necessarily cause some difference in the diseases of this tissue. The great mobility of the skin in the lower animals, the development of the cutaneous muscle, the existence of large quantities of cellular tissue, and of an exaggerated lymphatic system, under the skin, will explain the cause of these variations.

To understand the cutaneous diseases of animals, the veterinarian must be familiar with these affections in the human subject, and he who describes correctly the development and metamorphoses of a single disease, has done more for this branch of pathology than if he had invented a dozen new classifications.

Eczema is a common disease of sheep, although not described by authors. Under the name of herpes, many diseases have been described which have no connexion with it. True herpes, however, does occur in the horse and dog, and complicated with eczema in the sheep. The *bullæ* frequently attack animals, although not mentioned by writers, and one variety (a kind of rupia), is very troublesome to the horse, about the fetlock. Impetiginous eruptions frequently occur on the face of young animals; but they are so imperfectly described by veterinary writers that it is impossible to give their exact names. For example, it is difficult to tell whether the tinea contagiosa of cats is a porrigo or an impetigo. The porrigos are very common affections. A form of crusta lactea, similar to that observed in man, attacks calves and lambs, and is included amongst these eruptions. Porrigo of the feet is similar, if not identical, to the "grease" of veterinary writers, and the "mauke" and "eaux aux jambes" of the German and French authors; but so many diseases have been described and confounded under this term, that it is not easily individualized. It corresponds to the impetigo sparsa of man more than to any other disease. Favus has not been sufficiently clearly observed in animals. MM. Haubener and Greve, however, describe under the name of "porrigo decalvans equorum" an eruption similar to that which occurs in man; but the description is by no means clear. Variola is a common disease of animals. It does not, says M. Heusinger, originate with animals, but is transmitted to them from man; it passes readily from one

animal, to another and from animals to man, and *vice versâ*, and the more severe the disease the greater is the guarantee against a second attack. When well developed it has the strongest resemblance to the variola of the human subject; but, as in man, it often occurs in a transitory form, with slight febrile symptoms, and without eruption; also in the form of variola varicella; and there is even a species of varioloid which attacks sheep. It is highly contagious, and has been observed in new-born lambs which had contracted it in the womb. Sheep, dogs, rabbits, and goats are subject to variola. The variola of hogs is well known; it occurs in an epizootic form when the disease prevails as an epidemic in the human race. M. Heusinger entertains the opinion that the vaccine disease (*vaccinia*) was first produced by the transmission of variola, either immediately from man or medially through animals to the cow, and that it did not originate in an epizootic form, and without external contact, as alleged by many writers.

Malignant pustule, or anthrax epizooticus, is one of the most common diseases. It attacks quadrupeds, fowls, fish, all the domestic animals, and is sporadic and highly contagious. This disease originates in animals of the equine species, and is transmissible to man. Its elementary nature is unknown, but is supposed to be decomposition or a vitiated state of the blood *sui generis*.

The squamous eruptions are often confounded in animals as in man, with the desquamation which occurs in eczema and other of the exanthematous affections. A mild form of pityriasis exists in animals in the healthy state; but it also occurs in an exaggerated form on a red base, constituting a disease of very frequent occurrence amongst horses and swine. Psoriasis occurs in several forms. In the horse, P. ophthalmica, P. podicis, P. pudendi, have been often observed; but more frequently, psoriasis labialis (the *crusta labialis* of some writers) and psoriasis of the joints and extremities. Lepra has not been described, although several diseases have been mistaken for it.

URINARY ORGANS. Nephritis, although not generally known, occurs in the horse; and M. Heusinger is of opinion that the albuminous disease also attacks that animal, although hitherto it has passed unnoticed.* Inflammation of the mucous lining of the bladder, and vesical catarrh are common affections of animals; the former occurs frequently in an epizootic form amongst dogs. Rupture of the bladder occurs sometimes in the horse. Hydruria is a frequent disease of the horse, and occurs not unfrequently amongst the ruminants. The majority of cases of diabetes of the horse are cases of hydruria; but our knowledge of the chemical analysis of the secretion is too imperfect to enable us to say whether or not it corresponds to diabetes melitus in man. M. Lassaigne states, in an account of an epizootic of this malady, which occurred at Paris, that he detected in the secretion a large quantity of acetic acid partly free, which is rather remarkable. This epizootic attacked almost exclusively entire horses, seldom those that were castrated, and not a single instance is recorded in which mares were attacked. In man, diabetes melitus rarely attacks the female sex, and seldom before the age of puberty. In hydruria of the horse, exactly the reverse occurs. Hydruria in man

* Messrs. Clayworth and Percivall have described "albuminuria" as a disease of the horse, in the 'Veterinarian' for 1841.—REV.

occurs as often in the female as in the male. This is a subject of considerable interest as regards human pathology. Hematuria sometimes attacks dogs, more frequently horses and swine; but sheep and cows, amongst which it occurs in an enzootic and epizootic form, are still more frequently attacked. Lithiasis of the kidneys and bladder are common diseases amongst our domestic animals. M. Heusinger has collected many cases of this malady, which he himself had observed; but his analysis of them is not sufficiently complete to enable him to arrive at general conclusions. He has observed copious sediment so frequently in the bladder of several of the ruminants, that he regards it as a natural condition in those animals. Ischuria, dysuria, and eneuresis are often met with in the horse and dog, and the former with the same characters as those it presents in man.

SEXUAL ORGANS. Sarcocele and scirrhus of the testicle have been frequently observed in dogs, horses, asses, and swine. Varicocele occurs only in the horse, and hydrocele rarely attacks any of the domestic animals. The prostate is sometimes hypertrophied and scirrhous in the dog. Blennorrhea, and cancer, and scirrhus of the penis have been observed in horses and dogs; and phimosis and paraphimosis in the horse, the dog, and the bull. Inflammation of the uterus occurs more commonly in mares and cows, than in women, and is accompanied by a mucous, muco-purulent, or purulent discharge, forming a kind of dropsy of the womb. Puerperal inflammation (*metritis puerperalis*) is also more common in the domestic animals than in women. Polypus of the uterus is often met with in mares and cows, scirrhus and cancer rarely; they have been observed, however, in cows and bitches. Monkeys, which sometimes show a discharge similar to menstruation in women, are subject to attacks of uterine hemorrhage. Uterine hemorrhage of a different character occurs in dogs, cows, and sheep. Prolapsus of the uterus is common amongst all domestic animals. Leucorrhœa is a frequent disease of cows, mares, and dogs. Scirrhus and carcinoma are as common in animals as in women. They occur perhaps more frequently in bitches than in any other animals.

OSSEOUS SYSTEM AND JOINTS. Periostitis and ostitis are very frequent diseases. Osteophyta have been observed in most of the domestic animals. They are very common amongst poultry; they are very frequently met with on the dorsal vertebra of oxen and horses, and also on the feet of the latter. Hydrops bursæ is common to all the domestic animals, and it occurs more frequently in the horse than in man. The small free cartilaginous bodies (*chondroides*) which are sometimes but rarely found in these tumours in man, are very frequently met with in the same tumours in the horse, and especially in the chamois. Arthritis is a very common affection of the horse, and is not unfrequent amongst the other domestic animals. Anchylosis has been observed in almost all the articulations of the domestic animals. In the horse especially, it is much more common than in man.

CACHECTIC DISEASES. The cachectic diseases of animals are in several respects very different from those of man; but still there is considerable resemblance between many of the diseases individually, and no doubt a more profound study of their nature and character in both classes of animals would disclose a much stronger analogy than that which we are

now enabled to point out. Dropsical diseases (*cachexiæ aquosæ*) are common to all domestic animals, and are frequently met with in savage nature. They are often observed, at the same time and in the same place, in the endemic and enzootic, epidemic and epizootic forms. Some animals, however, are much more commonly attacked than others.

Scrofulous diseases occur in all animals, not only in quadrupeds but in birds, and under the same conditions and influences as in the human subject. They are generally enzootic, frequently hereditary, and occur in herbivorous animals more commonly than in the carnivori. They assume the same forms in animals as in man. Simple scrofula is a more common disease of animals than in the human subject, although this fact has escaped the observation of physicians. A very interesting and instructive article on the transition of the various forms of the scrofulous disease has been published by M. Erdt (Gurlt und Hertwig Magazin, vi, p. 292). A flock of 350 lambs were kept in a close and confined stable, badly ventilated, and were fed on very rich and succulent food; presently evident signs of scrofula were manifested, and as soon as these were observed the lambs were suddenly removed to a colder habitation, and put upon a less nutritious diet; the external symptoms vanished, but were immediately succeeded by a copious salivary discharge, atrophy, and rachitis, under which the animals died.

Each class of animals has a predisposition for certain forms of the disease; thus, in the horse the pituitary membrane is the part attacked; in the sheep the disease manifests itself in the form of rickets; in the calf it is confined to the skin, and in swine it almost invariably attacks the glandular system. It is contagious in animals. (?) Catarrhal affections have a great tendency to degenerate into scrofula of the glandular system, especially in fowls. Rachitis is a very common disease in quadrupeds, and even in birds. It is, in animals as in man, a disease of fœtal life or of the early periods of existence. The dog, for example, is frequently born with the disease, or contracts it immediately after birth, and according to the opinion of many naturalists, all the smaller breeds of the canine species, as terriers, poodles, &c., owe their dwarfish size to an hereditary rachitis.

M. Heusinger next proceeds to describe glanders and farcy; but as the author has nothing new to offer on the subject, and as we have discussed these diseases at length in a recent number of this Journal (British and Foreign Medical Review, XLIII), we shall pass on to the cachectic maladies which follow. Hemorrhoids have been described by many authors as a common disease of animals, but the author thinks that hemorrhagic discharges by the rectum from other sources (tumours, &c.) have been mistaken for this, as he himself has only observed it in the carnivori. Toggia, an Italian writer, however, describes it as a frequent disease amongst oxen.

The early writers describe arthritis as a very common affection of our domestic animals; but in a great many instances they evidently confound rickety affections with articular diseases, especially in the cases of lambs and calves. The dog, however, is really attacked by the "gouty disease" (which term is evidently, however improperly, meant to include articular rheumatism). Rheumatic diseases are extremely frequent amongst all the

domestic animals. The most common form of rheumatism in animals, and particularly in horses, is the disease called "founder," *la fourbure* of the French writers. Veterinarians recognise two varieties, the rheumatic and the inflammatory; but the former is merely simple rheumatism, and the latter an inflammatory form of that disease. Rheumatism of the joints and vertebræ is often the cause of exostosis and anchylosis in horses and oxen. Dogs are particularly subject to rheumatism of the spinal column, which generally terminates in permanent paralysis of the posterior extremities. According to M. Huzard, scurvy is a common affection in dogs and other of the carnivori, when these animals are fed almost exclusively on meat and are not exercised. The author has not observed this disease, and is doubtful of its existence.

Cachectic maladies of a septic tendency are, according to M. Heusinger, much more frequent amongst herbivorous animals than amongst the carnivori and man. The "*fièvre charbonneuse*" of animals corresponds in some sort to the typhoid fevers in the human species, and although the former are not subject to the depressing passions and nervous shock or weakness which so often causes the disease in man, they are nevertheless incapable of resisting many atmospheric influences, as negative electricity, excessive heat or cold, marsh miasma, &c.

All our domestic animals are subject to contagious discharges, vegetations, and ulcerations of the genital organs, which are transmissible by coition, and exhibit a resemblance to syphilitic affections. In the author's opinion these diseases are nearly in the same condition as the syphilitic affections of man were before the close of the fifteenth century. M. Heusinger discards the opinion of several writers, that the disease has been originally propagated from the human species to the brute. Carcinomatous diseases are not so common in animals as in man.

NERVOUS SYSTEM. *Diseases of the brain.* Serous apoplexy and chronic hydrocephalus have been observed in animals, but the latter much more frequently than the former. Pseudo-morphosis of the brain is much more common than the preceding, especially the transformation of a new product, which consists of a kind of cellular and cartilaginous tissue, into a state of ossification. This condition has been erroneously described by authors under the name of enchondroma. Hydatids are frequently met with. The *cysticercus* is found oftener in the brain of the hog than in that of any other of the domestic animals. The *cœnurus cerebralis* is very common in the sheep, less so in the goat, and rarely found in horses and oxen. Congestion and hemorrhage of the brain occur frequently in animals. M. Heusinger has even observed the latter affection in birds. Hemorrhage of the brain, however, is not so common as it is in man, which circumstance is no doubt mainly owing to the more natural life which animals lead. Nervous apoplexy also occurs in the brute, but much more rarely than in man.

Diseases of the spinal marrow. These complaints are extremely frequent amongst our domestic animals. Inflammation of the membranes is very common. Dropsy of the spinal canal is more frequent amongst the domestic animals than in man, and is often a congenital disease in lambs. Since the beginning of the present century a kind of plague has been frequently observed amongst sheep, known under the name of the trembling

disease (*maladie tremblante*), which is the result of the pathological condition just described. Effusion of matter is a frequent occurrence in horses, and is often fatal. The author has observed the case of a mare who died suddenly from the effusion of pus from a lumbar abscess into the spinal canal. Atrophy of the spinal marrow occurs frequently in horses, dogs, and sheep. Inflammatory softening of its substance is very common in all our domestic animals.

M. Dupuy thinks he has observed the roaring disease, "*cornage*," in horses, in which the recurrent nerves were compressed by indurated lymphatic ganglions. Mr. Ferguson, of Dublin, has observed the same condition. This disease resembles *laryngismus stridulus* in man. Paralysis is very common in animals. There is a singular epizootic disease of cattle of this kind, called by the Germans *sterzwurm*, which appears to be unknown to veterinarians of the present day. It generally terminates in gangrene. Trismus and tetanus are singularly common affections in horses, but rarely occur in the other domestic animals. The nature of these diseases is as obscure in the brute as in the human subject. Convulsions and epilepsy occur frequently in dogs, cats, and birds. Chorea has been observed in dogs. Canine madness (*rabies canina*) originates in the dog species, and also, according to some writers, in the cat. It has been observed as an original disease in the dog, the wolf, the fox, and jackal. It is sporadic and epizootic, and may be transmitted to all quadrupeds, to birds, and man, but its transmissibility from animals in which it was not an original disease is, according to M. Heusinger, very problematical.

Mental diseases occur in animals as well as in man, according to the author, who is supported in this view by M. Tscheulin, a German writer on the nervous diseases of animals. M. Heusinger believes that "instinct" depends for its healthy existence upon the organization of the nervous system, and, as that organization may be disturbed or deranged, it follows as a corollary that derangement of the instinct, or mental disease, will follow. The author considers that some light might be thrown on the *morbi mentis* in man by the study and observation of these melancholy disorders in the brute. The *furor uterinus* and the mania for destroying their young are subjects worthy of the special attention of the comparative pathologist.

Diseases of the eye in animals have been so ill-described by veterinarians, that the author complains he cannot even give a complete compilation of them. The same may be said of diseases of the ear, which is the last subject described in the 'Comparative Nosography.'

We have now completed our analysis of M. Heusinger's memoir. The original, being in a tabular form, loses much of its force in being transposed into consecutive sentences, and our abstract is of necessity abrupt in many places; but still the substance and meaning of the author are preserved throughout, and the reader is furnished with a number of facts, as interesting and important as they are unique.

Art. VI.

Denkwürdigkeiten zur medicinischen Statistik und Staatsarzneikunde für Criminalisten und Aertze. Von Dr. J. L. Casper, Professor an der Friedrich-Wilhelm's Universität., &c. &c.—*Berlin*, 1846.

Essays on Medical Statistics and Medical Jurisprudence, adapted for the use of Jurists and Medical Practitioners. By Dr. J. L. Casper, Professor in the University of Berlin, &c. &c.—*Berlin*, 1846. Imp. 8vo, pp. 397.

This is a remarkable work from the pen of a man who, by his extensive and elaborate contributions to medical literature, has long since deservedly acquired a European reputation. We have here *in extenso* the philosophy of medicine, dissertations upon subjects of vast importance to man in a social condition,—requiring enlarged experience, immense research, and a profound knowledge of physics and ethics for their proper elucidation. We know no one so well fitted as Professor Casper to undertake a work of this kind, involving as it does immense labour and requiring a mind well disciplined by the experience of years devoted to the study of medical philosophy in its higher and more recondite ramifications: and although some may be disposed to differ from the author in his conclusions, and to dispute the practical utility of his researches, there is no one, we think, who will refuse to admit that he deals with his subject fairly, candidly, and with an earnest desire to improve medical science and practice.

This treatise is divided into several sections, containing memoirs or essays upon subjects closely related to medical statistics and legal medicine. Some of these have already appeared in a more concise form, as monographs or contributions to the periodicals of the day:—they are here collected, arranged, and condensed; and those additions have been made which the recent progress of medical science had rendered necessary.

The first Essay is on the *Influence of the Weather on Health and Life:*—the second refers to *Experiments and Observations on the Mark produced by the Cord in Strangulation; and on the Signs of Death by Hanging:*—the third, on the *Geography of Crimes:*—the fourth, on the *Biography of a Fixed Idea:*—the fifth, on the *Mortality of the Prussian Army:* the sixth, on the *Influence of the Periods of the Day on Births and Deaths:*—and the seventh and last, on the *Phantom of a so-called Pyromania.*

The reader will not fail to be struck with the singularity of the titles of these essays, as well as with the novel and remarkable views which, when properly treated, they must necessarily lay open. To the English reader the titles will present a certain quaintness of which, without altering considerably the meaning of the author, they can hardly be deprived. We shall, however, endeavour hereafter to make good, by a copious analysis of the author's facts, any defect which may arise from an insufficient designation of the subjects of the essays.

From the preface to this work we learn that the facts here accumulated by Dr. Casper, and upon which his conclusions are based, have been collected by years of unwearied labour, while he was engaged in duties which

enabled him to have free access to official, and therefore authentic documents. He candidly admits that the problems which he has here attempted to solve, are so wide in extent, that his views must necessarily be in many respects imperfect. This remark particularly applies to the Essays on the "Influence of the weather on health;"—of the "Periods of the day on births and deaths," and the "Geography of crimes." The author's conclusions are here based on statistics, and if they cannot be received as perfectly satisfactory, he will be quite contented with the admission that he has at least succeeded in pointing out the way for future inquirers. In respect to pyromania, he entirely denies its existence as a ground of irresponsibility in charges of incendiarism, thus differing from nearly all modern writers on the subject of insanity. We have considered it only just to Dr. Casper to put these preliminary remarks on record, in order that our readers may see how little he is disposed to dogmatise, or to claim infallibility for his opinions.

I. ON THE INFLUENCE OF THE WEATHER ON HEALTH AND LIFE.

Influence of Season. While all are ready to admit that Health and Life are materially affected by changes in the weather, the most vague and erroneous ideas prevail even among professional men respecting the kind of influence exercised. So far back as the time of Hippocrates, it was well known that the mortality from disease varied materially at different seasons of the year. In his third aphorism this ancient observer remarks, that Spring is the most healthy, "*Saluberrimum et minimè exitiale,*" and Autumn the most unhealthy season, (*per autumnum* morbi acutissimi et exitiosissimi omnino.") Celsus adopted the view of Hippocrates. In modern times, by the use of instruments, and by the aid of tables accurately kept, many eminent statisticians have attempted to determine the degree and kind of influence which the seasons exercise upon health. Dr. Casper refers particularly to the researches of Villermé, Milne Edwards, Quetelet, Buek, and other well known statistical writers. But their researches are exceedingly defective: thus Quetelet's work refers only to the numerical deaths in a population,—the diseases which affect the living are entirely overlooked! Buek's observations are too limited to allow of any satisfactory inferences. Among the numerous difficulties which interfere with the settlement of a question of this kind, is the fact that habits and modes of life vary to such an infinite extent in different countries, that the conclusions drawn even from large numbers of instances, can have only a very limited application. Other circumstances, even in a single nation, must materially affect the results. The health of a community is liable to be affected by the temperature, elasticity, and hygrometric state of the atmosphere; and how widely different are these conditions in a crowded city and in the open country, among pauper and wealthy populations, and in manufacturing and agricultural districts!

It by no means follows that the facts which are required for the solution of this problem, can be deduced from the reports of medical practitioners in general; although this is the principle upon which tables of health and mortality are commonly founded. How many diseases (observes our author) attack individuals and disappear without medical aid being called for! What errors would have been committed in reference to the sanitary state of Berlin during the prevalence of the two influenza-epidemics, if this

had been determined by reference to that portion of the population which sought medical advice, compared with the immense number of individuals who received no medical attendance whatever!

As an important and satisfactory basis for calculations of this kind, our author refers to the official reports of hospitals and of infirmaries for the poor, where these are situated amidst large populations. The influence of the weather and seasons on health can here be appreciated on an extended scale. Dr. Casper presents us with the results of observations made on upwards of 155,000 recent cases of disease occurring in public medical practice among the poor of Berlin, including the receptions into the Charity Hospital. These observations extend over a period of seven years (1833-9):—the cases for each month and season being separately numbered so as to allow of a fair comparison. He says, and we believe with perfect truth, that no table so extensive as this has ever before been published on such a subject. For the table we must refer to the original work. We can here give only a summary of the results obtained. The numbers of sick paupers, including those received into the principal hospital of Berlin in the different seasons of the seven years, were respectively in—

Summer	40,700
Winter	39,024
Autumn	37,865
Spring	37,748
Total	155,337

From this table we learn that in Berlin the greatest number of cases of disease occur in the summer, and the smallest number in winter. Spring and autumn are nearly equally balanced. If, therefore, in speaking of the unhealthiness of seasons, we mean to imply those periods in which *sickness is most prevalent*, it is obvious from this table, that the summer must be regarded as the most unhealthy. These observations, it is true, are confined to Berlin; but it is by no means improbable, as Casper observes, that the results would be found to be similar, not only in North Germany, but in Central Europe generally. If, on the other hand, the unhealthiness of seasons is to be measured by the relative amount of mortality in a population, then the result, as we shall presently see, will be entirely different; for under these circumstances the summer is, beyond all question, the most healthy. The difference which is here pointed out by our author is of some practical importance. The only rule for estimating the state of public health to which we have access in this country, is the rate of mortality as indicated by the quarterly and yearly returns of the Registrar-General; but it would appear, from the extensive observations collected by Casper, that when there are the fewest deaths, there is the greatest amount of illness in a given population—a result for which most of our medical readers would probably be little prepared.

Dr. Casper refers to the observations of Cless, at Stuttgardt, as confirmatory of the conclusions which he has drawn respecting the influence of the seasons on health. Cless's observations extend over a period of ten years (1828-37), and they prove incontestably that the cases of sickness are most numerous during the summer season, while they are least numerous in autumn. Local position must of course, to a certain extent, modify the results:—thus there may be in and around certain towns and cities,

endemic causes, the influence of which will necessarily increase the autumnal or vernal cases of sickness.

When we come to test the relative salubrity of the different months of the year by the Berlin observations, we have, according to Casper, the following numbers representing the relative cases of sickness in each month based upon a total of 1000:

December	75·6*	June	83·0
May	77·8	September	83·1
October	80·0	April	83·3
November	80·5	July	85·8
March	81·1	August	93·0
February	82·2	January	93·3†

* Minimum number of Cases. † Maximum number of cases.

Hence it will be seen that the least amount of sickness occurs in December, and the greatest amount in January; the most unhealthy, on this mode of computation, following the most healthy month.

The influence of *season* on public *health* may, however, be more accurately determined by the relative amount of mortality in a given population. There can be no question, that whenever the number of deaths increases, the season must be most unfavorable to health. Another test has been proposed, which, however, is far from satisfactory, and is accordingly rejected by Casper: we allude to that based on the comparison of the births with the deaths. When the births are numerous in proportion to the deaths, this has been assumed to be indicative of a healthy season; but this is a most fallacious criterion, as the proportional numbers are liable to very great fluctuations. The registration of *deaths* appears to be carried on in a very perfect manner at Berlin. Every death is not only verified by a competent medical man, but he is required to fill up, in an official paper, fourteen different forms of inquiry respecting the deceased. These public documents therefore constitute an important basis for calculating the rate of mortality. The author admits, that in reference to pathological inquiries regarding the cause of death, they may be occasionally defective; but in reference to the present subject, the date of the death and the age of the deceased are obtained with sufficient accuracy to allow of a fair application of the results to the determination of the comparative effect of season on mortality.

These mortality tables for Berlin, compiled from official documents, include the deaths of the whole population of that city, as well as of the patients admitted into the Charity Hospital. They extend over a like period of seven years, (1833-9) and comprise 55,609 deaths, classified according to age and the month in which the death occurred. To transfer this most elaborate table to our pages is absolutely impossible. We can only give a summary of the deaths in the months,—the ages being subdivided into twenty-four periods, from 1 to 6 months to from 90 to 100 years.

January	4922	July	4670
February	4196	August	6020 max.
March	4143	September	5633
April	4725	October	4608
May	4338	November	4047 min.
June	4135	December	4172

An extensive table of this kind would convey no useful information,

unless it were accompanied by one which indicated for each month of each year, the respective number of deaths as well as the state of the thermometer, barometer and hygrometer. By the assistance of Professor Müller, Dr. Casper was enabled to construct a weather-table of this description, based on numerous and accurate meteorological observations.

After all, the practical value of these researches must depend on the means which we possess of instituting comparisons with similar tables of health and mortality, drawn up in different countries and cities, upon like accurate principles. The official documents of the French government, on the sanitary state of Paris, published triennially, have been thus employed by Casper, having been reduced to a scale which allows of a ready comparison with the Berlin observations. The Parisian table, which he has inserted in an appendix to his work, comprises 96 months, extending from 1819 to 1826, and includes 189,196 deaths. He has also made use of Emerson's Statistics of Philadelphia, in order to place observations made in this city in comparison with those obtained from the registers of Berlin and Paris. These last comprise 120 months, extending from 1811 to 1820, and include 23,456 deaths.

The following condensed table represents the proportional mortality in different seasons in these three cities compared with the mean temperature. The total deaths were 268,261, but Dr. Casper has reduced these in the table to a standard of 100,000, and has converted the Celsius's and Fahrenheit's to the Reaumur's scale. For the convenience of our readers we have, however, again brought his temperatures to the scale of Fahrenheit, the only modification which we have thought it necessary to make in this interesting table.

	Winter.		Spring.		Summer.		Autumn.	
	Mean Temp.	Deaths.	Mean Temp.	Deaths.	Mean Temp.	Deaths.	Mean Temp.	Deaths.
Berlin . . .	38°·5	24871	47°	24714	65·3	26312 max.	49°·5	24102 min.
Paris . . .	38°	25299	51°	28630 max.	65°	23449	53°	22619 min.
Philadelphia	33°	21158 min.	49°·3	23755	73°	31194 max.	55°	23891

An examination of these results will at once prove that there is no common law of Nature ruling the sanitary influence of the seasons, such as that which was supposed to exist by Hippocrates and Celsus. In not one of these three great cities, is there any similarity in the rate of mortality for different seasons. It will be perceived that the maximum mortality occurred in Berlin and Philadelphia during the *summer*, while in Paris the maximum was in *spring;* then, with regard to the minimum mortality in Berlin and Paris, this occurred during *autumn*,—in Philadelphia during *winter*. Hence, it is impossible to admit the existence of any fixed law with regard to the effect of season on mortality in different populations. As we might have inferred *à priori*, local differences, depend-

ing probably on soil, water, altitude, and the other conditions which give rise to a variety of climate, affect the results; and thus it happens that the season for the maximum of mortality in one place, is precisely that in which the minimum exists in another place.

Dr. Casper next proceeds to contrast the mortality of the present with that of the last century in reference to the influence of season. The comparative tables extend over a period of 150 years, and comprise no less than three millions of deaths. They are derived from an analysis of the mortuary registers of eighteen of the principal cities and towns of Europe. We can here only give a summary of the results, which are as follows:—the maximum and minimum rates of mortality occurred during the above period.

	Maximum.	Minimum.
Winter	8 times	3 times
Spring	12	1
Summer	3	12
Autumn	1	8

When the mortality of the eighteenth century is contrasted with that of the nineteenth century we have the following results:

	18th Century.		19th Century.	
	max.	min.	max.	min.
Winter	4	2	4	1
Spring	1	1	4	0
Summer	1	7	2	5
Autumn	1	4	0	4

From these observations it is clear that, judging from the amount of mortality in a population, the *spring is the most dangerous, and summer is the most favourable season to health.* This conclusion applies equally to observations made on the periodical rate of mortality in the past and present centuries.

It was remarked by Quetelet and Villermé that the maxima and minima, at different periods, varied for the same place. In the whole of the extensive tables, collected by our author, there is, however, only one fact in favour of this view. Thus, with regard to Berlin, the maximum mortality in the eighteenth century occurred during spring, while in the present century it occurs during summer. In the former period the minimum was in winter; it is now in autumn. Villermé endeavoured to explain this by supposing that the deaths did not undergo any marked increase in the most favourable seasons; but that there was an actual decrease of mortality in those seasons, among which the maxima had hitherto fallen. This explanation, however, does not appear satisfactory. We agree with Casper in thinking that, so long as epidemics prevail with variable degrees of mortality at different periods of the year, there must necessarily be great differences in the mortality of the seasons. Some epidemics are peculiar to winter and autumn, others to summer and spring; the shifting of the maximum period of mortality, therefore, is probably due to an influence of this kind exerted unequally over a cycle of years.

Influence of temperature. The influence of season is, however, only one element of this important branch of statistical inquiry. Meteorological observations are required in order to show the precise influence of the weather upon health and life. Among these there is, perhaps, no condition of the atmos-

phere so important as *temperature*. The facts already collected in relation to the number of deaths at observed temperatures, bring us to the following conclusion.—*Extremes of temperature, whether high or low, are eminently destructive to life.* This conclusion applies equally to the four seasons of the year, the mean temperature of each season being, of course, made the standard of comparison. Thus the mean temperature of autumn at Berlin being 49°5 if the thermometer rise to 59°; this must be regarded as a high extreme with respect to that season, although it would be low with respect to summer. It is indeed a well known fact that the extremes of temperature, which occur in different seasons, are exceedingly unfavorable to health, because the human body, which becomes only gradually accustomed to change of season, is very sensitive to sudden changes from heat to cold, or *vice versâ*. We consider it unnecessary to follow Dr. Casper through the minute details into which he enters in proof of this theorem. It appears that, taking 100 deaths in the seven years already referred to in the Berlin table, there were 10·6 for the hottest summer months, while there were only 9·2 in those summers in which the temperature was lower. On the same scale the deaths were for the same period (out of a hundred) 7·5 for the hot months of spring and autumn, but only 6 for these seasons when the temperature was lower. The same fact has been noticed in other localities. Thus, in twelve summer months in Paris, having a mean temperature above 66°, there were 22,471 deaths; while in twelve summer months, of which the mean temperature was below 66°, the deaths were 21,895. Buek remarked that, at Hamburgh, under a mean temperature of from 54° to 66°, the deaths were 8·1; while, with a temperature above 66°, they were 10·1. The same observation was made by Meyer in his table of 19,251 deaths at Dresden. Here as elsewhere the maximum mortality occurred while the thermometer ranged from 86° to 92°. This agreement of results, obtained in widely different localities, proves that the increased mortality cannot, under these circumstances, be ascribed to mere coincidence, but must be a consequence of the high temperature. This inference is supported by the fact that a similar result is observed with the opposite extreme, i. e. of cold. If we refer to the Berlin table, and compare fourteen very cold months, in which the mean temperature was below 34°, with fourteen cold months, in which the temperature was milder, i. e. above 34°, it will be found that the mortality in the latter case was much lower than in the former. The respective numbers were as 8064 to 9248, or as 15·1 to 17·3 per cent. of the total mortality of Berlin. The Parisian table of mortality leads to the same result. Comparing sixteen cold months with a mean temperature below 41° with sixteen other months, in which the mean temperature was above this degree, the deaths were respectively as 32,784 to 29,024, or as a remarkable coincidence with the Berlin table precisely 17·3 to 15·3 per cent. of the total mortality of Paris. The researches of Meyer and Buek also support the view. From Buek's observations, in Hamburgh, it appears that with a temperature of

5°, the daily deaths per cent. were	12·3
23°	11·5
23° to 32°	10·7
32° to 43°	9·3
43° to 54°	8·8

From these facts it is impossible to arrive at any other conclusion than that extremes of temperature, i. e. excessive heat and intense cold are destructive to life. As in regard to all atmospheric influences on life and health, this effect of temperature may be of a twofold character. On the one hand, a fact in accordance with daily medical experience, intense heat and cold may give rise to attacks of acute disease of a speedily fatal character whereby the mortality is increased. On the other hand, these influences may aggravate chronic diseases of long standing, and thereby accelerate death, thus adding to the mortality, when a more lenient temperature would probably have led to a protraction of the fatal result. A high degree of heat or a low degree of cold operates equally in the last-mentioned way; but, as it will be subsequently proved, their operation is very unequal upon individuals of different ages.

The results at which Casper has arrived, with respect to the influence of extremes of temperature on health and life, are such as might have been anticipated. Loose ideas on this subject have, for a long time, prevailed in the profession; but here, for the first time, we meet not merely with a demonstration of the fact, but a mode of determining the amount of influence, from an examination of the extensive tables which he has collected and analysed. The results here given are singularly confirmed by the tables of mortality for this metropolis, weekly issued by the Registrar-general. The effect of intense heat was strikingly shown by the prevalence and mortality attending diarrhœa during the last summer (1846.) For several weeks, during the prevalence of an unusually high temperature, the deaths from this cause among old and young were very numerous; nor, until the thermometer began to fall towards the latter end of August and the beginning of September, did the mortality sink to the average of the season. Other diseases were at the same time aggravated in their characters, and destroyed a greater number of persons than usual.* With respect to the effect of cold we cannot do better than refer to the remarks of the Registrar-general in the return for the winter quarter of 1845.

Influence of pressure. Since the discovery of the pressure of the atmosphere by Torricelli, more than two hundred years ago, it has been pretty generally admitted that variations of the barometer exert an appreciable, and in some instances an important influence on health and life. In spite of the large number of barometrical registers kept in all quarters with great accuracy and regularity, it is, however, not a little singular, as Casper remarks, that no attempt has ever been fairly made to apply these changes in any definitive way to the laws which govern mortality. It has been said, that with a high barometer, attacks of apoplexy are more frequent and more rapidly fatal, and that frequent fluctuations of the mercurial column are injurious to health; but even then common opinions rest upon no well observed data: their correctness yet remains to be tested by facts.

* The summer of 1846 was remarkable for a long continued high temperature. In the week ending August 8, when the mean temperature was 5° above the mean of this the hottest month in the year, the deaths were no less than 237 above the summer average! The same increase of mortality was observed in New York. We extract the following from a medical periodical: "During the hot weather, in one week, in New York, the extraordinary number of 4025 deaths occurred, or more than one in every 1000 inhabitants. Of these, 31 were caused by apoplexy, 21 by sun-stroke, and 52 by cholera infantum. 169 were of children under one year; of married women only 58 died."

According to our author, a high or a low degree of pressure has a more striking and more uniform relation to the rate of mortality, than even a high or low temperature, and the position which he undertakes to establish is this :—*In nearly all the seasons of the year a high atmospheric pressure increases, and a low pressure diminishes the rate of mortality.*

We consider it unnecessary to reprint here the very copious table of barometrical observations for each month of seven years (1833-9), four only being excepted, (Aug. to Nov., 1837), during which the cholera prevailed in Berlin and materially affected the general rate of mortality. To each month is attached the mean pressure compared with an assumed standard, and in another column the number of deaths. According to Casper, the mean pressure at Berlin is 336,000‴,* or 29·83 inches English. In order to divide the 80 months into two equal parts, the standard height, as mean pressure, is assumed to be 336,361‴, or 29·86 inches English, that is, rather lower than the English standard, which is commonly taken at 30, although this number is probably taken rather from convenience than from actual observation.

In forty months with the barometer above 29·86 inches, there died in Berlin 25,221 persons; and in forty months with the barometer below this point, there died 25,021. To give a more correct idea of the proportional rate of mortality, it may be stated that under a high barometer, the monthly deaths were 630·5, (or 18·48 daily); while under a low barometer, the monthly deaths were 625·5, (or only 18·25 daily.) For every 100 who died while the barometer was low, 101·2 died, when it was high.

The destructive influence of a high pressure will become more apparent, if we compare the mortality in the weeks in which there was an extreme high, with that of the weeks in which there was an extreme low pressure. Thus, in thirteen months in which the mean pressure was 338,000‴, (29·98 inches E.), the deaths were 8400, or an average of 646 per month, or 21·54 daily. On the other hand, in the thirteen months during which the average height of the barometer did not exceed 334,800‴, (29·72 inches E.), and was for the most part below this point, the mortality was only 621 per month, or 20·79 daily;—or for every 100 persons who died in the latter case, 103·9 died in the former, a difference sufficiently well marked in favour of a low atmospheric pressure. It becomes an interesting subject to consider whether this influence of pressure is equal at all seasons of the year. Casper's researches show, that so far as it relates to Berlin, there is a variation with the season. We must here again content ourselves with a summary of his results:

High Barometer.	Av. deaths per month.	Low Barometer.	Av. deaths per month.
In 11 winter months . .	656	In 10 winter months .	607; — 49
8 spring „ . .	586	14 spring „ .	679; + 93
14 summer „ . .	627	5 summer „ .	619; — 8
7 autumn „ . .	649	11 autumn „ .	576; — 73

* The author adopts the highly inconvenient practice of reckoning the barometrical heights in lines. A line is equal to ·0888 inch. This, being a circulating decimal, can have no strictly equivalent representative number in fractions. A line is commonly said to be equal to 1-12th of an inch; but this does not quite express the measurement, for a line is rather more than this in length. 336 lines as above would be equal to (·0888 × 336) 29·836 inches English, the mean pressure for Berlin.

Or, to give the results in a more definite form, the deaths were:

	High Bar.		Low Bar.
In Winter	108·0	to	100
Spring	86·3	to	100
Summer	101·3	to	100
Autumn	112·6	to	100

These differences are not merely local;—the same results with slight modifications have been met with in other places, and they all point to one general conclusion, which has been hitherto scarcely noticed, namely that *the influence of atmospheric pressure upon human life is not uniform at all seasons of the year.* That this result, derived from the Berlin tables, is not a mere coincidence, or dependent upon a small number of observations, will be presently made evident by a reference to the Parisian tables. In the mean time, it would be interesting to inquire what are the diseases either of a new or chronic character which add so materially to the mortality under a high barometer during the autumnal season, as well as those which on the other hand have their mortality diminished under a high barometer in spring; but facts are wanting to enable us to solve these questions. In the springs of these years, catarrhal and rheumatic diseases, with and without fever, cholera, hemorrhages, and congestive diseases were observed to prevail. The result proves that the conclusion that the prognosis for such diseases is more favorable under a low atmospheric pressure, is decidedly premature. In Paris, it was remarked that the mortality from disease was diminished during autumn under a moderately high barometrical column. Thus the following were the results with a pressure *above* 755·96 mm. (29·7 inches E.), and with a pressure below this point:

	Bar. above 29·7.		Bar. below 29·7.
In 16 winter months	2021	In 7	1971 — 50
11 spring	2334	13	2129 —142
13 summer	1841	9	1831 — 10
8 autumn	1754	15	1804 + 50

Or,

	High Bar.		Low Bar.
In Winter	102·5	to	100
Spring	106·4	to	100
Summer	100·5	to	100
Autumn	97·2	to	100

With the exception of this slight difference according to the seasons, the prejudicial influence to health of a high barometrical pressure, is as strikingly and as uniformly shown by the Parisian tables as by those of Berlin. We state the facts, but must pass over the series of observations. The Parisian tables prove that, as in Berlin, the deaths under a high, were to those under a low barometric pressure as 101·3 to 100; or as a daily ratio 66·32 to 65·42. A comparison of the extremes establishes with equal clearness that there is a manifest difference on the side of a low barometrical pressure in favour of the health of a population. In twelve months, during which the barometer was moderately high, the monthly deaths in Paris were 2016, and the daily deaths 67·20. In twelve other months, when the barometer was somewhat lower, the monthly deaths were 1985 and the daily deaths 66·17, the ratio in the two periods being 101·5 : 100. The observations made by Meyer, in Dresden, equally

lead to the conclusion that the mean rate of mortality is greater under a high, than under a low barometrical pressure.

It would be interesting to connect, if possible, the diffusion and mortality of epidemic diseases with the variable states of atmospheric pressure; but our knowledge of the causes and essence of these diseases is so limited, as to render it impossible at present to say whether any such influence on their progress does or does not exist. During the prevalence of epidemic cholera in Berlin, extensive observations were made by Casper on the barometer; and the changes in this instrument were compared with the daily fluctuations in the rate of mortality; but nothing certain could be deduced from them. Dr. Prout observed a remarkable change in the state of the atmosphere about the period at which this disease broke out in London. This gentleman states that for more than six weeks previously to the appearance of the cholera in London, he had been daily engaged in performing experiments to determine with accuracy the weight of a given quantity of air. On a particular day, the 9th of February, 1832, the weight of the air suddenly appeared to rise above the usual standard. The rise was at first considered to be due to some accidental error, but a careful repetition of the observations proved that this was not the case. Dr. Prout supposes that the increased weight of the air in London at this time, depended on the diffusion through the lower strata of some heavy gaseous body. About the 9th of February, the wind which had previously been west, veered round to the east, and remained chiefly in that quarter till the end of the month. It was precisely at this time that the first cases of epidemic cholera were reported to have occurred in London, and from that date the disease continued to spread.*

There can be no doubt that the cholera follows certain laws peculiar to itself, a fact which appears to be established by the mode in which its progress and mortality were influenced throughout Europe by local circumstances. Other epidemics of a less violent kind are, however, doubtless, influenced by the meteorological conditions of the atmosphere; but exact observations are wanting to show to what extent this influence operates.

Influence of the hygrometric condition of the atmosphere. In spite of the popular belief to the contrary, Dr. Casper contends that a humid state of the atmosphere is more healthy, and attended with a lower rate of mortality than a dry condition of this medium. The difference, he thinks, is easily explicable by the different nature of the diseases which are liable to be caused by these opposite hygrometric conditions of the air. When there is a surplus of humidity,—catarrhal, rheumatic, and glandular (scrofulous) diseases, with intermittent fevers, prevail. In a given population, there may be therefore much sickness, but the rate of mortality may either not be increased, or only in the very smallest degree. The result is far otherwise with those diseases which are apt to take their origin in a dry state of the atmosphere, as inflammations and hemorrhages, including apoplexy, with their serious sequelæ. Experience is decidedly in favour of this distinction. Thus the Berlin and Parisian tables, which have been already employed to show the influence of pressure, will serve to prove this remarkable influence of the hygrometric condition of the air. In the periods referred to, out of 100 deaths there were—

* Chemistry, Meteorology, and the Function of Digestion, 3d edition, p. 311.

	In Berlin.	In Paris.
During very dry months	52	50·5
In wet months	48	49·5

This comparatively beneficial influence of a humid atmosphere appears to exist equally throughout all seasons of the year, but very wet summers are rare in Middle Europe; hence, in order to obtain a proper comparison of this with other seasons, a much more extensive series of observations would be required than those to which we have at present an opportunity of referring. Making therefore due allowance for this deficiency, the following table represents the comparative rate of mortality during dry and wet seasons in Berlin and Paris. The numbers given under the different seasons are reduced to a standard of 100 deaths:

	Berlin.				Paris.			
	Winter.	Spring.	Summer.	Autumn.	Winter.	Spring.	Summer.	Autumn.
Dry . .	13·5	12·9	13·1	12·4	13·2	14·5	11·7	10·9
Wet . .	11·7	12·4	12·6	11·1	11·6	14·1	11·8	11·9
Difference .	−1·8	−0·5	−0·5	−1·3	−1·6	−0·4	+0·1	+1·0

Admitting that there exists here a difference in favour of the wet seasons, Dr. Casper thinks that the result will be made more obvious by comparing months according to temperature and humidity. The Berlin tables are in this respect imperfect, for the reason that, during the whole period, they present only three humid warm months, a number much too small to allow of the institution of a fair comparison. Our author, therefore, bases his calculations upon the Parisian table, to which the same objection does not apply. This table furnishes us with 10 months which were warm (compared with the mean temperature) and humid; 12 months warm and dry; 14 cold and humid; and 18 cold and dry. If the rate of mortality during these periods be compared with the average mortality for the respective months (as for example, the mortality of a dry and cold January, with the average mortality for this month), we obtain as the result of this extensive series of calculations, the following valuable summary:

Mortality in the months which were		Average mortality of these months.	Proportion.
Humid and warm . .	1853	1842	100·6 : 100
Dry and warm . .	1863	1829	101·8 : 100
Humid and cold . .	1882	1923	97·8 : 100
Dry and cold . .	2029	1986	102·1 : 100

We thus arrive at the conclusion *that a humid state of the atmosphere is more favorable to health and life than that which is dry;* for taking the average mortality in a given population to be 100, the deaths will be 101·9 in dry seasons, and only 99·2 in wet seasons. It is evident, not merely from these but the Berlin observations, that *no state of the atmosphere is so prejudicial to health and life as that of dry cold;* and again, contrary to the opinion of Kopp and others, it is not humid warmth, but humid cold which tends to increase the rate of mortality in a population.

From this review of the influence of the physical changes in our atmosphere upon life and death, the important practical question arises how or in what way these changes lead to the origin, prevalence, and diffusion of various forms of disease. Many difficulties are opposed to the proper solution of this question. In most tables of mortality, especially those of

a public kind, the nomenclature of disease is so uncertain as to render them in this point of view useless. Dr. Casper has, therefore, judiciously confined his analysis to a few diseases, the real description of which there could be no reason to doubt. He has, however, made a further subdivision, selecting from these diseases, those only (forming a small number) the origin and mortality of which are known to be influenced by the physical states of the atmosphere. Further, the diseases have been reduced to classes in order to condense the results. The tables kept in hospitals have been taken in preference to those which were of a public kind; since the diagnosis and description are better given, and the medical officers in these institutions take more pains in keeping an accurate record of facts, than those who are officially employed by government. A valuable table of this kind including the patients received in the hospital and the infirmaries for the poor, in Dresden, has been contributed by Meyer. It extends over a period of ten years (1828-37) and comprises 27,067 cases of disease. Dr. Casper makes use of this table according to the principles already described; we can, however, do no more than advert to its leading features. In the first place, although it differs from the Berlin table in showing that the receptions per 100 during summer were, when compared with those during winter, as 22 : 27, and therefore that the maximum degree of sickness did not exist in Dresden during the summer season; yet the results clearly corroborate the inference already drawn, that the minimum mortality occurs during summer, and the maximum during spring. The respiratory organs are proved to be especially liable to dangerous attacks during spring. One third of all the cases of inflammatory diseases of the chest collected by Meyer (360 : 1092=32·9 : 100), occurred during the vernal months. The per centage of cases of disease of these organs was during summer only 18·2; but in the winter months, it rose as high as 25·7. Again, the maximum number of cases of consumption occurred in spring, 29·9 : 100; and the minimum, in summer, 21·7; and, although many of the cases received into the hospital list may really have been of some standing, the results prove that at a certain period the diseases assumed an aggravated character; a result which admits of no other explanation, than that of being dependent on atmospheric influence. The increased mortality during spring among those labouring under this disease, is a sufficient proof that this season of the year exercises an important influence over it.

Meyer's table confirms the general observation, that congestive diseases and hemorrhages are much more prevalent during winter; but the results of the influence of season upon these diseases in Dresden, are not so striking as those which have been obtained in several other places. This influence becomes more apparent when we observe the number of *deaths* which take place suddenly at this season from hemorrhages and congestions—a clear indication of the greater severity of the isolated attacks. We have here an instance of the manner in which cold operates, by driving the blood from the surface to the centre; and every practitioner knows how much more frequently death takes place from cerebral and pulmonary hemorrhage during winter, than at other seasons of the year; and during long and severe winters, than in those which are short and mild. In confirmation of this view, Casper quotes from the second report of our Registrar-general the return of 2382 deaths from diseases of a congestive or hemorrhagic

tendency. It appears that these were distributed over the four seasons of the year as follows:

Jan. — March.	April — June.	July — Sept.	Oct. — Dec.
729 = 30·6 : 100	543 = 24·4	471 = 19·7	599 = 25·1.

The table published by Ferrario, of the number of sudden deaths (through apoplexy) which took place at Milan during a period of fifty-six years (1774-1830), in which the influence of weather, season, age, and occupation on this disease is accurately traced, furnishes additional proof of the exceeding prevalence of apoplexy during the colder months of the year. Out of 10,432 deaths from this cause, we have the following numbers for the respective months:

January	1176	May	829
December	1075	October	822
February	1030	September	718
November	963	July	689
March	956	June	681
April	848	August	645

Total, 10,432.

From this it will be seen, that the number of cases of apoplexy which occurred in January, was nearly twice as great as the number registered in August.

Meyer's table only confirms what has been long a matter of experience, that diarrhœa and other disorders of the intestinal canal are far more prevalent in summer and autumn, than at other seasons of the year. From a table prepared by Cless, and reprinted by Casper, of the patients received into the hospital at Stuttgardt, during ten years, from 1828 to 1837, diseases of this kind formed only 6·5 per cent. of the cases which occurred in winter and spring; while they constituted no less than from 40 to 47 per cent. of those admitted in summer and autumn. It is, however, obvious that many causes, independently of atmospheric influences, tend to produce these diseases. Cases of bilious fever with gastric symptoms, varied equally with the season; in winter and spring these amounted to 440, in summer and autumn to 774.

Dr. Casper is inclined to think, contrary to the general opinion, that the disposition to attacks of syphilis and gonorrhœa is influenced by season. In his opinion Meyer's table furnishes proof of the existence of an influence of this kind. It appears that of 839 cases of syphilis and 71 of gonorrhœa, there was the following distribution of attacks according to the season of the year:

	Winter.	Spring.	Summer.	Autumn.
Cases of syphilis	205	174	232	228
„ gonorrhea	16	19	25	11
	221	193	257	239

The occurrence of these cases would thus appear to be by 7 per cent. more frequent in summer than in spring. Other causes besides those depending on season may, however, have some influence in producing a greater number of these cases during summer. The author was unable to make any comparison according to season, of the admissions for these diseases into the great hospitals of London and Paris. It has frequently been observed by practitioners of long standing, that venereal cases occur

more frequently at one period of the year than another; but further researches are required in order to determine the degree in which this more frequent occurrence of syphilis is to be ascribed to the effect of season.

It is of great practical interest to compare at particular seasons, the relative amount of sickness (*morbility*) with the relative amount of mortality. This has been already in part done, but it must be confessed that we are yet unprovided with any certain means of measuring the degree to which the severity or mildness of particular diseases may be influenced by season; although researches on this subject would have a most important bearing on prognosis in particular cases. The comparison of tables of *morbility* (to employ Dr. Casper's term) with those of mortality when accurately made under similar conditions, will enable us to form an approximate judgment, and to deduce some useful inferences of a practical kind.

Our author then furnishes us with a table of upwards of 52,000 deaths, occurring in Berlin during a period of ten years 1830-39, of which the causes were well ascertained. Those fatal diseases only were selected which were known to be more or less under the influence of atmospheric changes. We do not give this table, because we think a summary of the results will fully answer the purpose of developing the nature of the question which the author here proposes for solution. This table confirms the views already brought forward; thus with respect to inflammations attacking the brain, throat, lungs, and abdominal viscera, we find that out of 100 deaths from this class of diseases there were:

In summer.	In autumn.	In winter.	In spring.
19·0	23·6	26·6	30·5

The deaths from inflammation of the lungs and acute hydrocephalus, were so numerous during spring, that more than one third of all who laboured under these diseases perished at this season: a fact which may furnish a hint to the practitioner when forming his prognosis at the bedside of a patient. According to Buek, the deaths in Hamburg from diseases of an inflammatory kind, had the following daily average:

In summer.	In autumn.	In winter.	In spring.
62	70	80	3

And he also remarked what is a striking correspondence with the Berlin observations, that one third of the deaths from inflammation of the lungs and hydrocephalus, occurred during spring. The same inference is deducible from the deaths registered in the Edinburgh infirmary for three years, from 1822-4. These facts connected with the fatality of inflammatory diseases, are derived from 9400 cases observed during the second and third decennial periods of the present century, in Berlin, Hamburg, and Edinburgh; and we may thus take it as an established fact, that of all seasons of the year, the winter has the greatest tendency to produce these diseases; while the spring appears to render them most fatal. This great fatality is especially seen during the latter season in cases of pneumonia and pleuritis.

Let us now pass to the examination of that difficult question so often raised; namely, the influence of season and climate on *phthisis pulmonalis*. Dr. Casper informs us of what we have already learned from the researches

of Sir James Clark and others in relation to this disease, that it is one of the most universally prevalent and fatal; and that in every climate large numbers of persons are annually cut off by its attacks. The advance recently made in science has enabled the physician, without any great difficulty to detect the existence of tubercular disease in its earliest stages. The stethoscope, plessimeter, microscope, the scalpel aided by profound chemico-pathological researches, have furnished the most valuable materials to assist the diagnosis of the physician; but in spite of the assistance thus rendered, the art of medicine appears still to be powerless before this devastating disease. Medical statistics with respect to *phthisis* have been for a long period accurately kept in different countries; and tables of its attacks and rate of mortality are so numerous as to render the selection of satisfactory data a task of no great difficulty. From these documents we learn that medical treatment has had so little influence in arresting the progress of this scourge to mankind, that under every variety of climate, from a fourth to one sixth of all those who die from disease, are cut off by phthisis pulmonalis. From the many excellent works on this subject, in various languages, and from the tables published by different authors, Dr. Casper has made a selection of all those cases which appeared to resemble each other in their conditions, and in the circumstances which attended them. The table which he has thus constructed, refers to the period of the last twenty or thirty years. We here give it as containing the most recent and accurate statistical information, which we have on the relative degree of mortality occasioned by tubercular phthisis.

MORTALITY FROM PHTHISIS PULMONALIS.

Places.	Periods in years.	Deaths from phthisis.	Total deaths.	Ratio of deaths from phthisis.
Berlin .	10 . 1830-9	12800	73216	1 : 5·7
Paris . .	4 . 1816-19	15375	85339	1 : 5·5
London . .	2 . 1840-1	14562	91565	1 : 6·2
Hamburg .	3 . 1823-5	2360	10963	1 : 4·6
Stuttgardt .	10 . 1828-37	924	4356	1 : 4·7
New York .	11 . 1816-26	7466	..	1 : 5
Philadelphia .	7 . 1820-6	3590	..	1 : 7·7
Baltimore .	8 . 1819-26	2243	..	1 : 6·7
Boston . .	7 . 1820-6	1481	..	1 : 5·9
		60801		1 : 5·8

From this table, comprising upwards of sixty thousand deaths among different populations, we find that on the average *one death out of six* is caused by this formidable disease; in England about 59,000 lives annually fall a sacrifice to it. In comparison with this amount of mortality, the devastations occasioned by the plague or by a transitory pestilence like the Asiatic cholera, sink into nothing!

It was supposed by the ancients, and it is even now a subject of popular belief, that the autumnal season, i. e. the fall of the leaf, is the period in which consumption is most fatal; but the comparative number of patients admitted at different seasons, as well as the tables of mortality, prove beyond all doubt that spring is by far the most dangerous season to the phthisical. Observations carefully made in Vienna, Paris, Strasburg, Geneva, Stuttgardt, London, Hamburg and Berlin, all combine to show that the maximum number of phthisical patients die in the spring; and after this in winter; while the minimum deaths occur in summer and

autumn. On this account, observes Casper, it is the practice with physicians in England and on the Continent, to send their patients to pass the winter in a milder climate; a fact which shows how necessary it is to extend our inquiries into the influence of climate over this malady. It is not a little singular to trace the history of those who are thus banished from their country. According to Heineken, quoted by Casper, of 35 consumptive patients who went to Madeira in 1821, 2-3ds died on board; 3 died in the first month after their arrival; 5 to 6 survived the winter, and about the same number survived the following spring. From 3 to 4 lived to the second winter, but of the whole number there were only 13 living in 1824. The graveyards of Rome, Naples, Marseilles, Pisa, Nice, and Malta, bear ample testimony to the fatality of this disease, among those who have been induced to seek a foreign clime in the vain hope of recovery!

A question of this kind, however, is deserving of still further examination; and in order to throw greater light upon the subject, our author has given in a distinct table in his appendix, a most extensive series of observations comprising the varying states of the atmosphere with the relative monthly mortality from consumption, for a period of nine years (1830-38). The facts were collected by Mädler, and they comprise no less than 11,472 deaths, a number amply sufficient for the purposes of scientific inquiry. We agree with our author, that so extensive and accurate a table as this, has never before been published; and the facts which it reveals are calculated to remove many absurd errors respecting this disease, which have been too credulously received as truths by medical practitioners.

The mean barometrical pressure for Berlin during the nine years, was 333,551‴ (29·79 inches E.), and the average number of deaths from phthisis in each month amounted to 106. In 84 months, the barometer was above this average; and in 36 of these months the deaths from phthisis were fewer, while in the remaining 48 these were in number greater than the monthly average. In 24 months, the monthly mean of the barometer was less than that above assigned, and under this low atmospheric pressure, the deaths in 14 months exceeded, and in 10 months were less than the usual monthly average. As far, therefore, as any inference can be drawn from the relative amount of mortality and the pressure of the atmosphere, it appears that proportionally fewer phthisical patients die when the barometer is high, than when it is low; a result which is the reverse of that obtained from the analysis of the general mortality of a population.

It may now be well to inquire what relation, if any, subsists between the mortality from this disease and fluctuations in temperature; for there is a general opinion, that there is some close connexion between them; Mädler's table furnishes us with the following facts: The mean temperature of the nine winters (comprising the months of December, January, and February) was 33°. The coldest winter (1838) had a mean temperature of 24°; and in that season 393 phthisical patients died; the warmest winter (1834) had a mean temperature of nearly 36°, and the number of deaths was nearly the same. The two winters of 1832 and 1835, had nearly the same mean temperature (32°·7 and 33°·5 respectively); and yet there died in the first 345, and in the last only 284 patients from this disease; a difference which is too great to be referred to a difference in the progressive increase of population. The warmest of the 8 springs

(1830) had a mean temperature of 50°; and the mortality was almost as much above the average as in the coldest (1837), in which the mean temperature was under 44°. How is it possible to deduce from conflicting results of this kind any natural law?

Although there may appear to be some relation between the mortality from phthisis and the sudden fluctuations of the barometer when extremes are taken, yet by a close analysis of the table, even this slight relation disappears. These fluctuations were at their maximum in the summer of 1830, and at their minimum in that of 1831, and there were 26 more fatal cases of phthisis in the former, than in the latter season. On the other hand, there were in the summer of 1834, 30 deaths more than in that of the following year, although both of these summers had the same mean in their barometrical fluctuations. In the summer of 1838 there were 50 more deaths than in that of 1832, and yet these summers were almost identical in their barometrical variations,—a clear proof that the fluctuations of this instrument could have had no influence on the mortality from this disease.

The facts displayed in this table show that changes of temperature are as little connected with the rate of mortality from phthisis. The greatest variations in the thermometer were observed in the winter of 1831, the smallest in that of 1832; and yet there was scarcely any difference in the deaths from phthisis during these two seasons. In the former, the deaths were 356, in the latter 345. The winters of 1836 and 1837 were very similar with respect to temperature; and yet the deaths were respectively 260 : 342. The maximum difference in the range of the thermometer was observed in the winter of 1830, while the minimum difference was in the winter of 1833; in other words, there was during this season a remarkable uniformity of temperature, and yet, notwithstanding these extreme differences, the numbers of phthisical patients who died during the two seasons were nearly equal, being 326 : 334. As to the effect of temperature in accelerating death, it appeared, therefore, to be quite immaterial whether the phthisical patients breathed and lived in an equally warm atmosphere, or whether they were exposed to frequent changes of temperature irrespective of the hygrometric condition of the air.

With respect to this last condition, i. e. the hygrometric state of the atmosphere, it appears from the table that this had really a well-marked influence on the fatality of phthisis. The facts already quoted on a large scale show that, in general, humid is more favorable to health than dry air, and from this table we find the following results were obtained. The deaths from phthisis were:

In dry winters	367
In winters, partly dry and partly humid	338
In dry summers	291
In mixed summers	289
In dry autumns	298
In mixed autumns	263

The springs formed an exception, for in those which were dry the average deaths were 329, while in those which were of a mixed kind they were on the other hand 363.

It is well known that there is considerable difficulty in determining the prevalence of particular *winds;* and the results obtained by Mädler, with respect to the effect of certain winds in phthisical mortality, are somewhat

of a negative character. Without quoting the details, it may be sufficient to state that the mortality remained nearly uniformly the same in two similar months, when in these months the prevalent winds were in diametrically opposite quarters.

Dr. Casper has thus far endeavoured to show from accurate data, that the different states of the atmosphere and weather exert but a very feeble influence on the mortality caused by phthisis; and that the only appreciable influence which appears to exist, is that which is dependent on *season:* and here, he observes, it is necessary to bear in mind that health and life are affected by change of season in other ways than by mere differences in temperature, pressure, &c. Thus the occupations, mode of life, and food of large sections of a population, are well known to vary with the season; and this of course must have an influence upon those who are suffering from disease. Our author then raises the question as to how far, with a knowledge of these facts, we are justified in sending persons affected with phthisis into the South of Europe. It certainly is, as he remarks, rather an important fact, that we are carrying on this practice to a great extent upon merely hypothetical grounds of a supposed benefit from change of climate. We are as yet unprovided with any satisfactory statistical tables of the frequency and mortality of this disease among the populations of those countries to which we consign our own patients; and thus, can institute no comparison with those which have been drawn up in Northern Europe. In one of the most recent works on the Climate of Nice, by Ernst, this author takes a widely different view of the question from that taken by Casper; and his tables of the mortality in and around Nice, lead to a conclusion the reverse of that drawn by this statistician. Again, Dr. Richelmi, an eminent practitioner of many years' standing in that district, states that in ten years there were 6987 deaths, and only 107 of these, i. e. one in *sixty-six*, due to phthisis. Casper objects to this statement, as proving too much. A solitary result so conflicting with accurate observations made upon this disease, in all other countries, must tend to create distrust in the correctness of the data, especially as the sources of the observations and the decennial period in which they were made, are not mentioned. However, in Casper's opinion, the table itself contains intrinsic evidence that the statements are inadmissible. According to Richelmi, the general mortality in the country around Nice, is 1 : 37; in Nice itself, 1 : 32. Our author believes it to be absolutely impossible, from his knowledge of Nice and its environs, that such an enormous difference, as more than one seventh, should exist in the relative mortality of the town and country. Tables which lead to conclusions so evidently erroneous cannot be otherwise than faulty. Dr. Casper contends that there is no proof that any benefit is obtained by the phthisical in changing from a northern to a southern climate. The contrary opinion has arisen from an imperfect acquaintance with the influence exerted by the atmosphere and climate on this disease; and its existence shows the necessity for procuring accurate statistical information. As an instance of the want of any scientific principle in recommending a change of climate, he states, but without quoting names, that an eminent English physician of great practical experience, and a German *literary* (!) physician, have recently and on widely different grounds adopted the practice of advising their phthisical patients to make a journey to—Russia!

Nervous Fever.—(*Nervenfieber, Der Darmgeschwür-typhus.*) Dr. Casper now passes to the consideration of another form of disease exceedingly prevalent, which has received as much attention from science as tubercular phthisis, but which in a therapeutical point of view is nearly as intractable. This is nervous fever. It appears that this fever, with gastro-enteritic symptoms and ulceration of the bowels, is very prevalent and fatal in Berlin; and at certain seasons of the year spreads in an epidemic form. Under these circumstances it might be supposed that there would be no great difficulty in tracing a connexion between it and atmospheric changes; but the contrary is the fact. We quite agree with the author that, in these investigations, negative results are not to be despised; for if attended with no other benefit, they have at least the effect of removing error and scientific prejudices. The following results are, therefore, worthy of being recorded.

In the 108 months of the nine years referred to, there died in Berlin, from this fever, on the average of each winter month 27,—of each spring month 18,—of each summer month 23,—and of each autumnal month 41. The influence of season over this disease is, in Caper's opinion, well marked. The results were obtained from nearly three thousand (2947) deaths, and therefore it may be fairly stated, *that this fever is most prevalent and fatal in autumn, and least prevalent and least fatal in spring.* By the closest analysis, our author was unable to deduce from the observations, any inference which showed that the rate of mortality from this disease, was at all influenced by barometrical, thermometrical, or hygrometrical changes in the atmosphere, or that the deaths were greater or less, under the prevalence of particular winds.

Inflammatory Diseases. The diseases under this section, including hydrocephalus, croup, laryngitis, bronchitis and pneumonia, are notoriously influenced both in respect to their origin and diffusion, as well as in their rate of mortality by atmospheric changes. Out of 1000 cases of this description, there died in the spring 322, in the winter 276, in the autumn 228, and in the summer only 172.

Various and conflicting opinions are entertained respecting the influence of barometrical fluctuations and of particular winds, such as the north and easterly winds, over this class of diseases: these have long required testing by experience and observation. Casper has compared the number of deaths which occurred from inflammatory diseases when the barometer was above and below the mean, including the sudden fluctuations of this instrument, with the rate of mortality; and he has ascertained that there is no kind of connexion or dependence of one upon the other. The rate of mortality appeared to be quite unaffected by the variations in the *pressure* of the atmosphere.

The effect of temperature appeared to be somewhat more strongly marked, although not in the way commonly supposed. Thus it was ascertained by a comparison of years, that a high temperature during spring rather tended to increase the rate of mortality from inflammatory diseases. In the four coldest springs 1832-5-7-8, there died 813 persons; while in the four warmest, 1830-1-4 and 6, there died 880 persons. This conclusion was strengthened by a comparison of the monthly deaths in March, according to whether that month was colder or hotter than the mean. The mortality at other seasons also indirectly confirms it. Thus,

in the four hottest summers, the deaths were 656; in the four coldest, only 610; in the cold month of July 1832, only 53; and in the hot month of July 1834, 75. We need not enter into further details, all leading to the same result; but, making full allowance for increase of population and taking the average deaths for each year, we inevitably arrive at the conclusion, *that cold winters, warm springs, summers, and autumns, increase the amount of mortality from inflammatory disorders, and vice versâ.*

It is generally supposed that a very dry state of the atmosphere predisposes the respiratory organs to inflammation, and increases the mortality from these diseases; but facts appear to be against the correctness of this opinion. In nine wet months, the deaths from inflammation exceeded the average in four; but did not reach it in five. A similar result was obtained by taking the deaths in nine dry months. In the dry winter of 1830, 220 died; in the following winter equally dry, only 164; in that of 1836, which was very variable with respect to humidity, there were 180 deaths; and in the following equally variable winter, they were 249.

The examination of the table for the whole of the nine years, showed that the influence of the wind on the number of deaths from this cause, was inconsiderable. There were but slight variations under the prevalence of different winds; but no satisfactory inference as to their effect could be drawn from the observations.

As a general conclusion, it may be observed that the seasons of the year, taken on the whole, exert a pretty constant and universal influence on the mortality produced by disease. The special conditions of the atmosphere in relation to this influence, cannot at present be arranged under any law of nature. A greater delicacy and accuracy in our instruments of research, and a more extended series of observations, are required before this desirable object can be attained.

Age. Man is not the same at all periods of life; the stages of infancy, of adult and old age, exert their peculiar influence on health and mortality; and next to the effect of seasons, this influence of age calls for special consideration. Many statisticians of eminence, among others Villermé, Milne Edwards, Quetelet, and Lombard, have collected a large number of observations, which, although presenting many differences when taken individually, furnish on the whole tolerably uniform results. Accidental influences here disappear, and the operation of a natural law becomes apparent. The practical bearing and importance of this branch of statistical inquiry, must be obvious when we consider that the results refer directly to the proper method of preserving life in infancy and old age.

In order to throw light upon this subject, Dr. Casper has collected in an elaborate table no less than 40,000 deaths, which occurred at Berlin, during a period of seven years. It is derived from official documents in which the deaths were registered, according to months and periods of life. In the table, the deaths are arranged in seven sections; the first commencing with the deaths at and under one year of age, and the last including the deaths between the ages of 65 and 100. We here present a summary arranged according to the seasons.

	1 year.	1-7 yrs.	7-14.	14-20.	20-50.	50-65.	65-100.
Winter . .	20·80 min.	24·25 min.	24·04	27·47	26·18 max.	27·97 max.	29·28 max.
Spring . .	23·19	25·27	26·29 max.	19·98 min.	25·88	27·19	26·99
Summer . .	32·74 max.	25·06	23·41 min.	21·67	22·10 min.	22·42	21·52 min.
Autumn . .	23·21	25·34 max.	26·20	30·83 max.	25·66	22·37 min.	22·15
Difference between maximum and minimum.	11·94	1·09	2·88	10·85	4·03	5·60	7·76

These numbers are calculated upon a basis of 100 deaths at the different ages corresponding to the different seasons. It will be apparent from this table, that the influence of the weather upon life, is very different at the different stages of existence. The greatest difference between the maximum and minimum of deaths exists during the age of infancy, and the least difference in the first septennial period. It shows further, how much may be accomplished by the adoption of proper precautions for the preservation of life. Children, under one year of age, are cut off in much greater proportion during the hot season of the year, i. e., in the months of July and August, than at other seasons; as for example, during winter, in which the deaths are actually fewest. No such disproportion in the number of deaths as here exists, is met with at any other period of life. Thus then it follows, that the heat of summer is more fatal to children than the cold of winter. This fact cannot be explained by the supposition that the number of births is greater during summer; for in Berlin, the maximum of births actually occurs in winter. While the researches of Schübler of Stuttgardt, and Emerson of Philadelphia, furnish similar results, those of Villermé in France, and Quetelet in Belgium, tend to show that the maximum mortality among infants occurs during winter, a conclusion which is in accordance with physiological principles, owing to the low state of the calorific function at this period of life. Dr. Casper refers this great mortality during winter, to the evil practice which exists in some Catholic countries, and in those localities where the provisions of the Code Napoleon are rigorously enforced, involving the exposure of new-born infants to the influence of cold, by carrying out the provisions for civil registration and by the adoption of early baptism. In other countries, where these practices are not adopted, the mortality of infancy is decidedly lower, because the child is withdrawn from the injurious effects of cold. The balance of mortality in other seasons, excepting the summer, differs but little from that of winter; the fact is—infants are very sensitive to *extremes* of temperature, and easily suffer from them. Hence the summer is so fatal to infant life; for it is far less easy to protect a child against the intense heat of summer, than against the severe cold of winter.

The second period of life, from one to seven years, is that in which the difference between the maximum mortality in autumn and the minimum in winter is the least,—reaching only to 1·09 per cent., from which we deduce the corollary, that the seasons exert the least influence at this period on life and death. This corresponds to what has been already announced by Dr. Lombard respecting the mortality of Geneva. From the age of two to fifteen years, he found that the seasons exerted scarcely any perceptible influence on the rate of mortality. In the second period of the Berlin table, the difference was only 2·88 per cent. between the maximum

(spring) and the minimum (summer); and this, therefore, is the most favourable age after the first septennial period. In the third septennial period, comprising the age of puberty (14—20), a difference exists almost as great as that which is found in early infancy. While about 20 per cent. at this age die in spring, more than 30 per cent. die in autumn. The tables of mortality throw no light upon the causes of this excessive difference produced by season. It is worthy of remark, however, that, from the twentieth year up to extreme old age, the winter is the most dangerous and the summer is the most favourable season for health; and it is curious to notice how gradually the maximum mortality for the winter season increases with the advance in years. As man progresses in life, the more necessary does warmth become to him;—while the summer lengthens, the winter shortens his days. This is already a fact well established by numerous researches;—those contained in the present paper only tend to confirm the conclusion at which writers on vital statistics have already arrived. According to Quetelet's tables of mortality in Belgium, the greatest number of deaths among individuals above twenty-five takes place in February, and the smallest number in July. While Schubler's researches in Stuttgardt show that, as in Berlin, the greatest number of children die in summer and the fewest in winter, the mortality is completely reversed as to adults—the greater number of these dying in the winter season. Lombard has proved, with respect to Geneva, that among adults the cold of winter regularly increases the amount of mortality, so that among individuals above eighty years of age, for one who dies in summer two die in winter. Buek states that in Hamburg the greater number of aged persons die in January, and of adults in February. Dr. Heberden remarked that the winter of 1795 was the coldest, and that of 1796 the hottest, until that time known in London; in the former thrice as many old persons died as in the latter. The important practical consequences respecting the regimenal and hygienic treatment of persons advanced in life are thus rendered sufficiently obvious; while they indicate the necessity, they confirm the utility of making the most extensive inquiries into the subject of vital statistics.

Dr. Casper has attached to this essay the following summary of practical conclusions:

1. In Berlin, while the month of January is the least, December is the most favourable to health.
2. The greatest number of deaths occur in spring, and the smallest number in summer.
3. Extremes of temperature are dangerous to life.
4. A high barometrical pressure tends to increase, while a low barometrical pressure tends to decrease, the rate of mortality.
5. The influence of atmospheric pressure on human life varies in different seasons.
6. No condition of the air is so dangerous to life as dry cold. On the contrary, humid cold has the greatest tendency to support life.
7. Of all seasons of the year, the winter gives rise to the greatest number of cases of inflammatory diseases, while the spring is most fatal to them, especially to cases of pneumonia.
8. Cold winters, warm springs, summers, and autumns increase the danger and fatality attendant on inflammation attacking the brain and respiratory organs; and *vice versâ*.

9. The maximum mortality from phthisis occurs in spring, and after this season in winter. The minimum mortality from this disease occurs in autumn and summer.

10. Variations in the state of the atmosphere appear to exert but little influence upon the relative number of deaths from phthisis.

11. Nervous fever is most frequent and fatal in autumn; it is least frequent and fatal in spring.

12. The influence of weather and season upon health varies with the different periods of life.

13. This influence is most marked in the ages of infancy and puberty, but it is least marked in the first septennial period of existence.

14. From the twentieth year upwards, the winter is the most dangerous, and the summer the most favorable season to life and health; and the older the individual the more striking is this difference.

The paper of which we next propose to give a brief analysis is one nearly allied to the foregoing, in which medical statistics are brought to bear upon a somewhat singular branch of inquiry, namely the *Influence of the periods of the day upon births and deaths.*

It is perhaps known to our readers, that some researches have been already made in respect to *births*, although without leading to any satisfactory results. The statistical tables of Ranken, Quetelet, and Buek, although differing from each other in data upon which the calculations are based, agree in representing that more children are born by night than by day. The most exact are those of Ranken, collected from 890 observations in his own midwifery practice, in which the hour of birth is uniformly stated. The tables of Quetelet include 2680 cases, observed in the Hospice de la Maternité, at Brussels, between the years 1811 and 1822. Buek's cases are not so satisfactory, because they have been loosely collected from various sources of private practice: hence but little reliance can be placed on them in reference to the question about to be discussed. Casper's data are derived from 809 cases occurring in the Maternity of Berlin, between the years 1830 and 1833. His results differ from those of Quetelet in the fact that they show a more gradual increase and decrease in the number of births at different periods. We cannot here find room for the number of cases which occurred at each hour, but the following table comprises the total cases which occurred to each observer in *terhoral* periods; and it shows not merely the absolute number of births, but also the relative proportion on a standard of 1000 cases:

Hours of the Day.	Absolute Number of Births.					Relative Number of Births per 1000.				
	Quetelet.	Buek.	Ranken.	Casper.	Totals.	Quetelet.	Buek.	Ranken.	Casper.	Totals.
12-3 A.M.	445	159	137	116	857	166	171	154	143	161
3-6	353	131	129	114	727	132	141	145	141	137
6-9	299	141	119	95	654	112	151	134	117	123
9-12	315	90	85	92	582	117	97	95	114	110
12-3 P.M.	279	101	97	112	589	104	109	109	139	111
3-6	295	70	88	94	547	110	75	99	116	103
6-9	351	101	117	89	658	131	108	131	110	124
9-12	343	138	118	97	696	128	148	133	120	131
	2680	931	890	809	5310	1000	1000	1000	1000	1000

It will be seen from this extensive calculation of cases by various observers, that while the number of births between 9 o'clock in the evening and 6 o'clock in the morning exceeds the average (125), those between 9 o'clock in the morning and 6 o'clock in the evening are below the average. The greatest number of births takes place in the hours following midnight, and next to this, in the terhoral period between 3 and 6 o'clock in the morning. There is less agreement respecting the period at which the minimum of births occurs; but this Dr. Casper thinks may be owing to the unequal number of facts collected by the different observers. The author then proceeds to show that if the day be divided into sexhoral periods, the differences vanish, and the births then increase in frequency in the following proportions per 1000:

From 12 to 6 A.M.	296
6 to 12	232
12 to 6 P.M.	215
6 to 12	257

Thus they occur most frequently in the hours after midnight; then in the hours before midnight; thirdly, in the hours before noon; and lastly, in the hours after noon. Dr. Casper refers to various hypotheses respecting the cause of this preponderance of births during the night, but none of them appear satisfactory. For the first time, he has endeavoured to determine whether the cause of the difference had any reference to the sex of the child, to its healthy or morbid development, to its maturity or immaturity, to its being a first, or a second, third, or fourth child,—to the delivery having been natural or instrumental. The result of his inquiries is, that none of these conditions have any demonstrable influence whatever on the period of the day at which the birth occurs.

The subject, however, has hitherto been regarded only in one point of view,—namely, the period at which the act of birth, i. e., the separation of the child from the mother, is completed. The time at which labour-pains first appear is obviously material to the inquiry; the exceptions here may have a smaller range than those which exist with regard to the delivery of the child, since this condition is more likely to be under the influence of a natural law. Besides Dr. Casper thinks it not improbable that some practical hint may be derived from an examination of this question, whereby the obstetric practitioner may be able to form a prognosis respecting the probable sex and the probable duration of labour. In illustration of these propositions, he presents us with a table of 787 births arranged according to sex, and according to whether the children were or were not those of primiparous females. We must, as usual, content ourselves with the results. The maximum number of cases in which labour pains come on, occurred in the terhoral period between *midnight and three o'clock in the morning;* the minimum number of cases were between *six and nine o'clock in the morning.* The numbers gradually approached each other between these two extremes. The proportion of cases in which labour pains come on at night, is much greater than that in which delivery occurs at night. Thus in the Berlin Maternity, for one day-birth there were 1·05 night-births; while the births in which the pains began by day were 1·31, those in which they began by night amounted to 1·75. It was also ascertained as a curious fact, that when the pains of labour began by day, the greatest number of male children

were born; while when they commenced by night, there was a preponderance of female births.

The period at which labour pains came on appeared to have but a slight influence on the duration of labour. The average duration of a labour, from the number which occurred in the Berlin institution between the years 1830 and 1834, was about *nineteen hours*. In those births in which the pains commenced by day, the average duration was twenty hours; while in those in which they commenced by night it was eighteen hours. With respect to children born living and dead, the following facts were ascertained:

Out of 295 still-born children, the proportion in 1000, was for the four diurnal periods of six hours:

From 12 to 6 A.M.	275
6 to 12	230
12 to 6 P.M.	193
6 to 12	302

The maximum, in regard to children born dead, unlike that of the general births, occurs in the six hours which precede midnight; but the minimum falls in both cases in the afternoon hours. The preponderance of night- over day-births, with respct to still-born children, is even greater than with respect to those born living.

Influence of the periods of the day on mortality. Proceeding on the principle that nothing connected with human life is accidental or dependent on chance, Dr. Casper next proceeds to examine the question respecting the relative rate of mortality at different hours of the day. This is just as much a legitimate object of medical inquiry as the determination of deaths at different seasons or in different months of the year. Our knowledge must, in all these cases, rest upon similar data, i. e., upon facts accurately observed and recorded. The researches hitherto made on this subject by Virey, Metzler, Quetelet, and Buek are, in the opinion of our author, insufficient to solve the question. The numbers are either too limited, or they have not been collected with sufficient attention to accuracy.

Casper's data consist of 5595 deaths, officially recorded in Berlin, by those who are appointed by government to verify death and report the cases. To make the results simple and intelligible, the number of deaths is taken at 100, and the day is divided into four sexhoral periods. It is obvious, if the rate of mortality was the same throughout the day, there would be 25 deaths in each period, but we find the following results:

From 12 to 6 A.M.	25
to 12	29
1 to 6 P.M.	25
to 12	21

From this it will be perceived, that the maximum amount of mortality occurs in the six hours before noon, while the minimum rate occurs in the six hours after noon. The other two periods of the day appear to be indifferent. It is rather singular as a result, to find that the morning hours are those in which the greatest number of deaths occur; but it must not be forgotten that the period will be in many respects modified by the nature of the disease causing death. We consider it unnecessary to follow our author into his tables of mortality, as they are affected by the various classes of diseases. It may suffice to say, that they are of a

most minute and extensive kind. We learn from them, as general facts, that *four fifths* of all deaths (5591 cases) are due to *chronic*, and *one fifth* to *acute diseases;* and, further, that the number of those deaths from acute diseases is smaller in the morning and larger in the afternoon hours, thus departing from the average results of the total deaths. The author attempts, but in our opinion not very satisfactorily, to account for this peculiarity in acute diseases. He then passes into an analysis of the differences observed in respect to the diurnal deaths for various classes of chronic disease, and discusses certain hypotheses respecting the causes of these differences. He afterwards examines the probable influence of seasons on the deaths at various diurnal periods. Our space is, however, already exhausted, and we shall therefore merely subjoin the general conclusions at which Dr. Casper has arrived respecting the statistics of births and deaths:

1. The greatest number of births occur between nine o'clock in the evening and six o'clock in the morning; while the smallest number occur between nine o'clock in the morning and six o'clock in the evening.

2. The pains of labour commence most frequently between twelve o'clock at night and three o'clock in the morning; least frequently from six to nine in the morning.

3. The influence of night is more marked with respect to the commencement of labour-pains than with respect to complete delivery.

4. Among those births in which the pains commenced by day, the greater number were male children, and *vice versâ*.

5. On an average, delivery was more protracted when the pains commenced by day than by night.

6. The preponderance of nocturnal over diurnal births is more striking in respect to children born dead, than in respect to those born living.

7. The maximum mortality occurs in the hours before noon, and the minimum mortality in the hours before midnight.

8. Individually regarded, the ratio of deaths from inflammations, phthisis, and pulmonary hemorrhage is greater in the afternoon; from fevers and exanthemata, just before midnight; from cerebral apoplexy, during the day; and from diseases of the nervous system in general in the hours which immediately follow midnight.

We here close for the present our examination of this volume. Our readers will perceive that we have given an analysis rather than a review of the principal medico-statistical papers which it contains. We have preferred in most cases allowing the author to speak for himself,—by no means pledging ourselves to the unqualified reception of the whole of his views. As we have before remarked, some difference of opinion will necessarily exist among professional men respecting the practical utility of statistical researches of this kind; but as the influence of weather and season upon health and mortality is a universally admitted truth, it is clear that the general influence can only be properly determined by minute researches like those of Professor Casper, on the special influences of heat and cold, high and low atmospheric pressure, dryness and humidity, which, in reality, constitute the complex elements of the weather, and on the various degrees of which, atmospheric changes depend. It would be difficult to point to any researches more accurate, more extensive, or more elaborate than those of the learned author of these essays, and there is no doubt, if ever it be permitted to the medical philosopher to determine the exact influence of the weather on the salubrity of places, and on the healthy condition of the human species, it will be by pursuing the course

so well marked out by Casper. It would be absurd to pretend that such researches have yet led to anything like accuracy in prognosis; but in this respect this branch of medical statistics resembles many other parts of medical science. We cannot, however, fail to derive from such a condensation of well-observed facts new, more correct, and more enlarged views respecting the influence of atmospheric changes on health. There are many opinions on this subject which prevail extensively among the public as well as in the profession; some of them have arisen from inexact observations and hasty generalization, and yet they have been allowed to influence practice and hygienic treatment: others appear to have a better foundation in fact. If no other present benefit flow from these statistical researches, we have at least the satisfaction of knowing that they will enable us to distinguish the true from the false; and will teach us to be cautious in relying upon doctrines which have not been confirmed by experience and observation. We hope in the following number of this Journal to present an analysis of that portion of Dr. Casper's work which bears upon many novel and important questions in Legal Medicine.

Art. VII.

Die Pathologische Gewebelehre von Dr. Friedrich Günsburg. Erster Band. *Die Krankheitsprodukte nach ihrer Entwickelung, Zusammensetzung, und Lagerung in den Geweben des menschlichen Körpers.* Mit drei Tafeln. *Leipzig,* 1845.

Pathological Histology. By Dr. F. Günsburg. First Volume. *On the Development, Connexion, and Deposition of Morbid Products in the Tissues of the Human Body.* With three Plates.—*Leipsic,* 1845. 8vo, pp. 287.

Several of the German journals have, for some years past, afforded evidence of the industry and accuracy of Dr. Günsburg as a microscopic investigator; and we gladly welcome the appearance of the present volume, as affording us the means of laying before the readers of this Journal the results of some of his most important labours.

The ordinary reader will undoubtedly regard the book as dry and uninteresting; it is exclusively devoted to the microscopic examination of morbid products, containing few illustrative cases, and being totally devoid of the startling and magnificent theories in which microscopists are too apt to indulge. It is divided into four parts, the first being devoted to the consideration of inflammatory products, the second to that of tuberculosis, the third to the typhous process, and the fourth to cancer.

The products of inflammation are separately considered according as they affect:—

1. Serous membranes.
2. Mucous membranes.
3. The external skin.
4. Muscular fibre.
5. Nervous tissue.
6. The organs of respiration.
7. The organs of digestion.
8. The urinary organs.
9. The generative organs.

Inflammatory products of serous membranes. The products of inflammation of the pleura are first considered, and are separately discussed according as they are perfectly fluid, coagulated, membranous, or stringy. From his observations on the membranous variety we extract the following notice of the cysts occasionally found free in the pleural cavity.

"The formation of free cysts in fluid exudation of the pleura is a higher form of development. The cyst is inclosed in a white, transparent tissue, similar to the pia mater; it consists of an external layer of thin, transparent, areolar tissue, and internally of a layer of closely crowded cells, whose diameter is about ·007 of a millimeter, and presenting three or four nuclei. Between the layers of areolar tissue, a capillary network may be seen. The pathological formation of these cysts is as interesting as it is obscure. Their contents differ in no respects from the fluid exudation surrounding them. If we regard these perfectly isolated structures as separate and distinct products from their earliest formation, then we must admit that blood-vessels are formed in a cell-membrane, which is in no way connected with the normal parts, and that the isolated structure endowed with an independent vitality generates organic elements. But taking into consideration the analogical production of blood-vessels in other morbid structures, it is very probable that the cyst in an earlier stage of its development took its origin from the investing membrane of the lungs or ribs, and that the blood-vessels have developed themselves from the capillaries on this membrane, and that at a later period separation has been effected." (p. 5.)

Nucleated and elastic fibres are the highest forms of the inflammatory products of the pleura; in the disintegration of these forms into inorganic elements, we observe, in the first place, pigment-cells, then crystals, and finally, larger concretions of crystalline and amorphous constituents.

The fluid exudation in the pericardial sac in cases of inflammation, contains only a few blood-corpuscles, but is the cytoblastema of very numerous and regularly shaped cells, varying in diameter from ·0075 to ·01 of a millimeter having nuclei and nucleoli, and a great number of minute vesicles strewed over their surface. These cells are for the most part aggregated in great numbers; and this circumstance, combined with the uniformity of their structure, explains the rapid development which exudations of this nature undergo.

Plastic exudations in the pericardium assume the most varying forms; they may give rise to membranes, causing the heart to adhere to the free surface of the pericardium; to doubly fusiform bodies (Doppelzapfel), with one extremity attached to the heart and the other to the pericardium; or to a more or less rough investment of the contiguous layers of the pericardium. In colour they vary from a glistening white to a clear red, and they most commonly possess the consistence of coagulated albumen. They consist of a network of coagulated fibres, interspersed with nucleated cells, varying in diameter from ·007 to ·01 of a millimeter. The fibres are characterized by their great transparency and the inequality of their diameters. The cells at many spots are arrested in their development.

The plastic exudations from the serous investment of the interior surface of the heart—the endocardium—may be arranged in two groups. The former embraces those changes which give rise to thickening of the margin of the valves, and finally result in atheromatous and earthy deposits. On instituting a microscopic investigation of this form of exudation, we observe, between the elastic fibres which constitute the normal constituents of the endocardium, a series of undulating cylinders lying parallel to one another,

and having a diameter of ·005 of a millimeter, and, between them, fusiform cells, with minute semi-transparent nuclei. The occurrence of these fusiform cells shows the tendency to the production of fibrous structure. The earthy concretions which are formed on the breaking up of the inflammatory products in the endocardium, frequently occur as uncrystallized, amorphous masses, but as frequently they present perfect crystals. These depositions are often formed on the mitral valves, giving rise to thickening of those structures. They likewise occur on the aortic valves, and in this case Günsburg believes that they are produced directly from the earthy salts of the blood. These concretions consist of carbonate of soda, and carbonate and phosphate of lime.

The second group of exudation-structures occurring in the endocardium, is that in which the new elements are not imbedded in, but merely agglutinated to the primary tissue. These are most commonly found on the free edges of the tricuspid valves; they vary from the size of a hemp-seed to that of a pea, are frequently collected in patches, and are tinged red by the blood. They consist of a membrane composed of cells, with a diameter of ·01 of a millimeter, and covered at points with angular epithelial cells.

In his remarks on the products of peritoneal inflammation, he observes, that the serous membrane investing the liver is frequently infiltrated with a solid exudation, and is adherent to the diaphragm by newly formed membranous structures, consisting of very fine elastic fibres lying parallel to each other in irregularly undulating fasciculi, which are inclosed in a structureless membrane of their own. Opaque nuclei may be observed arranged lengthwise on their fibres, and likewise minute vesicles, giving the whole a mottled appearance. Vessels filled with blood-corpuscles may be observed between the fibres.

The peritoneal surface of the spleen is a frequent seat of inflammatory products. Günsburg mentions a case in which the convex surface was covered with a membrane of the thickness of a line, and so puckered and wrinkled as to present a glistening scaly appearance.

The peritoneal coat of the kidneys is rarely the seat of infiltrated exudation, while that of the ovaries is very frequently affected in this manner. Amongst the various changes in the ovary to which plastic exudation gives rise, we may specially notice the formation of cysts which occur either directly on the surface, or are attached to the organ by a pedicle. They are altogether distinct from the cysts, which occasionally are produced in the tissue of the ovary, but they sometimes attain a considerable size, and their walls are formed of a network of nucleated fibres, on which blood-vessels may be detected. The fluid within these cysts contains a small number of cells, having a diameter of ·01 of a millimeter, whose capsules are soluble in acetic acid, and which possess 3—5 distinct nucleoli.

Inflammatory products of mucous membranes. The account of the exudation from the nasal mucous membrane is very complete. In the early stage of inflammation it occurs as opaque, whitish, viscid, semi-fluid matter, containing cells of ciliated epithelium, very angular pavement epithelium, stringy mucus, and perfectly spherical nucleated cells, with a diameter varying from ·01 to ·015 of a millimeter, and containing 3 or 4 opaque nucleoli. When the inflammatory process has attained more of a

chronic character, the effusion becomes more liquid, although it retains its viscid, stringy character. The secretion then consists of inflammatory cells, varying in diameter according to the degree of their development from ·005 to ·015 of a millimeter. They contain one or two nuclei, presenting several nucleoli. These cells and free nuclei are suspended in great abundance in the structureless viscid mucus.

The formation of nasal polypi is described in the following manner. In chronic inflammation of the Schneiderian mucous membrane, the exudation at certain points undergoes an individual or independent organization. The adherent portion or root of the polypus consists of fusiform cells with elongated nuclei, resembling the ordinary primitive cells of muscular fibre; between these there are deposited nuclei, in some of which a central nucleolus may be detected. It is from these nuclei that the fibrous cells are formed, and they are in structure identical with the nuclei visible in these cells. In making a section of a nasal polypus, we usually find its structural elements arranged in the following order. Externally there is a layer of polygonal cells of pavement-epithelium, forming a protective investment. Below the epithelium is a layer of the round nuclei of the above-mentioned fusiform cells, and in most cases below these is a thick layer of nuclei and cells—the organized product, on which depend its shape and firmness. The red vascular structure in the body, and at the extremity of a polypus, consists of a loose network of nucleated fibres, and of capillaries filled with blood and having a diameter not exceeding ·005 of a millimeter.

Günsburg's remarks on the inflammatory products of the mucous membrane of the rectum are illustrated by the following singular case:—

"A girl had severe peritonitis, which, as it gradually yielded, was followed up by an abundant purulent exudation from the mucous membrane of the rectum in the vicinity of the anus. This exudation consisted of inflammatory cells, varying in diameter from ·005 to ·01 of a millimeter, with circular nuclei, and from three to six nucleoli. Some of the cells even attained to a diameter of ·015 millimeters, and contain from ten to eighteen of these nucleoli. The whole mass was neutral, and remained unchanged in concentrated acetic acid, but when the acid was diluted the capsules gradually disappeared. Ether acted in a similar manner. With distilled water heated to 212°, the cells remained unaltered, but with acetic acid at the same degree of heat some of the cell capsules disappeared, while the nuclei became swollen, and presented an irregular appearance at their edges. Some of the cells were reduced in volume by the boiling acetic acid, and measured only ·005 millimeters." (Vol. i, pp. 23-4.)

In a note to this case Günsburg observes, that on the same day on which the discharge appeared from the rectum, the urine also presented an abundant glistening deposit, and he regards these simultaneous phenomena as indicative of a crisis. The sediment consisted of epithelial cells, whole and broken, in which might be observed many spherical corpuscles, numerous dendritic crystals (urate of soda), and roundish cells, varying in diameter from ·005 to ·007 of a millimeter, and containing three or four minute black nucleoli. In addition to the increased secretion of uric acid—a very frequent occurrence at a crisis—the mucous membrane of the urinary organs presented an inflammatory product capable of plasticity. The simultaneous formation of exudations in the mucous membranes of the urinary apparatus and rectum at the moment of the crisis indicates that the tendency to exudation in the blood was then limited to points at which they (the

exudations) could be eliminated by the natural passages without injury to the system. The extinction of the process of exudation in the peritoneum, and its simultaneous appearance at the extremities of the mucous membranes of the intestine and urinary organs, form a natural crisis by antagonism. Facts of this nature are not merely valuable in a pathological view, but are of high import in relation to therapeutics, since they lead us to the knowledge of the time and manner in which we can serviceably apply artificial counter-irritation.

The section terminates with an account of his observations on urethral and vaginal discharges.

The inflammatory products of the external skin next claim our attention. Our author considers—first, those which are deposited in the rete mucosum, and which break through the epidermis; secondly, those which are deposited in the intermediate cellular layer (of Henle), and gradually work their way through the rete mucosum and finally to the surface; thirdly, those which are originally deposited in the derma, and at the same time involve the superior layers; and, fourthly, cutaneous ulceration depending on a morbid condition of the blood.

Under the first head he places—

a. Inflammatory products after a partial, mechanical destruction of the epithelium and rete mucosum—traumatic inflammation. They consist of inflammatory cells, varying in diameter from ·007 to ·01 of a millimeter, and containing few nucleoli.

b. Those in erysipelas bullosum, where there are well-filled inflammatory cells, such as may be observed in the fluid of blisters produced artificially.

c. Those in eczema, which is caused by the uplifting of the cuticle at certain points through the deposition of a few inflammatory cells in the rete mucosum. If the inflammatory product is so abundant as to break through the layer of epidermis, it hardens on the surface and forms crusts; it likewise penetrates into the intermediate cellular layer. This is the origin of eczema impetiginosum.

d. Itch-vesicles come under this head. Günsburg regards the question respecting the true connexion between scabies and the acarus as one to which we cannot at present give a decided answer.

e. Acne, which depends on the deposition of inflammatory cells in the rete mucosum, around the mouths of the sudaminous ducts, giving rise to an hypertrophied deposition of epithelial cells, and thus causing a thickening of the walls of the aforesaid ducts.

Under the second head we have phlegmonous inflammation of the skin, and pemphigus. He describes the latter as forming "in the glands of the hair-sacs, which are partially deposited in this (the intermediate cellular) layer. Instead of the fat-vesicles, or more correctly speaking, from them and from the plasma yielded by the adjacent blood-vessels, there are formed inflammatory cells, which become deposited in the adjacent cellular tissue and rete mucosum. After the removal of the exudation, the hairs fall out, and leave minute depressions in the cuticle after the disease is cured." (Vol. i, p. 29.)

Under the third head there are placed, furunculus, ecthyma, acne indurata, rupia, variola, and varicella; and under the fourth, purulent exudations from scrofulous, syphilitic, and cancerous ulcers. There is nothing on these subjects calling for special notice.

Passing over the sections on the inflammatory products of muscular fibre, and of the nervous centres (the latter having been fully considered by our countryman Dr. J. Hughes Bennett), we proceed to "inflammatory products in the respiratory organs." We extract the following observations on the varieties of glands found in the mucous membrane of the larynx.

"1. Simple grape-shaped glands, filled with cells presently to be described, and devoid of an excretory duct.

"2. Ramifying grape-shaped glands.

"3. Grape-shaped glands with an excretory duct. The cells investing the glands studded with barbed and curved cilia.

"4. Simple flask-shaped glands, formed of several epithelial cells lying over one another, and filled with inflammatory cells. On their apices are one or more ciliated cells.

"5. Double flask-shaped glands, or several simple glands lying on one another, sometimes having an excretory duct in the form of an inverted cone or cup.

"The glands often attain to a length of ·25 and a breadth of ·3 of a millimeter. They are perfectly full of cells, varying in diameter from ·007 to ·01 of a millimeter, with a nucleus and several nucleoli, which entirely prevent us from distinguishing the construction of the glands. After their removal, the tissue is seen to be composed of accurately-fitting cells of pavement epithelium, with nuclei having a diameter of ·005 of a millimeter. Sometimes there are also removed the nuclei of glandular epithelium, analogous in all respects to inflammatory cells, and the glands then exhibit simply a homogeneous membrane strewed with blackish specks.

"The cylindrical epithelium to which the cilia are attached, the pavement epithelium, and those cells which are newly formed in the glands (the enchyma of Purkinje), constitute the greater portion of the expectoration in cases of inflammation of the mucous membrane of the larynx." (Vol. i, pp. 46-7).

These various forms are well illustrated by diagrams.

We now arrive at what may perhaps fairly be deemed the most important subject of the work—*the inflammatory products occurring in the lungs*.

Günsburg observes, that during the first days of pneumonia—the first and second days—the expectoration often contains no blood-corpuscles. The inflammatory cells contain a pale nucleus with a central nucleolus, the nucleus being rendered more apparent by the action of an acid; these cells are filled with minute globules, varying in diameter from ·001 to ·002 of a millimeter. They occur sparingly in almost every expectoration, and are the products of the irritated mucous membrane of the larynx and bronchi, from which source also are derived the epithelial cells, which are constantly present. Pneumonic expectoration submitted to boiling heat becomes converted into a brown caseous mass, and heated with acetic acid yields a brownish red precipitate.

With respect to the changes which occur in the lungs in the progress of inflammation, we find that in incipient pneumonia the pulmonary vesicles are much distended and filled with inflammatory cells, having a diameter of ·005 of a millimeter, and full of granules.

The exudation occurring in the stage of red hepatization, consists of very numerous inflammatory cells, perfectly round, varying in diameter from ·01 to ·015 of a millimeter, and containing from 2 to 5 minute, well-defined nuclei.

The exudations occurring in the stage of gray hepatization are formed of in-

flammatory cells, varying in diameter from ·005 to ·015 of a millimeter, with a circular nucleus and central corpuscle; also of a small number of granular cells. Between the inflammatory cells there are deposited minute vesicles, fat-globules, or molecules. Acetic acid dissolves the capsules of the inflammatory cells in the pulmonary vesicles, leaving merely the nuclei; while the granular cells remain unchanged. At those parts of the lung in which the pulmonary vesicles are completely destroyed, nothing can be seen between the bronchial fibres but granular and inflammatory cells and the oval, caudate, fusiform cells arising from them, either isolated or arranged in the form of continuous fibres.

The following questions especially suggest themselves in this inquiry.

Which element of the pulmonary tissue is the primary seat of the exudation? And what changes do the other elements subsequently undergo? The following are Günsburg's replies to these questions:

"It is but rarely that the inflammatory cells are distributed in a regular plane between the bronchial fibres; in these cases the pulmonary vesicles appear compressed and to have lost their contents. Most commonly the pulmonary vesicles are the seat of the exudation. They become distended to three or four times their proper size, and inclose great numbers of inflammatory cells, varying in diameter from ·05 to ·08 of a millimeter. In external appearance they are exactly similar to the formative cells of the lung in the embryo, and only differ from them in the circumstance that the nuclei of the latter are insoluble in water and acetic acid, while the inflammatory cells in the lungs undergo the ordinary changes with these reagents. As the disease extends, the walls of the pulmonary vesicles give way, and the inflammatory cells are then only limited by the elastic fibres.

"When the exudation is scanty, the capillaries are seen to be distended with blood; with the augmentation and extension of the exudation they disappear. The exudation sometimes entirely fills the pulmonary vesicles, and is limited to them.

"The pulmonary vesicles and the intercellular walls of the capillaries are the primary seat of exudation. The augmentation of volume of the whole lung is essentially dependent on the distension of the individual pulmonary vesicles, and on their being filled with exudation-cells. I have not observed any thickening of the epithelium of the mucous membrane, even after long standing induration of the exudation. The sphere of development of inflammatory cells is limited, since the excess of exudation is accompanied by a deficiency of blood in the tissue. Moreover, the elastic walls of the pulmonary vesicles do not aid in their further formation. In the stage of purulent infiltration the pulmonary vesicles are seen to be partly filled with granular and inflammatory cells, and partly lying free amongst these two kinds of cells. Whether the exudation is originally poured forth into the intercellular spaces, or whether it does not find its way there till after the destruction of the walls of the pulmonary vesicles, or, in other words, whether there is an essential distinction between vesicular and interstitial pneumonia, are questions to which, with our present results, we can give no satisfactory answers." (Vol. i, pp. 59-60.)

The section terminates with remarks on the microscopic appearances presented by the lungs in gangrene, emphysema, and compression.

Inflammatory products in the digestive organs next claim our attention. The following case of typhlitis stercoralis is interesting in several respects:

"The walls of the cæcum were of double their ordinary volume; the peritoneal coat was covered with vessels full of dark blackish blood, especially towards the iliac end. The appendix vermiformis on being opened presented a horseshoe shape; the mucous membrane of its lower portion was of a pale gray colour,

thickened and presented numerous minute depressions (grübchen), some of which were isolated, while others were associated into a sort of network. The upper portion was divided by a band into two diverticula, in each of which the mucous coat was converted into a gangrenous capsular membrane, inclosing a hard concretion. The concretion in the lower diverticulum had the form and size of a date-kernel, and was of a bright yellow colour, but soon became brown on exposure to the air.

"The diseased surface was covered by a thin layer of finely granular, yellowish-green matter, in which were detected,—1. A large number of four-sided rhombic prisms, scarcely averaging ·02 of a millimeter in length. 2. Fragments having the form of imperfectly round nucleoli, and in some instances surrounded by a capsule. 3. A quantity of cylinders anastomosing at certain points, and presenting occasional contractions and dilatations. These cylinders or tubes were both filled and surrounded with spherical corpuscles, having a diameter of ·005 of a millimeter and by immeasurably minute molecular granules, which when accumulated presented a blackish-green appearance. On this layer of finely granular matter there was a brownish, very injected membrane. In the fibrous network there were numerous engorged capillaries, inflammatory cells, and crystals. The peritoneal coat, which could not be very clearly made out, was covered with crystalline fragments, and penetrated by numerous injected vessels; inflammatory cells could be observed between its elastic fibres.

"Hence typhlitis stercoralis is characterized by an extremely rapid exudative inflammatory process, occurring simultaneously in all the coats of the affected part, and dependent on mechanical irritation. To this follows gangrene, in consequence of the destruction of the supplying vessels, and hence the disintegration, not only of the normal elementary constituents, but also of the inflammatory products, into molecular particles. At the same time the inorganic elements of these tissues assume the crystalline form." (Vol. i, pp. 69-70.)

The section on *the inflammatory products occurring in the liver* is illustrated by descriptions of the microscopic appearances in three cases of nutmeg, and nine of fatty liver in various stages. From a review of these cases it appears that in all of them there was distension of the capillaries. At the same time fat-globules appeared, at first scanty and afterwards in great numbers, diminishing by their compression the adjacent hepatic cells. Other cells increased in size from their nuclei being apparently converted into fat, in consequence of which the capsules finally dissolved. The amount of fat-vesicles stands in a constant relation with the rarefaction of the hepatic cells, which finally become much compressed and have their interstices filled with matter apparently consisting of nuclei.

In other cases the fatty liver appears to depend, not on the deposition of fat-vesicles externally to the hepatic cells, but in the conversion of the nuclei and the cells themselves into fatty matter; these exert a mechanical influence on the adjacent cells not essentially altered. The fulness of the capillaries, their distension, and the primary swelling of the cells are phenomena which also occur at the commencement of inflammation. From this formation all the material that would be employed in the production of inflammatory cells undergoes a retrograde metamorphosis into fat-globules. The simultaneous conversion of the nuclei of the hepatic cells into fat, their being tinged at certain parts of the liver with bile, and the occurrence of extravasations of bile seem to point out that the primary condition for this formation of fat must be sought in a chemical change in the composition of the blood, and in some abnormality in the secretion of the bile. The further changes leading finally to atrophy must be referred

to a combination of mechanical causes acting from without, and a simultaneous conversion of the parts themselves into fat.

The granular, atrophied, fatty liver is described as due to a combination of inflammation and the formation of fat. The granulations consist, for the most part, of hepatic cells of a polygonal outline, varying in diameter from ·01 to ·025 of a millimeter, and, with the exception of containing a few fat-globules, being perfectly empty. Other cells in these granulations contain an enlarged nucleus filled with agglomerated corpuscles; while others are so full of fat-globules as to resemble granular cells; and others again are totally empty. Between these hepatic cells, a few caudate, elongated, fibrous cells may be observed.

The structure occurring between the granulations consists of nucleated fibres running in various directions. In the meshes of the network formed by these fibres, spherical cells, with a diameter of ·005 of a millimeter, and containing several globules, may be observed. These are the remains of hepatic cells. The production of this form of diseased liver is explained by Günsburg in the following manner.

Around the larger vessels of the liver we meet with an inflammatory product succeeding to hyperæmia, and rapidly assuming a higher state of organization. The hepatic cells, inclosed by the fibrous cells and nucleated fibres, are no longer capable of secreting bile, in consequence of the deficient supply of blood they receive, as well as of its altered condition, and they become converted into fat-globules. That inflammation is the primary cause of this affection, and that its products lead to the formation of fat in the liver, seems proved by the facts (deduced from the examination of cases) that all the phenomena of inflammation of the liver are originally present, and that it is not until the symptoms of chronic disease of the liver have existed for a considerable period that dropsy, the invariable attendant on the granular fatty liver, presents itself. Further, the absence of free fat-vesicles and the presence of some uninjured hepatic cells in the large granulations show that the fat in the hepatic cells is formed from the nuclei, and that this abnormal metamorphosis stands in certain relations with, and is consequently dependent on the degree to which the inflammation had extended.

The inflammatory products occurring in the kidney are divided by our author into the three following classes.

In the first class he places those in which the product occurs in the genuine form of spherical inflammatory cells, or after a prolonged time becomes formed into granular or fibrous cells. The second class embraces those in which the inflammatory product takes the form of fibrous cells and fibres, causing displacement of the normal tissue, and compressing, or even destroying the urinary canals. This includes several varieties of Bright's kidney, namely, the first, second, third, and sixth forms in Rokitansky's arrangement.*

In the third class the inflammatory product does not assume the cellular form, but the elements of the cell-formation are broken up into masses and destroy the tissue of the organ, giving rise to fatty degeneration of the kidneys. This morbid change was first described by Gluge, but the dis-

* See Vol. XV, pp. 100-102, of this Journal.

covery seems to have been independently made by Günsburg. The following is our author's description of it:

"The kidney is about half as large again as in the healthy state, and presents a greater degree of resistance. On cutting through it we observe that it presents a yellowish-white colour, with reddish streaks, (externally resembling Rokitansky's second form of Bright's disease.) Three days after the examination, the naked eye may detect the escape of fat. On a microscopic examination, innumerable fat-globules having a diameter of ·005 of a millimeter may be at once observed. Between these, perfect granular cells having a diameter of ·02 of a millimeter may be detected. The perfect condition of these granular (or pus-) cells, shows that the formation of fat is dependent on elements which have had a tendency to exudation. The urinary canals appear, for the most part, as transparent empty cylinders.... The development of fat-globules, and the emptying and compression of the urinary canal, are characteristic of fatty degeneration. It differs from the fatty degeneration of other organs, in there being no free effusion of fat on its cut surface." (Vol. i, p. 91.)

The first part of the volume terminates with the *inflammatory products occurring in the generative organs*, under which are considered ovaritis, exudation in the follicles, serous cysts, ovarian dropsy, apoplexy of the follicles, the formation of pigment in them, their cicatrized condition, inflammation of the fallopian tubes, and fibroid of the uterus. The chapter contains several paragraphs that we had marked for insertion, but the length to which our review has already extended, warns us that we must proceed at once to the second division of our volume, which is devoted to the subject of *Tuberculosis*, and occupies the succeeding fifty-two pages.

Passing over the first two chapters of this division, devoted to the tuberculous products of serous membranes, and tuberculosis occurring in the nervous centres, we arrive at the chapter on the tuberculous products of the respiratory organs.

Günsburg commences by observing that, notwithstanding the endemic intermittent fever of the Silesian valleys, tuberculosis of the air-passages, is remarkably frequent, and decimates the population.

Simple tubercular granulations of infiltrated pulmonary tubercle consist, according to our author, of cells having a diameter of ·005—·0065 of a millimeter containing three, five, or even more distinct, minute granules, without, in most cases, the appearance of a nucleus. These cells are unacted on by water and dilute acetic acid, but in concentrated acetic acid the capsule becomes paler and finally dissolves. The blood-vessels in the surrounding pulmonary tissue are much distended, and around them we observe pigment-cells and free granular pigment.

The softening of pulmonary tubercle, the mode of formation and the contents of vomicæ, the expectoration, and the healing process, all meet with full consideration from our author.

The microscopical and chemical characters of the expectoration are recorded in eight cases of phthisis in various stages. At the commencement of tuberculosis few inflammatory or tubercular cells are discharged with the expectoration. That the inflammatory product arises for the most part from the pulmonary vesicles, and the finest ramifications of the bronchial glands, seems obvious from the circumstance that none of the epithelial structures are present; which, as the disease progresses, are thrown off from the bronchi, trachea, and larynx.

The tubercular cells occurring in the expectoration during the early

stage of the disease are not so perfect as those occurring in the tubercular granulations themselves.

When the disease extends to the bronchi and larynx, we have, as additional elements of the expectoration, ciliated and pavement epithelium; and when softening commences, molecular fragments of disintegrated tubercular cells are seen intermingled with other intact cells of the same character. At a subsequent stage—when the pulmonary tissue is being rapidly destroyed—we find that the expectoration likewise contains fragments of bronchial fibres, pigment-granules, and minute, generally rhombic, crystals.

In the remaining chapters, devoted to tuberculosis in the digestive and urinary organs, in the muscles and bones, there is nothing requiring especial notice.

The third division of the work treats of the typhous process, a subject we noticed at some length in our review of Rokitansky's 'Pathological Anatomy,' in Vol. XV, pp. 95-8, and to which we shall probably advert in reviewing the first volume of that extraordinary work.

The concluding division of the work may be very briefly dismissed. In the fifty-two pages of which it consists, we have full accounts of the microscopic appearances presented by the various forms of cancer occurring in the liver, stomach, pancreas, spleen, serous membranes, brain, muscle, bone, and external skin. The subject of cancer has been so copiously discussed in the recent numbers of this Journal, that we shall make no extracts from this division; we cannot, however, part with our author, without strongly recommending this, as well as the other portions of his work, to all who devote any attention to the microscopic investigation of morbid products.

ART. VIII.

Neue Untersuchungen in den Gebieten der Physiologie und der practischen Heilkunde von Dr. K. H. BAUMGAERTNER.—*Freiburg*, 1845.

New Researches in the Departments of Physiology and Practical Medicine. By Dr. K. H. BAUMGAERTNER.—*Friburg*, 1845. 8vo, pp. 402.

DR. BAUMGAERTNER, of Friburg, is one of those German physiologists to whose labours we are indebted for the natural history of cells. He claims precedence, indeed, in this field of discovery over Schleiden and Schwann. In the present work, he gives the further results of his researches in physiology and pathology, and which he wishes to be considered as a sequel to those undertaken by him in 1828 and 1829. The work is divisible into three parts: firstly, new researches into cell-life and development; secondly, sundry theories and hypotheses founded thereon; thirdly, empirical contributions to pathology. We shall notice analytically one or two of the more important matters contained in the first and third divisions of the work.

I. PHYSIOLOGICAL RESEARCHES. *The structure and development of muscular fibre.* Baumgärtner gives in detail the results of his researches into the embryonic development of muscular fibre, and into the microscopic anatomy, mode of reproduction, and relations to the tendons, nerves, and

blood-vessels of the developed fibre of muscles, whether voluntary, involuntary, or mixed. We shall content ourselves with a general summary of the results.

Shortly after impregnation the ovum divides into globular halves; each of these divide into other two halves, and the latter into other two, and so on continuously, until at last the whole yelk is subdivided into small globules—the formative globules—which in the frog, are 0·003 inch in diameter.* A separation then takes place in different parts of the yelk between the globules, and they unite into groups, and from these various organs are formed. In a certain part of the yelk, namely, in that corresponding to the vertebræ, may be observed a number of globules united like strung pearls, some of which rows when united form a cord; these are the muscles in their first formation.

Every formative globule consists at first of conglomerated yelk-granules, with an amorphous matter; a change, however, soon commences in the centre of the mass, and a homogeneous colourless granule is formed. This granule gradually increases in size by appropriating to itself the surrounding yelk-granules, until at last it occupies the whole space of the formative globule. This cell does not consist of homogeneous matter as the primary granule, but is divided into minute parts, which have the appearance of small globules with minute granules, and of flattened corpuscles. From the shell-like covering (Schale) is formed the tissue, and from the minute globules arranged in a row, the fibres of the tissue. By the junction of the fibres of each cell with those of the next, muscular fibres are formed, and as each cell is open above and below a tube is formed by their junction.

The formative matter entering into each muscular fibre, or rather tube, gives origin to one or several series of new formative granules, which pass through the same series of changes as the original granules, and in this way new muscular tubes are formed one within another, like a nest of pill-boxes. Sometimes these inclosed tubes have the same axis as the parent tube, sometimes the axis is not the same: a difference of arrangement depending probably upon fixed principles.

In none of the different kinds of muscular tissue are the fibres merely longitudinal or placed in one direction only; they are all interlaced. In the voluntary muscles, the cross fibres are fine and perfectly round; in the inner layers and among the fine tubes, they cross the others, so as to give the tissues a tuft-like appearance; but in the outer layers and among the broader muscle-tubes, they present a net-like appearance. In both cases the interlaced web of muscle-tubes is so constituted, that there are two bundles of fibres, of which the one passes spirally to the right, the other to the left.

The so-called primitive muscular fibrils are not simply fibrils, but are made up of several finer, after an arrangement very similar to that by which the muscle-tubes are made up. On examining these carefully, there may be seen lucid points recurring at regular intervals which give the fibril a knotty appearance. These spots mark the points where the very

* This cleaving of the yelk is not yet proved to be general, although from the embryological observations of late years, it probably is so. We observe that Dr. Reid noticed it in the development of the ova of the nudibranchiate mollusca, as described in a paper published by him in the June number of the 'Annals and Magazine of Natural History' for 1846.

fine transverse fibrils that form the tuft-like tissue, cross the longitudinal fibrils; this shining point of junction appears, on a more minute examination, to present the appearance of a square projection.

The whole of the motor apparatus consists of regularly arranged muscle-tubes, between which cellular tissue is found, termed here *perimysium* by our author, but which appears to correspond to the *sarcolemma* of Bowman. The nervous and vascular trunks occupy these cellular interspaces. From the nerves smaller branches are given off, which loop round the bundles of muscle-tube, and return into the trunks from which they are given off, or else join to a neighbouring branch. No twigs can be traced into the muscle-tubes, and it seems certain that the nerves do not terminate in them, but only loop round them. The arterial capillaries are given off from the small arterial trunks in the perimysium, and looping round a bundle of muscle-tubes, end in the venous trunks which accompany the arterial. These details confirm in their more important points the researches of Bowman and others.

The general structure of the skin. Baumgärtner limited his researches into the structure of the skin to the relations of its principal tissues. These are three; 1. The epidermis, or cuticle. 2. The firm thick structure constituting the dermis. 3. A double network of vascular and nervous loops like those of the muscles, ending in the veins and the nervous trunks. These loops of nervous fibrils are easily traced in the mucous membrane also; as, for example, in that of a frog's throat. On the tongue, the nerves are distributed thus: a distinct twig passes to each papilla, and is there spread out; the course of the twigs is not easily followed, but Baumgärtner has no doubt that loops are formed which return to the main trunk.

The structure of the brain. Are the nerves and brain constituted alike? or are they distinct tissues? and if the latter, how are they connected? Baumgärtner examined the brain and spinal ganglia of the crayfish (das ganz kleiner Krebse); the latter to illustrate the former.

When a spinal ganglion is stripped of its investing membrane, and placed under the microscope, it presents the appearance of a flattened body made up of two striated halves, and the trunks of connected nerves. The middle of the ganglion is opaque, but it is apparent, that it has at that point no striated or fibrillar appearance, and is of a colour unlike that of the nerves. If pressure be applied, the two strands separate a little, and bundles of transverse fibres are observed, together with a mass in which transparent globules or vesicles are perceptible. On a closer investigation, this mass is found to consist of those globules which are connected with each other by a soft membranous tissue. The globules themselves when placed in water, are found to be cells containing one granule or several. The connecting material contains also cells of a smaller size. If the surface of the strands be examined at their entrance into the ganglion, numerous small oval cells are seen, and on closer observation broad membrane-like striæ are observed to inclose these cells and form a network with wide openings round the strands. If the latter be a little separated, this network is found to form a bridge of connexion between them. Besides these cells there are smaller, that are heaped up in masses here and there.

The spinal strands and lateral nerves may be readily traced into the

ganglion; it is only in the centre that their course is obscure. Baumgärtner states the fact confidently, as observed by him that the brain consists of two tissues. 1. The proper cerebral substance made up of a layer of large and a layer of smaller cells, and which lie in a network. 2. The nervous fibrils entering into the ganglia which cross each other in various ways, and, forming loops, return to the stem from which they departed. Baumgärtner states that he has detected this structure in the brain of a chicken. Such of our readers as may attempt a verification of our author's statements, will do well to attend carefully to the cautions and directions given by him for the better observation of the structures he describes, since it appears that considerable management is requisite to get a sight of them. In particular, blood-vessels must not be mistaken for fibrils of nerves.

The cortical substance of the brain in animals of a higher organization appears made up of a soft membranous adherent mass and a network of vessels. Baumgärtner could detect only a few loop-like nervous fibrils running back into the medullary substance. The medullary substance appears to be made up of fibrils and a special network in which they lie. As a general summary of his researches, he observes that the nervous fibrils are distributed at their termination in the brain as at their termination in the muscles. They are partly collected in bundles and partly form loops, which, uniting, crossing, or decussating with others, return to the point of entrance and again join a main trunk.

Structure of the nerves. The researches of Ehrenberg and of his successors in histological inquiries have satisfactorily shown, that the nervous fibrils are tubular, and not simple threads. What do these tubes contain? What is the structure of the walls? The nerves of the crayfish when compared with those of man are like sacking compared with fine linen; they are therefore much more suitable for histological inquiry on these points. The fibrils of those proceeding from the cerebral and spinal ganglia have the following structures. They are flattened tubes made up of large oval cells, giving them a jointed appearance. If they be left in water for a while and their contents then squeezed out, the latter is found to be more or less fluid, and has the appearance of a granular mass, presenting here and there soft globules and fine cells, some of which may be also seen adherent to the inner surface of the tube. Many cells may be also seen on the margins of each tube, and it may be often observed that they are situate between the thin layers of its walls. The embryological development of the nerve-tubes is similar in all essential respects to that of the muscle-tubes. These researches of Baumgärtner appear to have been conducted without a knowledge of those of Foville.

Physiological relation of the nerves to the skin, muscles, and brain. The microscopic researches just related are a confirmation of the opinion generally received, that the nerves form loops at their terminal points, whether in the skin, muscles, mucous membranes, or brain. But Baumgärtner says, they are not lost in these structures. They are to be considered as long ellipses, the one segment of which is in the central axis, the other on the periphery. How do these loops act in the phenomena of motion and sensation? That the muscle-fibres must have an active share in the production of motion is obvious from the fact, that the loops do not universally

pervade them, as well as from various circumstances connected with muscular action, and which has led to the general adoption of Haller's views of irritability, as an inherent property of muscular tissue. Baumgärtner differs from Haller and his disciples: he attributes muscular action to the opposite polar currents of the muscular fibrils developed in them by innervation. The nerve-fibrils are conductors; the muscular fibres are non-conductors. He thinks that the granules of the cells are the active bodies, and the surrounding tissue (perimysium) connects them into a whole and gives elasticity to the structure. When innervation occurs, the granules are attracted to each other and adhere, and thus shortening of the fibrils takes place. Of course when the muscle is relaxed, or innervation ceases, or the antagonistic muscle acts, a separation of the coherent granules takes place, and a consequent elongation of the muscle and a relaxation of the contraction. The tuft-like interlacing of the muscular tissue is, according to our author's views, admirably adapted to the mode of action here supposed. The theory has the merit of ingenuity, but we can hardly say that it is novel. It is analogous to that propounded some years since by Mr. Wharton Jones.

Mode of sensation. It is matter of observation, that the smallest possible point of the skin is sensitive; but there are not nervous fibrils reticulated over every point. It is therefore inferred that the direct or immediate agency of the sensitive nerve is not necessary to the communication of a sensation. It is certain, too, that the mere physical action of light on the retina is not all that is requisite to vision. The skin, Dr. Baumgärtner thinks, must be looked upon as consisting of conducting and non-conducting tissue; the former being the cutaneous fibrils, the latter the epidermis. In each sensory apparatus, a distinct chemical or physical process is developed by external agencies. The whole explanation is too long and too hypothetical for our pages, we will therefore substitute an observation on spectral illusions by the author.

"In consequence of the illness of one of my children, I had passed many sleepless nights and experienced much distress of mind. When the mournful scene was at last coming to a close I threw myself on a couch, and found that a confused number of images arose in my brain as soon as I closed my eyes. The sick child had often directed its hand to my nose, and I now continually saw a hand stretched out to my face. I saw the body of a child, which at the abdomen ended in the body of a dog, and both portions contorted themselves in a peculiar manner. I further observed a long row of grinning spectres, a dog's head on a stump, a ship with broken masts, &c. I had my consciousness perfect, and meditated on these interesting phenomena. I could cause these spectral appearances to appear and disappear at pleasure. When the daylight fell on my eyes and the forms of surrounding things were obvious to me, the illusions disappeared; but the moment I shut my eyes and external visual influences were excluded, they reappeared." (p. 90.)

We are acquainted with a physiologist who had a similar, and perhaps better, opportunity of investigating the nature of cerebral sensorial changes, in consequence of an accidental laceration of the scalp. His consciousness was perfect also, as in Baumgärtner's case. He found that things in the room, which he did not regard attentively, but which only casually caught his eye, assumed various forms, but principally those of still or statue-like objects. The shadows cast on the wall from the watcher's

candle assumed various grotesque and gigantic forms; the folds of the white coverlet appeared sometimes like beautiful marble statues, the trunks of which tapered away apparently *ad infinitum*—sometimes they were like laundresses at work, &c. The character of the spectres seemed to depend upon coincident excitor phenomena. Thus, a noise of a basin gave rise to a spectre of interminable rows of potters, all busily at work and hammering; then, the fire-irons making a slight noise, the scene changed, and there was row after row of blacksmiths, ending in a long perspective, each a repetition of the other, and all hammering away with the utmost noise and diligence. Sleep was in fact prevented by these spectral sounds and sights.

The following opinion of Baumgärtner is interesting, but we must for the present hesitate to admit its validity. The readers of this Journal will require no new grounds for an unbelief:

"I am of opinion that there is psychal activity at every point where the nerves are supplied with cerebral or ganglionic matter. I have convinced myself on this point with reference to the spinal cord of lower animals by numerous experiments. I have observed decapitated frogs, snails, eels, flies, wasps, crayfish, &c., to perform movements which were exactly in harmony with the position in which the animal was placed, which exhibited adaptation, and which could not be considered as mere reflex movements." (p. 91.)

The opposite polarity of the tissues. We pass over various points in nutrition to notice a theory propounded by our author, which is ingenious, to say the least of it, and which is capable of various applications. It has been already explained, that the muscular fibrillæ shorten only when acted upon by the motor nerve. If, however, a piece of muscle be separated from the body, together with a portion of its nerve, we find that, on repeated stimulation, the irritability is lost, and is only recovered after a period of rest. Since the nerve is entirely dissevered from the central axis, the recovered power of excitory movement cannot be derived from the latter; nor can it be in consequence of renewed *enhæmatosis*, because the circulation is equally cut off. Baumgärtner argues therefore, that the power must have been derived from the muscular tissue itself, and that in fact there is a sort of polar or ganglionic action between the two forms of tissue. As an analogous process in ganglionic action, he points out the increase in magnetic power of a magnet induced by an increase in the weight of the iron. Baumgärtner therefore concludes, that the terminations of nerves are not simply recipients and conductors, but are also active agents in innervation, and have a polar relation to the tissues in which they terminate. Another curious theory, or analogy, brought forward by our author in connexion with nutrition, is the opposite polarity of apparatuses, as of vision and the optic nervous centres. The reaction of cosmic agents within the living body also presents our author with a subject of curious inquiry, especially with reference to sleep. This, he observes, is dependent upon the law of alternate activity and repose, and not upon cosmic agents only. There is less stimulation during the night, and consequently the tissues rest, to recruit their power, or become "charged" again. During the state of activity in the day there is greater stimulation, and the accumulated power is "discharged."

Organic motion. The laws of organic or vital motion are sufficiently

obscure to warrant our noticing Baumgärtner's theoretical views, especially as they are supported by observation. In fact, a knowledge of these laws is of the highest importance in practical medicine. It is not possible to understand fully the nature of congestion, inflammation, fever, &c., without being acquainted with the causes of organic movements. Baumgärtner has devoted considerable research to this subject, and has arrived at two principal results: 1. There is an organic attraction and repulsion, as well as an electric, a magnetic, or a galvanic. 2. In fully developed animals, having a complete nervous system, the movements are under its control. In the movements of the solids there is, as a result of innervation, an attraction and repulsion between the constituent parts, and, in the movements of fluids, the innervated containing tissue puts the fluid in motion.

The nature of the changes that occur in the ovum after impregnation, show that polarized currents are developed in it. The movements of the globules resemble the movements of particles on an electrified plate, or floating free in water. They unite to each other, then separate, and again unite, and so on continually, until material changes take place in them. It is to be noted too that the ovum divides into two distinct halves, or opposite poles, or centres, and each of them into two other halves or opposite centres, and so on as before described; polarization taking place continually and developing the formative globules. It is further to be observed, that there is a decussation of these organic currents, or lines of attraction and repulsion, just as Oerstadt has found in electro-magnetic currents.

The subsequent changes in the constituents of the ovum indicate the influence of a polarizing imponderable agent. After the yelk-globules have been worked up and the formative globules developed, an action takes place from the centre to the circumference of the latter—a repulsion—and a cell is formed. The linear arrangement of the cells in the formation of tissue is dependent (Baumgärtner argues) on organic currents, and the nutritive process is analogous to that of crystallization.

The above statements are illustrative of the *dualistic* theory now advocated in Germany by numerous physiologists; a theory intended to comprehend phenomena apparently the most remote and the most dissimilar, and which only requires the substantial support of demonstrative proof to render it universally prevalent. There is something extremely fascinating to the mind in a theory which includes in one category the phenomena of life in its minutest details, of molecular physics, of light, heat, electricity, and galvanism, and of the vast bodies that move through eternal infinite space, and submits them to the operation of the simple laws of terrestrial and celestial motion. The explanation of the motion of the blood by the dualistic hypothesis, as given by Baumgärtner, is ingenious; our space will not, however, allow us to enter upon it.

We have an interesting chapter on the development of blood-globules or cells: and another on the structure of the hydra viridis, both which we recommend to the notice of histologists: it appears from Baumgärtner's researches, that the hydra viridis is made up of three layers of cells, (cells not united to form a tissue, but each an independent cell;) the inner layer is the stomach, the outer is the sensitive layer, the middle the motor.

There are no traces of nerves or of blood-vessels; the cells draw their own nutriment from the food taken into the stomach, and act also as conductors and motor agents.

A contribution to morphology. The next chapter is so curious a morphological fragment that we are compelled to notice it. Our author applies the dualistic theory to the development of the skeleton, and propounds the doctrine, that the latter is neither more nor less than a double ossific cell. He enters into details to show that at first an animal consists of one cell only, and that this divides into two cells (blase), an anterior and a posterior, inclosed in a common membrane. Each of these divide into two lateral halves, and into an upper and a lower half, and then into further subdivisions: on one side, the brain and spinal cord being developed with the posterior spinal column and the connected muscles, on the other, the anterior spinal column, the lungs, and the abdominal viscera. Lastly, from each of the two primary body-cells (körper blasen) four projections arise—those springing from the posterior cell being developed into the upper and lower jaw, those from the anterior corresponding to the four extremities. We need not prosecute these curious views further; at present they are merely curious.

Cells. The next chapter is on globules and cells. In this, Baumgärtner claims the discoveries usually attributed to Schwann and Schleiden. He refers to his elements of physiology and general pathology (published in 1835) for views, facts, and researches, like those brought forward by Schwann in 1839, when he propounded the theory of cells. He also claims for his views more completeness than is exhibited by those of the cell theorists, inasmuch as the latter bring forward no plan or theory which connects the multitudinous cells into an individual mass, whereas his dualistic theory attains this. We must leave the question of priority to be settled in Germany, simply observing, that Baumgärtner claims priority of discovery over Schwann by four years. In the present and succeeding chapter, is a review of the whole doctrine of cells founded on personal researches. We propose ere long, to give our readers a review of the cell-theory, which shall be supplementary to that in which we some years ago introduced it to our readers, and which shall include the dualistic doctrines propounded in this chapter by Baumgärtner, should future inquiries confirm his.

A chapter on "Animal chemistry" is devoted to an application of the dualistic theory (or theory of animal polarity) to the processes of nutrition and secretion. The elective affinities displayed by various tissues for the different constituents of the blood, are explained by our author with the aid of his theory. His views are, however, too hypothetical to be presented to our readers in detail: we therefore simply indicate where they are to be found.

II. Pathological Researches. *Nature of parasitoid formations.* Tubercles first appear in small, gray, semitransparent points. If such a small mass be removed and examined, it is found to be divisible into smaller masses. If examined by the microscope, these are seen to consist of small granular bodies and globules, the latter being contained in a granular cell, which adheres to the next adjoining cell. Baumgärtner

thinks there is no doubt, that the formation of tubercle commences with the formation of simple granular bodies, that these cohere, and a globule is formed, and after that a granular cell, the process being in fact analogous to that exhibited in the development of the ovum.

The caseous matter of tubercles consists of small granular bodies which are not widely dissimilar from the formative globules (previously described) of the frog's ovum, such as they appear immediately after the formation of granules. If the caseous matter be in a more advanced stage of softening, it is found to consist almost altogether of globules, or cells containing a granule, their parietes being also somewhat granular. In proportion as the tubercular matter approaches the stage of suppuration, the granular bodies disappear, and the pus-globules (having a cell contained in a cell) become more numerous and interspersed with transparent molecules.

From these and similar results of microscopical investigation, Baumgärtner is led to think, that tubercle is a species of false membrane, and that their formative process is analogous, in all respects, to the formative process observed in the development of the ovum. The granular bodies are analogous to those of the yelk, which they resemble; out of them, as out of the latter, formative globules are developed, and then these latter change into cells. Here, however, the analogy ceases. The cells of the healthy process unite together to form the different tissues; the pus-cells remain in this low stage of development.

We pass over a long discussion on the pathology of heterologous formations, and also another on fever. In the latter, Baumgärtner applies his doctrines of dualism to the elucidation of febrile phenomena, and advances many ingenious hypotheses, which unfortunately are hypotheses only. He moots, for example, the idea already advanced by Schultz, that the periods of febrile diseases may depend upon the periods of development and decline of morbid cell products circulating with the blood. But then there is no proof advanced; it is a mere supposition, and so on with others. Our author's chapter on inflammation and its products is, in all respects, similar to that on fever. The dualistic cell-theory is applied to its pathology, but rather by hypothetical ratiocination than demonstration.

A chapter on Therapeutics contains some clear general applications of the previous theories to the cure of disease. One of these is, the destruction of parasitic animals by inunction. No plant, or animal, nor even a solitary cell can exist without an indirect or direct communication with the atmosphere. An oil enema destroys ascarides; and illination with olive oil will cure the itch: it seems not improbable that the method of cure in other cutaneous diseases that are removed by inunction is similar. Temperature has also an important effect. Aphthous products do not well bear a diminished temperature; consequently, cold gargles will be beneficial. Cold or warm remedies may be also applied locally. Poisonous applications have been extensively used in cutaneous and aphthous affections, as borax, alum, mineral oxides, salts, and acids, sulphur, iodine, phosphorus, &c. It is not improbable that arsenic, administered internally in cutaneous affections, acts as a poison on the morbid products, and might be used very effectually as an external application. Of course it would require the greatest care in its management.

A chapter entitled: "Final observations on the dualistic theory of life," is a singular composition. It extends the theory into cosmogonic metaphysics, if we may be allowed to connect two such words. It considers the theory of creation, of God's existence, the metaphysical notions of Descartes, Spinoza, Hegel, Schelling, &c.; the cosmogonical ideas of Laplace, Humboldt, &c. (applying dualism to an explanation of the theory of planetary formation, with which the British public has been lately made acquainted by the author of 'Vestiges of the Natural History of Creation;') and links the smallest particles of organized matter with the mighty masses of infinite space.

III. Clinical Contributions. *Typhus fever.* It was with some interest that we entered upon the perusal of these clinical contributions, because we were curious to ascertain, whether the study of transcendental physiology had impaired the practical tact and therapeutic skill of the writer. We are bound to say that no such noxious influence appears to have been excited on his mental powers or professional ability. He descends very gracefully from the sublime to the common-place. Ulcerative or abdominal typhus appears at Freiburg, ordinarily with an adynamic type. There is a febrile condition with little vascular excitement, a depression of the system, the typhoid countenance and tongue, and small pimples, or stigmata (frieselblasen) on the breast,—a very common symptom in one form of pestilential fever in the middle ages—the febris stigmatica. There was also diarrhea, bronchitis, vertigo, and slight delirium. Active cerebral excitement rarely occurred, and ecchymoses, petechiæ, or hemorrhages, in few cases only. The gastro-enteric, or mucous form of typhus, was not of frequent occurrence. In some cases the bronchitis masked the typhus, and the symptoms were not unlike acute phthisis; in others a complication with pneumonia led the most acute practitioner into mistaking typhus for the latter disease. The expression of countenance, however, led to a correct diagnosis.

Treatment of typhus at Freiburg. Baumgärtner, after trial of different methods, relies upon the expectant method as the best. At the commencement he ventures on an emetic, and he is inclined to think that it has occasionally "cut short" the disease, but on this point he speaks doubtfully, as the cases thus treated successfully might not have been true examples of the disease. He says, "I have treated many cases of typhus without administering any potent remedy throughout the whole course of the disease. Small doses of ipecacuan, or liquor of acetate of ammonia have been given, and these because patients are apt to lose confidence, and be dispirited unless something medicinal be done. The cases in which he adopts the expectant method are those in which the excretions are not suppressed, in particular those in which there are urinary deposits. In the first stage of cases complicated with pneumonia and bronchitis, he ventures on large doses of calomel, and occasionally a general bleeding; but in the second stage the calomel has been decidedly hurtful. In cases of extreme prostration, he administers wine freely, and in the rare examples in which the pulse is intermittent, and cramps take place, he places the patient in a warm bath, uses gentle cold affusion on the head, and administers musk.

Treatment of angina tonsillaris (rachencroup). Baumgärtner speaks of cauterization with lunar caustic as the most effectual of all means in angina tonsillaris, when false membranes form. He has never lost a patient from this disease since he adopted this method of cure. Incision of the inflamed tonsil is another effectual means of relief.

Colourless hemorrhage. Under this term Baumgärtner wishes to describe cases of flux in which the blood-globules are retained, and the colourless serum, plasma, or lymph, only passes out of the circulation. It is to be seen on a small scale as a sequel to hemorrhage from a small wound. It occurs principally after spontaneous hemorrhages, as from the uterus. In one example of this kind which Baumgärtner has described, the colourless hemorrhage was so profuse after the ordinary flooding, that the fluid soaked through a hair and a straw mattress, and flowed upon the floor; and he is of opinion, that similar cases (the discharge being in a more moderate degree) are often considered as, and mistaken for leucorrhœa. He would also classify under this head, the serous fluxes which occur from the bowels after diarrhea, or dysentery, and thinks that they are dependent upon a structural change in the mucous epithelium and subjacent parts, such that the serous contents of the capillaries escape. This serous or lymph-like discharge from the bowels has occasionally come under our own notice as a sequel of hemorrhage, and we believe has retarded the recovery very materially. We have found astringent enemata useful.

Effusion into the serous cavities is, in many cases, to be considered as an example of colourless hemorrhage. To this class, Baumgärtner thinks those cases are to be referred, in which the effusion is rapid, and unaccompanied by inflammatory symptoms, and particularly when blood-globules are here and there observed floating in the fluid.

Prison scrofula. Baumgärtner refers to scrofula as being one of the opprobria medicorum. The supplies of scrofulous cases are derived principally from orphan asyla, infirmaries, and particularly houses of detention. He has carefully examined the hygienic condition of the prisons in his neighbourhood, and he indicates the causes of the excessive prevalence of scrofula amongst them. Firstly, it appears that the prisoners are principally employed in the dusty and sedentary employment of weaving; secondly, there is a neglect of proper ventilation; thirdly, a want of proper food. Commonly, the prisoners have a *small* portion of animal food once only in eight days, but Baumgärtner knows prisons in which it is given out only once a fortnight!

The remaining articles in the volume are on the various forms of gastrodynia; a case of poisoning by strychnia, belladonna, and prussic acid (an apothecary taking his own drugs); an interesting case of aneurism of the root of the aorta; and a short essay on the prevention of the plague. The principle he recommends for accomplishing the latter is a modification of the morbid poison, by first passing it through the bodies of brutes, and then inoculating man, as in vaccination, the vaccinia being considered a similar modification of variola—an old suggestion. Numerous plates, illustrative of histological anatomy, are appended.

Art. IX.

Thoughts on the Nature and Treatment of several Severe Diseases of the Human Body. By E. J. Seymour, M.D., F.R.S., &c. In two Volumes. Vol. I.—*London*, 1847. 8vo, pp. 260.

Incredible as it may appear to those who shall attempt to follow us in the same path, it is, nevertheless, true, that we have gone over the volume before us three times—have literally read it thrice from beginning to end: such are the painful but imperative duties sometimes imposed on the conscientious reviewer! We had several reasons for the performance of this feat, the glory of which, we venture to believe, will for ever remain exclusively our own. In the first place, we were desirous of knowing thoroughly the "thoughts" of a man who had been long recognised as one of the busy and fashionable physicians of London, and who, over and over again, boasts, in the volume before us, of his great opportunities and exertions past and present. "These 'thoughts' (he tells us) have been put on paper under unceasing professional occupation" (Preface); "after more than a quarter of a century in the daily exercise of my profession" (p. 39); "in St. George's Hospital, during eighteen years' most assiduous duty" (p. 26); "a most diligent observer of post-mortem examinations, and with great opportunities for more than a quarter of a century" (p. 66). In the second place, all the diseases treated of being of daily occurrence, and consequently of great importance, we were determined that our readers should not be deprived of any information that might be in any degree useful in practice, through any disagreeable influence created in our mind by the form and mode in which it was conveyed. In the third place, we will candidly admit that we took up Dr. Seymour's volume with a slight prepossession against the author, originating in the popular reputation attaching to his name, of his being the only practical physician in Europe, under sixty years of age, who derided auscultation and set the stethoscope at nought. Having ourselves, in common with every other physician of Dr. Seymour's standing but Dr. Seymour himself, had the unquestioned and unquestionable physical truths of auscultation established as part and parcel of our mental furniture for more than twenty years, feeling it just as easy to doubt of the existence of the organs with which the phenomena of auscultation are associated as of the existence of these phenomena, or of the positive diagnostical significance of many of them; knowing, moreover, that it is impossible to do justice to one's profession, to one's self, or to the victims of many pectoral diseases, without the knowledge of these truths, and that they were, furthermore, of easy acquisition; we found it hard to conceive how any man of sound mind, of an average standard of intellect, and with even a moderate amount of moral endowment, could think as Dr. Seymour thought, and act as Dr. Seymour acted; and, consequently, we felt some difficulty in believing that Dr. Seymour could produce a medical book of any sort worthy the notice of his brethren. But this being merely *à priori* reasoning which, though generally just, might be exceptionally false, we determined that any prejudice thence arising should not be permitted to warp the judgment. A fourth reason for bestowing unusual attention on Dr. Seymour's book, originated in the expectation of finding

a source of interest growing out of the very peculiarity just named as a defect. Looking to the mental qualities requisite to raise and keep a man "in that bad eminence" which Dr. Seymour had attained—an eminence which certainly entitles him to be reckoned the boldest physician of his time—we thought we would be sure of meeting in his practice with boldness at least, if not originality, and might thus find metal more attractive than meets us in pages which never think of catching a grace beyond the reach of ordinary art. Lastly, we found many passages of the book so hard to understand, from the singular style in which the greater part is written, that we were compelled to go over it more than once for the mere purpose of coming at the author's meaning.

We regret to state that the result of all this extraordinary labour and attention is far from satisfactory; inasmuch as we ourselves have obtained nothing which we regard as adequate to repay us personally for our trouble, and only a scanty gleaning of very ordinary materials which we can communicate to our younger readers as worthy of their notice. Indeed, the perusal of this book has been to us, in many ways, a very painful task: among other consequences, it has given a heavy blow and great discouragement to more than one of certain cheering professional glorifications in which our contented and sanguine temper has often led us to indulge. For example—it has been a favorite theme with us, in talking over the subject of professional prospects with our younger brethren, to maintain the proposition that superior talent, and learning, and professional knowledge, *ceteris paribus*, are sure to lead to superior success—we mean worldly success—in practice; and *vice versâ;* and in proof of this, we have been accustomed to point to the character and actual position of all, or almost all our physicians and surgeons most estimated and best remunerated by the public. "By their fruits (we have said) ye shall know them;" and referring to the works that have done most honour to British medical science in our day, we have had to name as their authors the very men who stand highest in the world's esteem. It might be invidious to give names in illustration; but we believe it will be found that taking the twenty or thirty physicians and surgeons who have the largest practices in London, they are really, with two or three exceptions, the very men whom Science would place where they now stand. At any rate, we should find that the most eminent of them who have given books to the world, have shown themselves in them to be well entitled to take their place in the front rank of educated and learned men. To refer to a few of the works of our senior physicians only that we have had occasion within these few years to notice in this Journal,—where shall we find in our medical literature writings more correct and scholarlike than those of Holland, Watson, Latham, Clark, Ferguson, Bright, Prout, Paris, &c. &c.?

The volume on our table, we lament to say, is of a very different stamp. It is one which, to speak mildly, it is neither creditable to a physician to have written, nor creditable to medical literature in the nineteenth century to have given forth. It is not enough to say that the book is written in a style that is careless, slovenly, and rude, often obscure, sometimes unintelligible; a considerable portion of it seems written in utter disregard of the ordinary collocation and relation of words and phrases, and not a little of it is actually ungrammatical and un-English. It would be waste of

time to adduce instances of these defects on purpose; we doubt not most of them will be exemplified in a greater or less degree in the extracts we shall have occasion to make with other views; and we venture to say that no reader can turn over three consecutive pages of the volume without admitting that our statement is not beyond the truth. Nor are the demerits of the production confined to the style merely. A considerable portion of it is really a surprising mass of confusion and bungle, without order, arrangement, or consequence; facts and opinions, observations and conjectures, the author's and other people's notions, the past and present, the right, the wrong, truisms and transcendentalisms, being all jumbled together in most admired disorder. To analyse such a work, so as to exhibit anything like a regular epitome or even outline of its contents, will be admitted to be no easy task, and is one we should never have thought of undertaking, but from the position and standing of the author. Nor, indeed, shall we even now attempt to give a regular analysis of the book, but shall content ourselves principally with noticing, as we go over the pages, what strikes us as calculated to convey information of more or less importance to our readers, whether that information be of a positive or negative, of a commendatory or a monitory stamp.

It would, indeed, be most strange—more strange even than such a wonder as this book is—if two hundred and sixty pages could be written by "a physician and formerly a teacher," after the experience of "more than a quarter of a century," without containing something not generally known, that might be of value to the younger practitioner. But even here, we must content ourselves with very moderate doings, and must be excused for so contenting ourselves; the unlucky schoolboy who, after losing himself and frightening himself in the thicket or the quagmire, has just got into fair daylight and beaten tracks, must not be expected to have a rich stock of eggs or nuts to show when he gets home.

I. The first of the "Severe Diseases" respecting which Dr. Seymour gives us his "thoughts" in this his first volume are, Diseases of the Stomach. These he proposes to treat under the head of their more prominent symptoms, as pain, vomiting, &c.; and he gives his reasons for so doing, with his wonted lucidity and elegance, in the following sentences which constitute the opening paragraph of his book:

"At first I wished to have made a distinction between disorder and disease of the stomach; but one of these appears to me to lead, when neglected, so much into the other, that I shall speak of all under one head. There are also two modes of describing the different diseases: one is pathologically, which leads to obscurity, some pedantry, and much difficulty in the adaptation of remedies; and as my only object is practical advantage, I prefer the second, which begins with what is obvious, and proceeds with what is difficult and obscure to the observation in general. I proceed, then, to speak of by far the most obvious and distressing symptom in these troublesome disorders, though often not present in the most serious ones—viz. pain in the stomach." (pp. 1-2.)

The whole essay, which extends to seventy pages, is just such a performance as this introduction prefigures. It contains nothing that is new, and represents the old things in forms that are much less attractive and intelligible than we have been accustomed to meet with them in scores of preceding treatises on this interminable theme. Dr. Pemberton's

slim volume, published some thirty years since, will convey much more information to the junior practitioner than all Dr. Seymour's whirligig lucubrations. The commonest remedies, rhubarb, magnesia, soda, bismuth, hydrocyanic acid, &c., prescribed in the commonest combinations, yet paraded in all the display of *Recipes*, are the staple of this part of the book.

The only thing unusual in the treatment of *Cardialgia* which we meet with is the large doses of opium recommended in certain cases. Dr. Seymour gives a grain three times a day, and mentions a case (p. 9) where this was continued for four weeks. No doubt this is true as regards this individual case, but we have no hesitation in saying that such medication as this will generally be found not merely ineffective, but most injurious. Opium has always been a favorite remedy with empirical and heroic practitioners, and we shall afterwards see that it is especially patronised by Dr. Seymour.

The milder cases of *Pyrosis* are said to be easily cured as follows:

"Gr. v of Pulv. Kino Comp. given three times in the day, and the bowels kept open by enemata or aloetic purgatives, speedily cures the disease, especially if combined with rhubarb, &c. &c. and a diet principally consisting of poultry, butcher's meat (once in the day), eggs, potted meat, curried food, and weak brandy and water as a beverage." (p. 10.)

Does the author give this diet as in any way peculiar?—We are glad to see, that in such cases he discountenances the use of "calomel and purgatives," informing us that, when so treated, they "became greatly protracted as to time and severity" (p. 10),—protracted as to severity! Under the date of 1843-4, he gives a case which is a very instructive one indeed. It was a case of stomach disorder in a lady, marked by vomiting, &c. "I repeatedly *asked her* if, by putting her hand on the pit of the stomach, she could discover any hardness or lump. The answer was: Certainly not." Some nine months after this she consulted Dr. Seymour again, with aggravation of the old symptoms, and with the addition of "an extreme sense of debility." To the same *queries* as to the state of the epigastrium, she replied as before:

"The patient became worse. About three weeks after this meeting, she had a fainting fit, and when summoned I found her in bed. This enabled me to examine the abdomen, and greatly was I shocked to find, not one tumour, but a number of tumours, varying from the size of a pigeon's egg to that of a hen's egg, occupying the whole epigastrium and the left hypochondrium—fungoid disease! I went home, resolving nothing should ever deter me from examining the stomach, especially before giving an opinion, in future." (p. 12.)

The patient died of course. Now we cannot help expressing some surprise that Dr. Seymour should have waited till the year 1844 to learn the necessity of examining the abdomen in all such doubtful cases, or that he should not have here been "enabled" to do so, until he accidentally found his patient in bed. This is surely not the kind of lessons he taught his pupils when "formerly a teacher."

The following elegant paragraph introduces the subject of gall-stone; and we may say that it completes it also, as we have scarcely anything else on the subject, except references to two cases which seem to be introduced—the one to show that the author was "Physician to the Asylum for the Recovery of Health, in the year 1827," and the other to

record the unique fact of an old lady who to relieve pain "applied bottles heated to such a degree as to *burn the muscles* of the part." (p. 16.) Burn the muscles! Why, the bottle must have been not only red hot but solid or filled with solid matter, to retain heat sufficiently long to effect this.

"The next most obvious and most frequent case is where the pain in the stomach or at the scrobiculus cordis is indicative of gall-stone; either the passage of a gall-stone alone along the hepatic duct, the cystic duct, or the ductus communis; or even where there are a very large number of these concretions in the gall-bladder, so that, during the operation of digestion, any change made in its situation produces tension of the gall-bladder itself. The pain, in either of these cases, is similar. There is most violent pain at the scrobiculus cordis, shooting through to the back; pain as of grinding the part is felt. The pain is so intense that it appears absolutely impossible that it should long be borne; nevertheless, the pulse is never quick, and often preternaturally slow, as 50 or 60 in a minute, rarely above the latter. Sometimes this intense pain is accompanied by vomiting, but not always. However severe, it ordinarily disappears suddenly, as if by magic, and the patient, allowing for languor, is nearly as well as usual.

"When the calculus has entered the duct, whether it gradually proceeds forward or falls back, the pain is succeeded by a greater or less degree of jaundice, which immediately marks the disease; then, of course, the excretions are pale, often as white as chalk, while the urine is of the colour of porter; and this state continues until the concretion escapes into the bowel. Still there may be obstruction from a gall-stone, and subsequent intense pain at the pit of the stomach, tingling or itching of the skin, vomiting, while the urine is of the natural colour, and the fæces with the usual proportion of bile. In this case the calculus is in the cystic duct." (pp. 14-5.)

In the next fifteen pages we have an account of chronic ulcer of the stomach, perforation of this organ, and hæmatemesis, all mixed and intermixed in so strange a manner that it becomes almost impossible to find out to what particular disease individual observations or individual remedies apply. Here, also, the author ventures to leave the only path he should attempt to tread, and gives us some general statistics respecting the degree of prevalence of perforation of the stomach in the two sexes. We are disposed to agree with him, however, in believing that "there is no preponderating difference in either sex as to predisposition to this terrible disease." (p. 30.)

In the middle of this discussion the author, *more suo*, breaks out into quite a different theme: here it is a discourse on "diet in general;" and though he says he can "scarcely help incurring blame" for his opinions, we must say that they appear to us the least blameable part of his book. With the common-sense views of the following extracts, we entirely agree, and have the more pleasure in transcribing them because they are really good English—and do not, we believe, contain more than a single grammatical error.

"Other persons insist, when living in London, and never quitting their arduous duties, that they are free from all impeachment of irregularity, provided, as they say, they only eat meat, beef, mutton, and poultry; never sweets, entremets, jelly, cream, and the like; never eat pastry, not even apple-pie, and live in a delightful dream of civic anchoritism. These persons take no exercise, sit up late, often in heated rooms; their hearty meal of meat overcharges their blood (happy had it been divided with a little fruit jelly, or baked fruit, or even the anathematised apple-pie); they get heated, feverish, pains in the joints, pains in

the back, and at length gout,—wondering where it can come from, as they never indulge!

"In speaking of health, the great advantage is to be sought from regular exercise, and in London life, if possible, on horseback; and great moderation. But if general rules be difficult, what shall we say to partial ones—to the unhappy hypochondriac, who sees poison in all that is wholesome, and has some book on diet to look at *à la dérobée*, to sustain him in his imaginary distresses? Is it a cause or an effect, that persons dreadfully sensitive about what is wholesome are always weak and sickly?

"If we watch, as I have watched persons on this subject, where is the agreement? or, what is more, from what is our experience to be derived? Some stomachs cannot bear nor digest animal food, as beef and mutton. To these, dishes cooked in the French fashion, where food is much subdivided, can be taken without pain, and with comfort. To some, fish, even those which are thought to be most injurious, give no pain; salmon, eels, mackerel, are all eaten without trouble, where mutton chops and beef-steaks give pain. In another case the first-named aliments are productive of sickness, pain, bad taste in the mouth, and all the annoyance of what is called bilious derangement .....

"How, then, are we to judge? I have been much struck by the facility, in illness, and in delicate constitutions, with which food much desired by the stomach is digested. Should not what I may call the instinct of this viscus be a little more consulted? and if there be a strong feeling or desire on this point, may it not be advantageously gratified in defiance of arbitrary rules? Every day, in extensive practice, we may be answered in the affirmative!

"Unhappily writers on this subject recommend from their own feelings or prejudices. A healthy hearty man recommends what he finds keeps himself in excellent health: while one with a weaker stomach cannot help conveying his ideas on the subject, which are very different. A man accustomed to the fare of a hot country like India, cannot manage the diet of a cold country, where great and active exercise is taken. The endeavour to bring an Indian to the wholesome food of an English farmer, would be to enact the old story of the man who brought his horse to live on a straw, and—he died.

"The whole question resolves itself, except in actual disease (fever, bowel complaints, violent cough, expectoration of blood, consumption, and diabetes, &c. &c.), into one small word,—moderation. To man in health, and in many diseases, all is comprised in that one word, moderation. Take what your inclination shows you to be desirable when in health, only avoiding what is manifestly injurious from your own experience. In illness nearly a similar rule may apply. A man in a fever could not be made to eat beef; and it is only in convalescence, or recovery from recent illness, that a man must be obliged to listen to lessons of absolute self-negation!" (pp. 32-4.)

Although our author tells us that, in this part of his work, he has "followed the natural course as far as the mind can be carried from one immediate subject to another," (p. 34,) and although we think we understand what he means when he says so, we must confess that we find it as hard work to keep up with him as if his "course" were the most unnatural in the world, and his subjects were anything but "immediate." We must, therefore, be content to pick our way the best we can, and hop, skip, and jump with him backwards and forwards as he pleases. After his digression on diet, he jumps back to *vomiting;* of which he describes numerous varieties. This is one:

"Vomiting is also a prominent symptom in some cases of phthisis pulmonalis,—nay, in some rare instances, I have seen it so severe, as to draw the patient's attention and that of his friends entirely away from the real disease. Vomiting in phthisis pulmonalis, occurring almost always after coughing, if it arise early in

the disease, is the proof of a severe and rapid form of it; if late, *it betokens that large collections of matter are locked up in the lungs*, that is, have not yet found an outlet through the larger branches of the bronchi. I have had occasion to point out this symptom during life, and to illustrate its cause after death, to the pupils of the hospital." (p. 41.)

In illustration of this statement a case is given, of what Dr. S. calls "phthisis pulmonalis of the languid kind," and which exhibits just such blundering as must often, of necessity, follow the rejection of auscultation in practice.

"Sarsaparilla, followed by gentle tonics, and afterwards quina, with a mild and nutritious diet, and medicine to allay the cough, *appeared* at the end of two months *to have effected a very material change*. The sweats had disappeared, there was increased strength, and *it was proposed to the patient's anxious friends that, to avoid the March winds, he should be removed to Hastings*. The last consultation, before this plan was to be carried into effect, took place between the late Dr. Nevinson and myself. I remarked—'Well, he is wonderfully improved to all appearance, but I never can think this change otherwise than delusive so long as he continues to vomit at the conclusion of a fit of coughing.' 'It is a very disagreeable symptom,' said Dr. N., 'but it is the only one?' About eight and forty hours after this conversation a vomica gave way in a fit of coughing, and more than a pint of purulent matter was brought up. I say a pint, for it nearly filled one of the large finger-glasses used at table. The patient died four days after this event, the more violent symptoms of hectic immediately following the discharge." (pp. 42-3.)

How different, in all probability, would have been the prognosis here, and also the advice, had the physicians only condescended to percuss the patient's chest or apply the stethoscope but for a few minutes! And we could adduce several other illustrations of the same kind from this volume, all showing the miserable shifts to which the author has reduced himself by discarding auscultation from his practice. Without stopping to inquire what Dr. Seymour means by a "vomica," and thinking it very probable that the pathological state in the case in question was quite different from what he supposed it to be, is it not melancholy to see a physician in these times, looking to a mere sympathetic symptom like vomiting, for evidence (conjectural and circumstantial at best) of a great organic change in a vital organ, when positive demonstration of its existence and amount could be at once obtained? Dr. Seymour, like the late Sir Matthew Tierney, a name of authority in the fashionable world in the days "when George the Fourth was King," though now forgotten, no doubt considers the practice of auscultation as unscientific and undignified, fit only for "low mechanicals," and debasing the noble profession of physic. To simple and humble-minded men like ourselves, however, this flying from what is easy, clear, and certain, to what is difficult, obscure, and problematical, seems egregious trifling: and Dr. Seymour's diagnosis of what he calls Vomica, by means of the symptom Vomiting, reminds us of the super-scientific tailors of Laputa, who, disdaining the vulgar modes of fashioning garments by direct *mensuration* and *palpation*, took measure of their patients by the quadrant and astrolabe—a highly ingenious proceeding, doubtless, but resulting, as the veracious historian tells us, in marvellously ill-fitting clothes.

Dr. Seymour says that there is a peculiar kind of vomiting, which is

pathognomonic of the disease which has been called by Dr. Baron, "tuberculated accretion of serous membranes," or chronic tubercular peritonitis. This vomiting is of "a green so dark that it is only to be compared with that which the sea acquires at great depths; or blue, as intense nearly as that of indigo; deep green when regarded in one way, blue in the other." (p. 47.) He does not say that it is always present, but only "when it does occur, it is diagnostic of disease." This affection, he truly tells us, in his own odd way, is "by no means uncommon in young people, especially females, and not very uncommon in youthful adults" (p. 46); and we agree with him as to the mistake so commonly made of confounding it with disease of the mesenteric glands. We fear, however, that further experience will yet teach Dr. Seymour that his "peculiar vomiting" is not pathognomonic of this disease, but that it attends more than one other affection of the abdomen.

After what they have already seen, our readers will not be surprised to learn that the author brings into his book, under the head of diseases characterised by vomiting, *Asiatic cholera.* We think, however, they will hardly expect to read such platitudes as Dr. Seymour gives us on the treatment of this disease (and we extract *all* he says of Asiatic cholera); any more than he could expect such a specimen of the Mrs.-Gamp-school of pathology as the next extract presents; all, too, written in the year of grace, 1847, and by a learned Fellow of the College of Physicians, and in what he terms "a paper purely practical." We need hardly call attention to the accuracy, elegance, and precision of style in which the information is conveyed: he that runs may read.

"I have spoken of a peculiar coloured vomiting, which, when it is present, distinguishes the disease known by the name of 'tuberculated accretion of the peritoneum;' but in the most frightful disease with which modern physicians have become acquainted, Asiatic cholera, the vomiting which characterises it is entirely devoid of colour, like thin gruel, or water in which rice has been boiled; and where it is tinged with bile it is considered to be a favorable symptom. Various modes, in the hands of different practitioners, have been used to stop this discharge or correct its quality. In India, it would appear that calomel and opium most frequently succeeded. (See a paper in the Medical and Chirurgical Transactions, communicated by the late Sir Gilbert Blane.) In the disease which visited Europe in the year 1830, and England in 1831, *great good was expected* from evacuating the stomach early by a mustard emetic; others found more advantage from very small doses of Epsom salts, ℈ss. and magnesia, gr. v, with two or three minims of laudanum in a table-spoonful of water, every three or four hours, or oftener,—a mixture which has been found very useful in inordinate vomiting in the diseases already mentioned, and also in the dysentery and diarrhœa of India. It is very remarkable, that the very remedies which, on the first onset of the disease, were utterly useless, were found very beneficial *as the time became longer* and the disease progressed. In all the northern countries, and in our own, the same proportion of deaths occurred during the first month of the invasion of the disease. This diminished greatly as time advanced, and towards its disappearance the proportion of those who recovered, to those attacked, bore a very large ratio to the mortality which happened in the first month." (pp. 56-7.)

"The pain at the pit of the stomach [in chronic gastritis] is equally a symptom of distended liver; and it is not easy to understand how a viscus supplied by the same vessels as those which supply the liver and spleen can be inflamed even for a short time without these viscera suffering. The stomach may, indeed, suffer from alterations in its peculiar secretions, especially in their chemical composi-

tion; but, that inflammation should occur in a chronic form, without affecting the neighbouring viscera, fed and nourished from the same trunk, seems difficult to believe." (p. 63.)

II. The section on GOUT occupies nearly eighty pages, all of which we have gone over and over again, without being able to find any one new truth in pathology or practice, or an observation of any kind that could either justify or account for its publication. The "thoughts" are here less disturbed in their "natural course" than they were when the author was dealing with the multiform affections of the stomach; still they are very flighty and vagrant, and what is meant for ratiocination is not a whit more consequent or logical. The language seems not at all improved as the work advances, as our extracts will probably show. It is in the following strain that Dr. Seymour introduces his subject:

"Experience in my profession for more than twenty years has shown me many cases of gout in the classes of society where it is the most frequent in its various forms, whether of positive disease or constitutional disposition. The following remarks contain my observations as a physician, and formerly a teacher, on this frequent and most painful disease: still, to write observations on this subject appears to be the commencement of an undertaking more than ordinarily imprudent. The disease was well known to the ancient physicians, as I shall often have occasion to mention. In modern times, and in England alone, when I say there are upwards of 120 treatises on this disease (I speak very much below the real number), does it not appear presumptuous to add to this number, in the hope of saying anything new? I do not know that I can say anything new, but the arrangement of our present knowledge on the subject, our treatment and the reasons for that treatment, are not satisfactorily stated, in my own view of the case, concisely and for practical use. It is to supply this (temporary if it be) want, that having experience on this subject, I undertake the task of making the following observations. This disease was first known to dissolute society, where good living and indolence were mingled together, and it descended to the offspring of those who indulged; and as indulgence increased with luxury, so the disposition to transmit acquired disease was established. In fact, this disease was principally restricted to the rich and powerful of every age, and hence became the object of interest to physicians, and attracted their attention.....

"If, in the course of two thousand years, remedies, especially from the vegetable kingdom (which in such a space of time has undergone no alteration), have been recommended by the wisest of their day, have fallen into disrepute, buried or forgotten, have again been revived and used with great advantage, and perhaps this has happened a third time,—such a confirmation of their utility in different ages, under different theories, and in the habits of different nations, ought to make them acquire a greater stamp of value, rather than the contempt attributed to implements invented in an early and a savage state.....

"What is gout? More than two centuries have elapsed, during which men, versed in various kinds of learning, and while new discoveries were pouring in, afforded a satisfactory reply for the moment to this question without attempting to give a full answer." (pp. 71-5.)

"Gout is not *the result* of a natural state of things; nor do I believe it ever exists in a state of nature. It is the scourge of civilized life; and hence is mixed with the pleasures and enjoyments of the table, and the cares and exertions of ambition." (p. 142.)

We entirely agree with the author in thinking that for him, to "write observations on this subject is an undertaking more than ordinarily imprudent;" but we hope he really does not mean that this is only "the commencement" of the undertaking! We would also fain hope that however

"dissolute" our good society may be, we may continue to get on without any more of the "thoughts" or "observations" here "commenced," more especially as gout will always be an "object of interest to physicians *and* attract attention;" and we humbly trust—whatever Dr. Seymour's fears may be—that "the vegetable kingdom" which supplies us with our remedies, will "undergo no alteration" even "in the course of two thousand years," but, by continuing to yield colchicum, burdock, &c., may enable us to give "a satisfactory reply, though not a full answer" to all who shall seek reasons for the faith that is in us.

After a miserably meager, and very incorrect view of the established opinions respecting the pathology of gout, the author comes down to his own proper sphere—that of telling us what his own eyes have seen in ordinary life and common practice,—and gives us the following very good account of a first attack of gout. *O si sic omnia!*

"At a period generally between thirty and forty years of age, earlier if hereditary, later if arising for the first time, the patient, usually a good liver, of sedentary occupations, and who has suffered most frequently what is commonly called 'bilious complaints,'—that is, he has suffered much from dining out frequently, and eating and drinking largely,—has acid in the stomach, flatulence, and a fear, not of the quantity he eats, but of the quality: saying, that he is a most careful man, always eats plain things, mutton or beef; never tastes sweet things, never eats butter, and considers himself a paragon of care: but he does not say that he works all day and part of the night—perhaps in the House of Commons—eats late of fish and soup, and then heartily of *beef* and *mutton*, and potatoes, and porter, and being worn out, takes some wine afterwards. Well, all this is very well, if he rode six hours a day, or followed the hounds, or walked many hours. But the nutrition is constantly introduced, while the exhalation from the skin produced by exercise is wanting; the regularity of the alvine and renal excretions is all in abeyance. Such a man, between forty and fifty, having been low-spirited, flatulent, and unable to eat, with heartburn, is perhaps, after exposure to cold, perhaps after a late party, perhaps after sitting up late writing, much against his wishes, attacked about two o'clock in the morning with acute pain, acute enough to make him cry out, in one foot." (pp. 80-1.)

We cannot find a single novelty in the account Dr. Seymour gives of the treatment of gout, which he truly and quaintly tells us "has varied little since the earlier knowledge of the disease, *except from occasional violent practices.*" It "consists in keeping the bowels open with warm and aromatic purgatives, in regulating the diet, and, during the fit, employing opiates to relieve pain, and colchicum to diminish inflammation." (p. 87.) He is a great advocate for colchicum, and combats the opinion of its being injurious to the constitution when used in moderation. "In *my opinion*, (he says) the wine of the root and the acetous extract, are the most frequently and effectually employed." p. 96. Dr. Seymour draws a just distinction between acquired and hereditary gout in regard to practice. The following extract may be useful to the young practitioner, as giving in a few words all that is essential in the treatment of a paroxysm—though, indeed, all his teachers must have told him the same thing long ago:

"Speaking, then, first of the gout which is acquired, or the case in which gout in the family is a very remote circumstance, the following is ordinarily the practice:—The bowels are to be freely opened with calomel, colocynth, and scammony (℞ Hydrarg. Chlorid.; Pulv. Gummi Scamm. aa. gr. ij; Extr. Colocynth. Comp. gr. iv. M. Ft. pilulæ, ij h. s. s.), and then the colchicum is to be given, about

twenty drops of the wine twice daily, and at night three grains of the acetous extract, with five grains or more of Dover's powder, according to the *nightly pain:* this must be soothed at all events. This course may be continued. The purgative should be given every second or third night, and the patient kept on low diet; and in a first attack from six to ten days is the usual duration of the paroxysm. But where the attack (without marked hereditary taint) has often been repeated, it will be better to give a dose of colchicum only at bedtime, combined with an opiate; taking every alternate morning a dose of Cheltenham salts in *warm* water to obviate constipation. ℞ Vini Rad. Colchici, ʒss.; Solut. Acetatis Morphiæ, ℥xx (gr. ½); Aquæ Flor. Aurantii, ʒj; Aquæ Fontanæ, ʒx; Syrupi Aurantii, ʒj. M. Ft. haust. h. s. s. o. n. If, indeed, fever runs very high, with hot skin, pulse very quick, and great febrile distress, to the night draught may be added from twenty to thirty drops of the liquor antimonii tartarizat. with good effect. Variations of this treatment will generally meet all the difficulties of regular painful gout in the paroxysm; still, after several attacks, more must be thought of." (pp. 99-101.)

Dr. Seymour dwells much too little on the *preventive treatment* of gout, or the treatment in the intervals; although this is infinitely more important than that of the paroxysm. His practice is the common every-day routine, which is good enough as far as it goes, but very bad when it is considered as, in any degree, a substitute for that non-medicinal treatment which can alone lead to results of any substantial and permanent value. Here is the medicinal treatment; and it is really curious that the author could not tell us so simple a thing as this without puzzling us as to his meaning and his grammar, twice at least in his two sentences.

"The usual practice, and a most effectual one, at present, in the intervals of gout, is to endeavour so to act upon the stomach as to prevent the formation of acid, which first arising in the stomach, seems to point out the morbid condition of the secretions of the bowels, kidneys, and skin. For this purpose, various combinations of a fixed alkali with a vegetable bitter, some warm stomachic made gently purgative, seems to fulfil the intention of the ancient physicians with the alterations shown by modern chemistry. Thus, rhubarb in all its forms has been joined with soda or potash, and with cinnamon or ginger." (p. 130.)

To be sure, we have also a brief account of the effects of warm and a much longer one of the effects of cold bathing, (including the new system of Hydropathy) both of which are commended; but we desiderate that complete and combined system of action in regard to regimen and general hygiene, which can alone justify the confidence of physiological physicians. When treating of hydropathy, (in which Dr. Seymour seems to us to possess a reasonable, qualified confidence,) he makes the following sensible observations, which will remind some of our readers of the article in our 44th Number.

"Now let us look at the question as it applies in general, and as regards that class of society who are affected with gout. A man worn out with fatigue, and living in this close town,—perhaps an artist, with warm and vivid feeling, toiling all day, and finishing the evening with supper and spirits and water, and perhaps cigars,—becomes hypochondriacal, loses appetite, and loses spirits. He is recommended to a hydropathic institution in the country. He *must* remain there a fortnight or three weeks. He is at leisure; he uses the alternations of heat and cold; the skin acts plentifully; he has good wholesome diet, not in excess, meat and vegetables, in a beautiful country, with clear, pure air, and early rising, and cheerful society, &c., drinks water in considerable quantities, and no wine. Can anything be imagined more likely to restore his spirits, tone down his irritability, release him from anxious thoughts, at the very time that the novelty of the treat-

ment makes him consider himself to be in some degree an object of observation? Can any one doubt that this must, in many cases, be useful?" (p. 141.)

We think not. But it is but justice to Dr. Seymour, as well as to ourselves, to add, that the cases must be carefully selected for this practice, and also, that the practice must be applied and modified according to the exigencies of individual cases, and the laws of physiology and pathology. Therefore our author justly adds to the above extract:

"But, had the seeker after health a diseased heart or diseased lungs without his knowledge, or repressed gout, diabetes, or diseased kidneys, or dropsy from diseased liver, would these effects have been produced? Certainly not. It will restore impaired and faded strength, and with effect exactly proportionate to the mental wear and tear of the sufferer, but nothing more; nay, it will do harm if urged beyond the circle of functional disorders." (pp. 141-2.)

III. The third section of Dr. Seymour's work, which occupies about eighty pages, has this lengthy, clumsy, and not very intelligible title prefixed to it, in capitals:

"On mental derangement, especially that form termed melancholia, or the alternation of melancholia with violence, termed lypomania, or suicidal madness, especially in reference to the treatment of this form of disease, by the employment of opiates, or of that class of medicines called sedatives."

Without stopping to inquire on what authority Dr. Seymour makes lypomania and suicidal mania one and the same disease—for we presume this is the meaning of his words,—or to criticise his subsequent observations on the nature of mania in general, we proceed at once to notice the principal feature of the essay, the use of opiates in insanity. We entirely agree with the author in thinking that where there exists no manifest organic affection of the brain, in "this the most afflicting disease in human nature," "we are bound to apply the resources of our art to its cure" (p. 153); that is, to try the effect of medicinal as well as moral means in its treatment. The great medicament in the author's opinion is opium. We ourselves believe also that it is a very valuable remedy in certain cases; but the difficulty is, to recognise the fit cases in practice. Had we found that Dr. Seymour had here done what he professes to do, and probably thinks he has done, viz. "to point out the cases in which such treatment is or is not admissible" (p. 155), we should have acknowledged that he had rendered a great service to practical medicine and to afflicted humanity. We are, however, compelled to say, after carefully studying his book, that we can find no such indication fairly laid down in it. The muddle-headedness, and puzzle-headedness so characteristic of the whole volume, are fully as conspicuous in the essay of insanity as in the other parts of it; and, after turning the pages backwards and forwards half a score of times, we gave up the task in despair of finding what we so eagerly sought for. The following passages state clearly the mode in which Dr. Seymour administers the remedy; and one expression contained in the extract, as well as the title of the section formerly quoted, authorises us to conclude that it is in cases of *melancholia* that this mode of treatment is recommended; but we can find no clear definition of the forms or varieties in which it is especially applicable.

"The preparation which I have preferred, and, with two or three exceptions, I have always used, is the acetate of morphia; the mode of preparation the solution: forty drops of the solution which I have generally employed contain one

grain of the alkaloid salt. It has generally been, in mild cases, my practice to begin by a quarter of a grain every night in solution; then, after a week, to increase this to half a grain: it has rarely in such cases been necessary to increase the dose beyond half a grain. In severe cases I begin with half a grain (twenty drops), and increase it speedily to a grain (forty drops); rarely, most rarely, beyond this dose. The medicine is given at bed-time, and only at bed-time, the period which is intended for sleep; but, it must be repeated, without the *intermission* of a single night, for several weeks in mild cases, for at the least three months in the most severe ones.—In some of these cases at first sleep is not produced; in very few *rest* is not produced. Slight nausea, and disturbance of the head, are felt the first few mornings, but in these cases almost always at first, and *always after* a short time, both sleep is procured and the waking hours are free from pain.

"The effect of the medicine is in precise analogy with what follows. Suppose a man toiling with professional anxieties, and with domestic cares, returns home, after a larger proportion than usual of the annoyances of his profession or calling, fatigued beyond his powers, wearied in mind. He returns to rest unhappy, discontented, inveighing against his lot, and what he considers to be his peculiar cares. He sleeps sound, and when about to rise in the morning, the sun streaming in at the windows, after a sound sleep, how does he look upon the evils of the preceding day? do they not lose a large portion of their affliction? does he not look in a totally different point of view at the very causes of distress which afflicted him the night before?

"And this is precisely what the effect of morphia, properly employed, effects in cases of melancholy mental derangement, but not once or twice, as would be the case in trifling distress. Hence it must be repeated regularly every night until the nervous system is soothed! Thus it requires weeks for the medicine to be repeated regularly, even without a single night's intermission, and the cure, as I shall show by examples, is the result." (pp. 159-61.)

"It is to be used in small quantities, regularly repeated, and never increased *beyond a certain point, whether taken for six weeks or six years!* I may say, with the greatest truth, I have never seen one single case where the treatment has succeeded, or where it has failed, that the smallest injury has been done, nor in a single case where the medicine has been continued beyond the time when health was re-established." (p. 163.)

Now, if Dr. Seymour could, indeed, "show us by examples," that "the cure" of melancholia generally is "the result" of such treatment, he should assuredly be our great Apollo in the treatment of insanity. He tells us, it is true, that during the last fifteen years, he has treated about ninety cases in this manner, and has thus cured by far the greater number.

"As fifteen years have elapsed.....during which I have been anxiously watching the result of cases, especially of the recoveries, I can now with greater certainty point out the best mode of using this particular treatment. Upwards of seventy cases during that period of time have recovered; and I consider no case to be called a recovery unless two years at least of unabated health have elapsed since the treatment concluded. In nearly twenty cases the treatment has failed, or only given temporary relief." (p. 156.)

If there existed no source of fallacy in the grounds on which this statement is based, we should regard it as extremely important, notwithstanding our own experience and that of still higher practical authorities is far from being in accordance with it. But there is a most obvious source of fallacy in the premises, which completely invalidates the conclusions deduced from them,—at least in a large number of the cases: viz. *the very*

long period during which the remedy was employed. Every physician conversant with insanity knows that a considerable proportion of cases of melancholia, when observed in their earlier stages, recover within the period assigned by Dr. Seymour to the administration of his daily opiate viz. *months or years.* What right has Dr. Seymour to assume that a recovery that has ensued after months of his opiate treatment would not have ensued without it? Before we can be called on to admit this, we must not only have much more extensive statistics than he has given us of cases treated by opiates, but an equally large category of similar cases *treated otherwise, and with inferior results.* We are far from denying that the sedative treatment is beneficial in certain cases; and we admit that it was evidently so in some of the cases narrated in this book; but we feel it to be our duty strongly to caution our readers against being misled by Dr. Seymour's statements, as we are convinced that his treatment, if generally adopted, will not merely disappoint expectation, but be positively injurious. As an occasional remedy in the proper cases, and employed for a limited time, we regard it as valuable; but we believe the proportion of cases in which it is more valuable than other and commoner modes of treatment, is very small. It has, moreover, the great disadvantage attaching to all empirical and so-called specific remedies,—that, namely, of engendering and fostering a blind, uninquiring confidence, and sleepy satisfaction with our own proceedings, subversive of all rational treatment, and fatal to all improvement. We have ourselves, met with more than one lamentable example of this result, where patients had been pursuing this Seymourian treatment, not merely for months but years,—and not only in cases of melancholia, but in cases of mania and even of imbecility! Dr. Conolly, Dr. Hutcheson, and all the superintendents of large institutions whom we have consulted on this subject, object to the long-continued opiates recommended by Dr. Seymour; although all are in the constant habit of using them as occasional remedies, and with benefit in particular cases. The general feeling among the gentlemen of largest experience is, that the best sedatives and soporifics, speaking generally, are not to be found among drugs. The following are some of Dr. Conolly's views on sedatives:

"One of the best preparatives for good sleep," says Dr. Conolly, "is exercise in the open air; and an evening walk may act as a sedative. Rest at night is also more likely to be enjoyed by those who have not been irritated in the day, or before going to bed, or sent to bed cold or hungry, or thirsty and feverish. A kind manner, a little bread, sometimes a supper of bread and cheese and porter, or ale; or arrow-root with a little brandy; sometimes a glass of water, or a warm bath may be better than any sedative in the materia medica."*

"Any resident officer who frequents the wards late at night will learn that there are many of the patients to whom a cup of tea, or of broth, or of sago, or sometimes a glass of water, or sometimes a few kind words, act as an immediate sedative, and contribute also to the great end of producing satisfaction in the day, as well as solace in the night. I have repeatedly witnessed the good effects of such attentions, and have known violent and noisy patients tranquillized for the night by being allowed water to wash their hands and face, and water to drink, and by having the bed made up afresh, and a cool dress to put on, and a cheerful good-night wished to them by kind-hearted attendants. To withhold all this, as if it were so much weak indulgence of the capricious, is to treat the insane as criminals, not as patients, and cannot be too strongly reprobated."†

* The Construction and Government of Lunatic Asylums, p. 99. † Ibid. p. 103.

We believe that Dr. Seymour's observations on "the effect of travelling, or what is called variety," in melancholia are correct.

"Thus it is, and thus it has been forced on my mind, in a great number of examples, both of men and women, that change of place and variety is eminently injurious as a means of cure, until the melancholy hallucinations are completely at an end." (p. 179.)

Under the head of employment, Dr. Seymour tells us that "the fact of being able and willing to work, is a *prima facie* evidence that their mental disease is curable." (p. 181.) This idea we suppose is derived from his experience as a commissioner of lunacy; but we believe that at the time he held that office opportunities of employment were rarely afforded. He also speaks of "compulsory employment;" but who ever recommended it?

We entirely demur to the recommendation (p. 184) of applying leeches to the os uteri in the case of unmarried women. We believe that leeches applied to the sacrum or pubes, in suppressio mensium, and in hysterical mania, are often as efficacious as when applied to the os uteri; and in every respect less objectionable. These unfortunate women, the hysterical especially, are submitted to many needless and cruel measures, and in Parisian practice, to many villanous methods of treatment, of which the effects are always lamentable, and, we believe, not seldom fatal.

Dr. Seymour makes some just and important observations on the relative value of seclusion in one's own house, in private houses, and in asylums. We are, however, astounded at the following statement: "I must, however, in common justice, premise that during my period of duty as a commissioner of lunacy, I never witnessed one case of *cruelty*. The greatest care was taken, the most assiduous, nay, the most invidious (to innocent people) inquiries were made." We think it shows how little a commissioner may know of what passes in the sphere of his duties. It is discredited by the records of numerous inquests, and by the state in which, even now, patients are brought to public asylums from private asylums. We are disposed to agree with Dr. Seymour in demurring to the truth or justice of the following propositions:

"1st, then, it has become a system, partly from tradition, partly from usage, no physician taking it up, that no one can recover except when removed from his family.

"2d. That it is not only desirable, but necessary, that the patient should not see his friends, except on visits very few and far between, during his confinement.

"3d. An extreme disinclination to let the patient have a trial; that is, to be removed home." (p. 218.)

In his examination of the first of these objections, he says:

"Something is to be attributed to the indisposition to undertake a difficult and unmanageable case by men actively engaged in the profession. They answer, 'really this is a case I am not accustomed to; you must send to one of those gentlemen who make it their exclusive study.' Now, this is precisely what I think to be erroneous: it is because they make it their exclusive study, that I think they are less good judges than they otherwise would be of the *cure* of such cases." (pp. 218-19.)

It is surely a strange doctrine this, that persons should be worse judges of any matter in proportion to their experience, observation, and reflection! What will Dr. Conolly, Dr. Sutherland, or Sir Alexander Morison say to this doctrine? On the same principle of *lucus a non lucendo*, we do not

doubt that Dr. Seymour considers himself much better qualified to give an accurate diagnosis in diseases of the chest, and consequently to treat them more scientifically, than Sir James Clark, Dr. Latham, Dr. Williams, or Dr. Walshe.

IV. The last essay in the volume, that on SCIATICA, extends only to thirty-six pages, and is by much the best of the four. Although it contains nothing that will be new to any practical physician of experience, we think the young practitioner may read it with advantage. It has qualities which the reader of the other essays would hardly expect to find in it. Though abounding in errors of style, it is written clearly and intelligibly enough; the thoughts even run in natural sequence; and, what is still more remarkable, the disease and its treatment are studied and expounded in a rational pathological manner, and not merely empirically. With some of the pathology we cannot agree; but we are disposed to regard the practice recommended, as that which most men of experience will be disposed to sanction.

The all-important point in setting about the treatment of a case of sciatica, is to know what the disease is we are called on to treat, as other diseases of an entirely different kind are sometimes confounded with it, while sciatica, even that properly so called, varies greatly in its nature in different cases. Dr. Seymour notices the distinctive symptoms between sciatica and the two affections with which it is most apt to be confounded, viz., diseases of the hip-joint and disease of the kidney.

"Having thus ascertained that pain in the course of the sciatic nerve is neither connected with incipient disease of the hip-joint, nor dependent on irritating secretion in the urine, the physician must proceed to inquire whence it proceeds, and whether it is idiopathic, that is, depending on the state of the nerve itself, or symptomatic of disorder of the stomach, or of syphilitic taint, or, finally, whether it is the first indication of disease in the brain itself. The first attention should be paid to the general health of the patient. Is he robust, living well, taking much food and little exercise, having enjoyed good health for many years? If this be the case, it is more than probable that a brisk succession of purgatives will cure the disease; such pain, if on the right side, occasionally depending on distension and obstruction in the caput coli; or, if it has succeeded to cold blasts, upon the body being in the state mentioned, then cupping, followed by blisters, will be useful to relieve the pain; or the vapour-bath, or diaphoretic remedies carefully continued, will cure the case." (pp. 229-30.)

When the disease occurs from over-exertion in walking, in a person unaccustomed to exercise, and of a weak disordered habit, "the painful affection of the nerve is to be soothed by sedatives, while the muscular force of the body is upheld by nourishment and strengthening medicines." (p. 230.)

"Is the patient a gouty man? Has he suffered from gout? If he has, and this painful affection occurs in an interval of the disease, it may fairly be classed with other cases of erratic gout, and treated accordingly. The various preparations of colchicum will be of great importance; but in the cases which I have witnessed (and they were very severe), quinæ sulphas in large doses given in the day time, with thirty drops of the wine of colchicum in a draught at night, were most effectual; but neither separately produced permanent relief." (pp. 231-2.)

Sciatica, like other forms of neuralgia, may arise from many other disorders of the system, and must be treated accordingly. But it exists also in an idiopathic form, "without any discoverable cause at a distance."

Dr. Seymour is "inclined to be of the opinion" of Cotunnius—of whom he says in his own droll manner, "He lived to a great age—*hence* his work may be looked upon as an *ancient* classic,"—that the disease in certain cases "arises principally from effusion of fluid into the sheath of the nerves;" and he thinks that acupuncture, which he strongly recommends, may act by "setting free the fluid." Dr. Seymour passes in review all the remedies, in ordinary use for tic douloureux, but tells us nothing new; and tells us much less and less satisfactorily than we are told by Dr. Hunt and others who have written *ex professo* on the disease. But the most important part of Dr. Seymour's observations is that which refers to Sciatica, "as symptomatic of disease of the brain." He says he has seen five cases of this sort, in some of which, however, the pain was seated in the brachial nerves; and he has given notes of two. In both of these, vomiting supervened, and is considered by Dr. Seymour as indicative of the cerebral cause. As, however, Dr. Seymour had, *more suo*, prescribed sedatives in these cases, it was doubtful whether the vomiting might not be attributed to them. In common with other physicians, we have ourselves met with similar cases, and have been more than once not merely puzzled but at first mistaken, as to the true character of the disease. We are sorry to find that Dr. Seymour gives us no help towards the more accurate discrimination of such cases in future.

Although Dr. Seymour may not be inclined to follow any advice we may give, we shall, nevertheless, advise him to be content with what he has already done, and eschew printing for the future. He may rest assured that such is the most friendly advice that could be given to him. But if, as coming from us, he should (wrongly) imagine it not to be friendly, we would remind him that, like a nauseous medicine, it might yet be beneficial—*fas est ab hoste doceri*. If, however, he is resolved to carry out the plan he has announced, giving us, according to the positive declaration of the title-page, one more volume, or, according to the conditional threat in the preface, two more volumes, we trust that he will, at least, take means to convey to us his future Thoughts in a form less offensive to good taste and less obnoxious to criticism, than the form in which they reach us in the present. From some passages in the volume before us—and we have extracted the best of these—we have a suspicion that Dr. Seymour could write better if he chose; but whether he can not, or can and will not, we do hope that, for the sake of his order and the character of the medical literature of the country, as well as out of regard to the interests of those he desires to instruct, if not for his own reputation, he will not permit the succeeding volume or volumes to be like the present. If not aware of the fact, we take the liberty of informing him, and on the unimpeachable authority of the 'Times' advertising columns, that there are hundreds of "men of letters" in this metropolis, who will be delighted to see his or any other person's MSS. safely through the press, in creditable and workman-like manner. Or should he prefer a less formal and more economical proceeding, we doubt not that he will find among the present pupils of his old hospital, more than one who have not yet forgot their grammars, and who will be proud to be allowed to translate his galimatia into the vernacular tongue.

Art. X.

The Construction and Government of Lunatic Asylums and Hospitals for the Insane. By John Conolly, M.D., &c. With Plans.—*London*, 1847. 8vo, pp. 184.

Led by the association of subject, on laying down the book which formed the basis of our last article, we very naturally took up the present: both books treating of Insanity; and both the authors being physicians who are known to have been engaged in the treatment of this disease. But what a contrast between the books! Dr. Conolly's possesses every merit which Dr. Seymour's wants. It has, indeed, as many merits as the latter has defects, and we could hardly pay it a higher compliment. The subjects treated of by Dr. Conolly are just as clearly conceived and as logically arranged as Dr. Seymour's subjects are the reverse; and the style in which the information is conveyed is to the full as accurate, clear, and elegant, as Dr. Seymour's is inaccurate, obscure, and vulgar. In a word, if we have said that Dr. Seymour's book is creditable neither to its author nor to our literature, we say, with equal confidence, that Dr. Conolly's is in the highest degree creditable and honorable to both.

Although the contents of Dr. Conolly's little volume are strictly in accordance with its title, and treats mainly and most fully of everything relating to the Management and Government of Asylums, it also contains, incidentally but necessarily, a great deal on the general treatment of the insane. It must not therefore be imagined that it is exclusively interesting to those having immediate connexion with asylums. To all, indeed, who have such a connexion, whether lay or professional—whether as physicians, surgeons, chaplains, matrons, attendants; or as governors, magistrates, visitors, committeemen, projectors, or architects of asylums, —it must prove invaluable, as containing all that they require to know, and much of the greatest possible importance that they should know, and which they cannot find elsewhere. But to the medical profession generally, it presents not only a subject of great interest admirably handled, but it offers much valuable matter relating to the character of the insane and the mode of treating them, which every member should be acquainted with.

In a prefatory note the author tells us that "the greater part of the book is a reprint from Lectures published in the Lancet, in 1846, supplementary to a clinical course on the symptoms and treatment of the various forms of insanity." We should much more regret that Dr. Conolly has restricted the republication to these few lectures, did we not entertain a hope that he will give us the others on the general subject of insanity, on some future occasion, in an enlarged form. To him, of all men, the great and influential promulgator and promoter, if not the actual introducer, of the humane system of treating lunatics, which does such honour to our time, the profession looks for information on all that relates to the nature and cure of insanity. As members of that profession, we do so likewise; and we shall, therefore, choose to regard the present publication—invaluable as it is—only as a small instalment of the great sum of information which we would fain fix on our author as an imperative debt to his brethren, yet to be cancelled.

As it would be impossible to convey to our readers the manifold minute yet most important details respecting the construction and arrangement of asylums, without transcribing the greater portion of the chapters devoted to these, we must content ourselves with a strong recommendation to all interested in these subjects, to study the original. As we go over the different chapters we shall merely pick out, here and there, some of those passages which have a more general bearing on the treatment of insanity. These will speak for themselves as to the elegant and refined style of the work, and will give a general idea of the gentle and philanthropic spirit in which it is conceived. They will, however, convey but a faint impression of the depth and tenderness of the sympathies of the author towards the poor and beloved subjects of his care, with which the whole volume abounds. The book, indeed, presents us with a striking contrast in the manner in which the author speaks of his patients and their attendants on the one hand, and of the governing and ruling authorities of asylums, on the other. He uses the utmost freedom in commenting on the proceedings and shortcomings of governors, matrons, &c., but he lets slip no occasion of defending the claims and rights of his poor patients and their hard-worked attendants,—or rather, we should say, his feelings of affection and sympathy towards them, well out from his mind on all occasions unconsciously, and irresistibly.

The following extract is the conclusion of the author's obsevations respecting the best site for an asylum; it shows how his mind delghts to take the loftiest as well as the gentlest views of things.

"Patients of all classes derive advantage from the circumstances of situation; and if it is intended to receive patients of the educated classes into the house, it should unquestionably be situated amidst scenery calculated to give pleasure to such persons when of sane mind. Those whose faculties have never been cultivated derive little satisfaction from the loveliest aspects of nature, and experience little emotion amidst the grandest. The sun rises and sets, the stars shine and fall, the hills reflect all the variety of brightness and shadow, of wildness and of verdure, and yet are scarcely noticed with more than mere passing attention. But when education has called the higher faculties into life, impressions upon them, even from external nature, become powerful for good or ill, and in the case of a mind diseased, may act as remedies, or aggravate the malady. The celebrated Robert Hall attributed much of his unhappy state of mind, and even his temporary insanity, to a change of residence from a picturesque and interesting part of the country to a cheerless plain, of which the dulness, flatness, and invariable monotony saddened his heart. Cowper, whose writings indicate exquisite sympathy with the sights and sonnds of common rural retirement, and who, like Robert Hall, was occasionally afflicted with insanity, felt awe-struck and overwhelmed when visiting a friend whose house was situated among lofty hills covered with trees. There are few persons of any degree of education much of whose daily and habitual pleasure does not arise from the view of the objects around them; and the first desire of all who can quit the crowd and toil of business, is to be where they can enjoy 'a prospect,' or to surround their houses with shrubs and flowers. Even in the populous city, the pent-up artisan has a bird, to sing to him whilst he works, and a few flowers, which he cultivates with care. We must not neglect such instincts and capacities if we profess to cure diseased minds. Our practice can only securely rest on the consideration of everything, great or little, capable of affecting the mind beneficially or hurtfully." (pp. 9-10.)

The following extracts show the importance of minute things as calculated to affect the feelings of patients:

"Much ornament or decoration, external or internal, is useless, and rather offends irritable patients than gives any satisfaction to the more contented. In some of the Italian asylums, busts, pictures, and ornaments abound, and the walls are painted with figures representing various allegories or histories. These would appear more likely to rouse morbid associations than to do any good. I also disapprove of painting numbers and titles on the walls of airing-courts, by which the walls are disfigured, and the patients are reminded of their confinement as insane persons when walking out for relaxation, or led to consider themselves prisoners. The wards and bedrooms also should only be designated by simple numbers.

"When it is remembered that many patients are sent to an asylum whose senses are as perfect, and whose feelings are as acute, as those of sane people, and that from the moment they enter the outer gate everything becomes remedial with them, or the reverse, the reason will at once be seen why the external aspect of an asylum should be more cheerful than imposing, more resembling a well-built hospital than a place of seclusion or imprisonment. It should be surrounded by gardens or a farm. Even the part of the building to which the patient is first admitted is important. In the new asylum about to be built in the county of Middlesex, I feel sure that the magistrates will anxiously avoid an inconvenience which is very much felt in the present building, where the reception rooms open directly into large towers, in which a circular staircase is so guarded with iron palisades as to give the patients looking through them, in the three different stories, the appearance of persons shut up in tiers of iron cages. A very painful impression is often made by this prospect being suddenly presented to the new comer; and it is strongest in those in whom the hope of recovery renders the avoidance of all counteracting agencies of the most consequence. The reception-room should be a cheerful and neatly-furnished sitting-room, and so situated that the newly-admitted patients can proceed from it to whatever part of the asylum it is thought best they should be sent. Thus, violent and noisy patients would not be led shouting and struggling through tranquil wards; and timid and quiet patients would not have to pass along corridors containing patients whose unfamiliar looks agitate or affright them." (pp. 13-14.)

"All the locks in the house should be so made, and so kept in order, as to open and close with the least possible noise. The jingling of keys, the clang of the locks, and the violent opening or shutting of the doors of bedrooms and galleries, are generally considered of no consequence by the attendants; but they produce the most uncomfortable feelings in the patients. The gallery doors should have large handles, enabling those who pass through to shut the door quietly. The state of a whole ward may immediately be altered by persons hastily and noisily passing through. There is, however, no point so little attended to by the generality of attendants, servants, workpeople, officers, and even by the official visitors of all asylums. The physician's caution is generally despised, and his example disregarded. Doors are thrown suddenly open; attendants are loudly or fiercely called for; unexplained intimations are given of something being wrong somewhere; the ward is hastily passed through; the hindering patients are put aside; the door is violently shut; the next ward and the next are similarly invaded; and the whole of the inmates, attendants as well as patients, are left in a state of agitation, or even of alarm. I have often, in former times, discerned the impression of these impetuous visits so distinctly left as to make me inquire into its cause; and have had deep reason to lament that it should be so much forgotten that the manner, the voice, the words, the very step of those who pass through a house full of insane people ought to be calm and tranquillizing. Those most conversant with the insane will the most readily acquit me of exaggerating the importance of these matters." (pp. 29-30.)

Cruel economy. "There is a refinement of economy practised in some private asylums which grievously interferes with the comfort of educated patients, and which consists of allowing a long interval to elapse between the departure of daylight and the introduction of lamps and candles. A great amount of suffering is entailed on the patients by this ingenious neglect. This is generally allied with an equal anxiety to save in the article of fuel; and servants are permitted to forget to mend the fires with entire impunity. The same principle creeps sometimes into public asylums; and to save the expenditure of a few shillings in a week, the patients are allowed to grope their way about the wards, and find their way to bed as they can, when the days are shortening, and when the patients are also cold and comfortless and irritable, in consequence of passing many hours in too low a temperature." (p. 37.)

Importance of cleanliness. "The provisions necessary for cleanliness are humble things to dwell upon, but they are the auxiliaries of health, and deserve the most vigilant attention of the physician, who ought to remember, when he detects a bad smell in passing any door in a gallery or passage, that some of the inmates of the asylum are exposed to that offensive air all the day long, and all the night, and that some of them never go out beyond the boundaries of the asylum grounds, so as to recruit, by change of air and scene, their constitutional power of resisting the influence of local malaria. And whoever pays much attention to the habits of human beings, must know how much the irritability of the mind is increased by habitual ill health, in whatever way produced: and it is with this irritability, on a large scale, that we have to deal, and which it is our business to remedy or prevent in asylums for the insane.....There should be in every ward a lavatory or washing-room, containing half-a-dozen enameled-iron washing basins, sunk in a leaden table, and each basin supplied with a watercock, and having a moveable plug. There should also be one or two large round towels in each room. Among the late additions to the wards at Hanwell, none has been more productive of comfort to the patients than this, and the washing-rooms are the perfection of neatness. These rooms are open to the patients of quiet wards at all hours of the day; and the means of having a clean face and hands, and the refreshment of washing when fatigued, or after various occupations, is very much enjoyed. An additional means of personal cleanliness is also afforded by each patient having a warm bath once a week. Where these regulations prevail, the skin of insane patients loses its most repulsive and unwholesome aspect; general health is promoted, and with health, comfort, and cheerfulness; and the peculiar odour of the insane, so often described, does not permeate every ward, and is, indeed, seldom perceived. There are private asylums, on the old plan, which I could recognize as such, if taken blindfolded into them; and where this terrible and peculiar smell tells the visitor, as plainly as any impression on the sense can tell, that the rooms are not thoroughly clean, that the bed and body linen are not sufficiently often changed, and that washing and bathing are very little attended to." (pp. 38-40.)

The following extracts will be read with deep interest, as giving us a glimpse of the early scenes witnessed by Dr. Conolly at Hanwell, and the effect they had on his mind.

"It was in the female infirmary at Hanwell, exactly seven years ago, that I found, among other examples of the forgetfulness of what was due either to the sick or insane, a young woman lying in a crib, bound to the middle of it by a strap round the waist, to the sides of it by the hands, to the foot of it by the ankles, and to the head of it by the neck: she also had her hands in the hard leather terminations of canvas-sleeves; she could not turn, nor lie on her side, nor lift her hand to her face; and her appearance was miserable beyond the power of words to describe. How long she had been in this state it is not material to record. That she was almost always wet and dirty, it is scarcely necessary to say. But the principal point I wish to illustrate by mentioning this case is, that

it was a feeble and sick woman who was thus treated. At that very time, her whole skin was covered with neglected scabies, and she was suffering all the torture of a large and deep-seated abscess of the breast. Let it be considered what must be the effect on the attendants of having customary recourse to the imposition of restraints, when such complicated suffering as this became comparatively disregarded by medical men, in consequence of the spectacle presented to them being, at each visit, not that of a sick person requiring aid, but of a dangerous lunatic cruelly fastened and bound. But this patient was neither dangerous to herself nor to others. The excuse alleged for this mode of treatment was, that she would eat the poultices employed, and which contained lead, and that she was very mischievous: that was all. However, she was liberated; no bad consequences ensued, and in a few weeks I saw the poor creature at the chapel, and even heard her play the organ, which she had been accustomed to do in the church of a village in Middlesex before her admission. This patient died very recently; having from the time of her liberation from restraints scarcely ever given any trouble to the attendants. Perhaps if I had never seen such a case, I should have been less earnest to adopt the system which I knew had been tried at Lincoln, and slower to try to manage the patients of this great asylum entirely without restraints. Many a case was yet to be managed in which every ingenious difficulty was created or encouraged to baffle this attempt; many anxieties were to be endured, many misapprehensions to be submitted to, and much suffered: but all is now passed; and I thank God, with deep and unfeigned humility, who has permitted this great experiment to proceed, for full seven years, without one accident calculated to discredit it; and with a general result on the asylum best known to those who knew the asylum before; and a general effect on all other asylums in almost every region of the globe, which can never be entirely lost." (pp. 47-8.)

"Those who now visit the asylum, and who witness no such scenes, may be excused for imagining that I am needlessly apprehensive of the use of restraints being revived, and may talk, with apparent reasonableness, of the occasional and moderate employment of such methods. Experience has taught me that their shocking abuse, if once more witnessed, would subvert all such reasonings. Their abuse, to an extent which now can scarcely be described without a suspicion of exaggeration, and in an asylum distinguished from its first opening by the comforts and advantages it extended to the pauper lunatic, shows that their use, to any extent, cannot be permitted without danger. Attendants trained in asylums where restraints are used, immediately apply them in private and recent cases, thus producing the worst consequences. Of this I meet with frequent and lamentable instances in private practice; where days, precious to the treatment of recent cases, are passed in all the misery and disadvantage of bonds, and dirt, and darkness, and unregarded fever, restlessness, and thirst." (pp. 46-7.)

We must take a few extracts from the pages devoted to the very important subject of *exercise in the open air*.

"Both with reference to health and recreation, as well as to employment, the airing-courts and grounds and gardens of an asylum are worthy of especial attention. A most gratifying improvement has taken place in these respects also, within the last few years, in many asylums. In the older institutions these had no place, or dismal yards and barren courts were alone to be seen. No external influence could have been devised more powerful to depress the mind, and sink it into inactivity, than monotonous gravel courts, surrounded by walls from ten to fifteen feet high; without a tree; without a shrub; without a blade of grass; without shade in the heat of summer, or shelter from the rains of winter—the only luxury being a bench fastened to the wall, with large iron rings suspended over it, so that even in the open air restraint might still be substituted for superintendence. In these respects, asylums were, until very lately, precisely like jails, in one of which a man, who had been long imprisoned for an act committed in a fit of insanity, used to say to me, when I visited him,—

'Sir, I have not seen a flower or a green leaf these seven years!'—a painful observation to the ears of those who can pass from such gloomy precincts to fields and pleasant footpaths, never more to be trodden by the miserable. Within equally melancholy boundaries might be seen, in most asylums, but a few years ago, every form of gloom and eccentricity which the absence of all external objects of interest could foster; and the patients walked up and down some chosen path beneath the hopeless walls, until the very ground was worn into hollows; or, debarred from the full exercise of the muscles of locomotion required by their excited brain and nerves, expended all their energy in exertions of voice, distressing to all within hearing. Even at present, such arrangements may yet be witnessed, and the consequent concentration of morbid excitement of a crowd of insane people, which ought rather to be allowed relief by action and expansion in liberal space." (pp. 49-50.)

"It is astonishing to find that one of the particulars of treatment in which the rich patient is sometimes more unfavorably situated than the poor, is that of having the opportunity of enjoying free and frequent exercise. The untrodden lawn, the dusty and desolate courts, the paved yards, the wretched sheds, the lonely outhouses, together with the closed doors and windows of a private asylum, often give to such places an external character which makes the visitor regard them with dread, and the passer-by speak of them in whispers. The idea of their entrance is connected with that of the fatal gate, which whoso entered left all hope behind. If a patient is visited in such a house, the unlocking of doors, and the threading of long passages, and ascent of gloomy stairs, with the close atmosphere of apartments filled with patients sitting by the walls, oppressed with indolence and monotony, are all features too familiar to those who knew such houses before later improvements penetrated into them. And it is only in a few of the best private asylums that we even now find cheerful sitting-rooms on the ground floor, opening on gardens, into which the patients may walk when they please; and the benefit of this improvement is yet too generally limited to the most rational of the patients, and not extended to the irritable and troublesome, who ought also to be able to go out of their sitting-rooms, although into gardens more secluded and secure. The extent of ground assigned to the patients, their subdivision during exercise, and, indeed, all the other arrangements in private asylums, vary so much in different establishments in these days of movement, that the remarks justly applicable to several which have adhered to the ancient ways, would be most unjust, as well as painful, to the proprietors of some, in which no sacrifice has been spared to secure for every patient all the advantages which a physician or a conscientious superintendent would wish them to enjoy." (pp. 58-9.

The following, out of many admirable remarks on the subject of *dress*, we recommend to all proprietors and superintendents of private and public asylums.

Dress of Nurses. "The number of female patients who go about bareheaded is always greatest where there is the most neglect. It is not natural to the woman to neglect the dress of her head, and if the faculty is impaired, care will often restore it. When convalescence is commencing, the patient generally becomes more cheerful if some assistance is given her as regards her dress for Sunday; and of this a neat, or even a pretty cap, is an important part. The men also, when recovering, often ask for newer clothing to go to chapel in. The attendants should set a good example. Slovenly attendants generally increase the number of dirty patients. The male attendants should not be permitted to wear loose holland frocks, which give them the appearance of butchers; and the female attendants should not be discouraged from observing the neatness and taste in dress which are natural to them. No mistake is greater than that of supposing that being dressed in unbecoming clothing, in stuff gowns and in mob-caps, is either a virtue in itself, or an incentive to virtue. Such sentiments form part of

a gloomy and selfish system, including mortifications and degradations, especially unfavorable to goodness of any kind, and only gratifying to those who impose them. Such dresses are especially distasteful to English women of any class; they always discard these outward signs of poverty as soon as they are raised above the lowest condition of pauperism. A neglect of this really proper feeling is a frequent cause of discontent in asylums, and sometimes retards recovery." (pp. 61-2.)

Dress of private patients. "Many private asylums are open to the charge of great neglect as respects the dress of patients of the classes far above pauperism. Tattered and threadbare coats, very shabby hats, trousers not always free from an offensive smell; and equally slovenly dresses on the female side of the asylum, shoes out of repair, hair in curl-papers, make the unfortunate patients objects of pity or of ridicule. They feel themselves degraded, lose their self-respect, and with it the little self-control their malady has left them. It is very true, that in some cases the patients will not dress themselves properly, that they have an affection for old and ragged garments, and insist upon their being fit to go to court in, and are violently offended if better clothes are substituted for them; but such cases form only a small proportion in any asylum; and in many instances, habits of personal neatness may long be preserved, and in some restored, after being long lost. Even an apparent disregard for cleanliness sometimes proves to have been the result of previous neglect and ill-treatment. Patients are often brought to Hanwell who are reported to us as uncleanly, but who do not prove so when properly attended to. Others who have been pronounced uncleanly, and sent to wards appropriated to such patients, have recovered the power of keeping themselves neat, and avoiding any uncleanly habits, after some accident has caused them to be subjected to the superintendence, and benefited by the care, of the attendants in the infirmaries. Such cases are not very frequent, but they prevent despair in circumstances which appear hopeless.

"The rule should be, in private asylums, that each gentleman should be encouraged to dress according to his station, and to be at least as careful of his whole dress, his boots, his hat, his gloves, as he used to be when well. Ladies should not be allowed to forget that they are ladies; but should be required to dress appropriately, both in the morning and for dinner. Their friends are sometimes more in fault than they, and the patients are disfigured against their will; but it is disadvantageous to them to be thus permitted to fall into a negligence characteristic of advanced and incurable forms of disorder." (pp. 63-4.)

The following is the only extract our limited space will allow us to take from the admirable chapter on *diet* and *employment*.

"It is impossible to be too careful in directing that all the service of the table should be in accordance with the habits of the patients. The sense of banishment from home, and of confinement, and the consciousness of mental infirmity and dependence, are mitigated in the mind of many a silent, uncomplaining patient by these means. All the comforts added of late years, and with so much advantage, to the wards of county asylums—such as moveable tables and chairs, window-blinds, plants, musical instruments, bagatelle boards, books, and pictures, with free access to agreeable gardens—show that the majority of the insane of any class are inclined to respect the decent arrangements made for them; and it will generally be found that the more conformable the furniture of the rooms, the tables, &c., of the higher class of the insane is to their habits and rank, the less they will be disposed to destroy or derange it. During violent paroxysms of mania, they are of course regardless of everything; and everything about them should be plain, simple, and secure. But when convalescence begins it should be respected. Among the depressing recollections of the insane of the higher classes, when recovering from insanity, I know that none are more frequent, or felt to be more degrading, than those connected with any want of respect shown to them, or any disregard of decent customs as to their meals. Yet, without great attention,

they will sometimes be found, when quite well enough to appreciate what is done, sitting down to a dinner of meat, vegetables, and pudding, all sent to them on one plate. Negligences of this kind produce fretfulness and discontent, and tend to retard convalescence. If attendants are allowed to practise this kind of negligence, they soon fall into habits of rudeness, and even of inhumanity, fancying that the patients do not observe their conduct, and that their feelings are of no consequence." (pp. 76-7.)

Chapters V and VI are devoted to the subject of *attendants*, their qualifications, duties, and treatment, and contain a vast deal of information of the greatest importance as well as novelty. Here, as in every other part of his book, the author's sympathies are strongly enlisted on the side of the weakest. We could willingly transfer the whole to our pages, but we must be content with a very few brief extracts.

Selection and duties of attendants. "If I may rely on my own observation, no subject connected with the management of the insane, either in asylums or in private practice, has received less adequate attention than the selection of proper attendants, their proper treatment, their just government, and their instruction in the various, and peculiar, and exhausting duties which necessarily devolve upon them. The important and delicate task of regulating the conduct of persons of unsound mind, of controlling excitement, restraining waywardness, or removing mental depression, is unavoidably confided to persons of limited education; but these are too frequently chosen with little regard to their disposition, temper, or intelligence: they are permitted to commence their duties with as little preparation as if their office was merely that of a servant, and are governed either with severity and injustice, or without the consideration and indulgence requisite to support their patience, and to encourage them to be considerate and indulgent to those on whom they attend, and who are wholly in their power." (pp. 83-4.)

"An attendant must be expected to devote his whole time and attention to the patients; to consider himself the friend and adviser of those who are separated from every other friend and acquaintance, and the protector of those whom disorder of the mind has incapacitated from taking care of themselves. The functions they are to perform are not merely those of servants; they are the only sane persons always with the insane, and their temper, their manners, their cheerfulness and activity, their neatness and order, or the want of these qualities, will exercise a continual influence on all who are committed to their charge. On very many occasions I have noticed the remarkable effects produced on a patient, and often on a whole ward, by a change of attendant. Dirty patients have become cleanly, idle patients have become active, and morose patients have become cheerful. Contrary changes may not unfrequently be observed also, when the change is from a good attendant to a bad one." (p. 110.)

Importance of good treatment of attendants. "Attendants who zealously endeavour to perform all their duties well should be treated with confidence, and allowed every reasonable indulgence. The natural pride they feel in the state of their ward, and in their influence over troublesome patients, should not be discouraged by frequent removals either of the patients or of themselves from one ward to another. Everything that affects them will be found to exercise a secondary influence on the patients; their health, their circumstances, their instruction, their mode of living in the asylum, their diet, the comfort of their sleeping-rooms, are all matters of much importance, generally attracting too little attention, and sometimes wholly overlooked in the arrangements of an asylum. Yet nothing can be more certain, than that attendants who lead an uncomfortable life are not likely to be in a state of mind to make patients comfortable. If it is forgotten that they have ordinary human feelings, or if they are treated as if presumed to be always dishonest and unworthy of trust, or are subjected to heartless or capricious refusals and mortifications when desirous of a little extra leave of absence, they cannot but lose all attachment to the officers, and all interest

in the asylum. It is a mockery to tell attendants, who are not sure of their places for a day, that they are to devote themselves entirely to the patients, and to be themselves patterns of forbearance. Yet the want of devotedness on their parts is a want of that for which there is no substitute, and the want of forbearance in the attendants will lead to the worst consequences. So strictly connected is the proper government of an asylum with the welfare, and even with the safety of the patients. Unjust officers, or unjust rulers, make attendants indifferent or cruel, and indifferent or cruel attendants make the patients wretched." (pp. 113-14.)

The last chapter considers the Religious Services of an Asylum—Schools—Government—Treatment of Officers—Qualifications and duties of the Superintendent.

The following beautiful passage shows at once the author's opinions as to the good and evil of religious observances in relation to insanity.

"The institution of religious services in asylums has created new and peculiar duties for the officers; and although I can readily conceive the apprehension with which the medical superintendents of some asylums regard this subject, and know how unjust it generally would be to ascribe such apprehensions to indifference, I am quite satisfied, that with reasonable caution in the exercise of his peculiar duties, a chaplain may become a valuable officer in asylums for the insane. It is unfortunately true, that no cause of mania, melancholia, and imbecility is more common than a gloomy religion, which excludes the idea of God's mercy so carefully, and brings forward God's judgments so prominently, as to alarm, and depress, and enfeeble many enthusiastic and weak persons who are exposed to its doctrines. Among persons of education, and particularly among women, I believe that nearly one half of the cases of derangement of mind arise from this perversion of religion alone. Exciting meetings, enthusiastic exhortations, false reports of wild missions, foolish biographies of sickly and delirious children, incoherent tracts, and books of unfruitful controversy, constitute all the intellectual exercises of these sincere and misguided persons. All elegant literature, and almost all science is kept from them, as demoralizing or tending to unbelief. All cheerful avenues of thought are forbidden to them. A restless, meddling, dictatorial spirit, much opposed to real charity, assumes the guise and name of benevolence and religious zeal. By degrees, the mind—so ill-exercised, so ill-governed, so excited—becomes weakened, and then the mask falls to the ground. Spiritual and worldly pride, idle prophesyings, convictions of eternal wrath, fierce denunciations of neighbours, or parents, or children, or relatives, and too often despair and attempts at self-destruction, declare that madness has supervened. Knowing all this, by daily observation, I feel as strongly as any physician can do, the danger of misapplying religious attentions; but I still believe that many insane persons are capable of deriving much satisfaction from being permitted to attend the services of their church; and that a good and prudent clergyman may become a useful auxiliary to a physician, by correcting fanatical delusions, moderating spiritual conceit, vindicating God from the unjust views of his creatures, and reviving every hope that is permitted to the imperfect and the penitent." (pp. 123-24.)

With the following extracts relating to the duties and qualifications of the medical superintendent of an asylum, we must take our leave of this fascinating volume. Our readers may probably be able to find a local habitation and a name for such offices and such an officer.

Duties and qualifications of a superintendent. "It is to him that the whole house must at all times look for the principles by which everything done in it is to be regulated. His supposed or his known wishes should be present to the mind of every officer and every attendant in every variety of accident, and his character of mind and heart ever in their view. Indifference on his part must lead to

negligence on the part of those who execute his commands; severity exhibited by him must lead to brutality on the part of the attendants. His steady discouragement of negligence, his known abhorrence of cruelty, and his real and deep sympathy with his patients, may be reflected from every humane heart in the asylum.

"His duty comprehends a wide and careful survey of everything that can favorably or unfavorably affect the health of the mind or the body. He has to regulate the habits, the character, the very life of his patients. He must be their physician, their director, and their friend. The whole house, every great and every trifling arrangement, the disposition of every officer and servant, should be in perpetual conformity to his views; so that one uniform idea may animate all to whom his orders are intrusted, and the result be one uniform plan. Nothing should be done without his sanction. The manners and language of all who are employed in the asylum should but reflect his; for everything done and everything said in an asylum is remedial or hurtful; and not an order should be given, or a word spoken, except in accordance with the spirit of the director of the whole establishment. By such a system alone can it ever be proved to what extent the cure or the improvement of the insane is practicable.

"That he should be a person naturally benevolent is indispensable; and it is extremely desirable that he should possess an almost inexhaustible patience. The qualities to which, of old, much importance was attached—a commanding stature, a stern manner, a fierce look, a loud voice—have become either unnecessary, or positive disqualifications. Threats or reproofs, seconded by these attributes, may terrify the patients, but they loosen the bonds of affection, and generate feelings which will burst forth into expression in the next paroxysm, or revengeful designs, which will wait their time. Even remonstrances, to be successful, must be calm and carefully timed, being, as they are, addressed to the afflicted rather than the faulty. If sickness lays open all the delusions of life, madness often shows all human weaknesses magnified, and they must be viewed with never-failing charity, at no time forgetful that the dispositions so exhibited are impaired and deformed by insanity." (pp. 140-1.)

Results and rewards of good management. "If the whole of the system which I have imperfectly endeavoured to sketch be steadily persevered in, —no anger—no severity—no revenge—no deception—no disregard ever shown to the insane,—the resident superintendent will no longer find himself living among the habitually furious, or the incurably gloomy, or the constantly discontented. Calmness will come; hope will revive; satisfaction will prevail. Some unmanageable tempers, some violent or sullen patients, there must always be; but much of the violence, much of the ill-humour, almost all the disposition to meditate mischievous or fatal revenge, or self-destruction, will disappear. Some of the worst habits that beset the poor lunatic will also be got the better of; cleanliness and decency will be maintained or restored; and despair itself will sometimes be found to give place to cheerfulness or secure tranquillity. I could walk through such an asylum as I have described, and point out illustrations of every word in every ward.

"Resolved, therefore, to make his asylum a place where everything is regulated with one humane view, and where humanity, if anywhere on earth, shall reign supreme, the resident medical director must be prepared to make a sacrifice of some of the ordinary comforts and conventionalities of life. His duties are peculiar, and apart from common occupations. His society, even, must chiefly consist of his patients; his ambition must solely rest on doing good to them; his happiness on promoting theirs.

"None but those who live among the insane can fully know the pleasures which arise from imparting trifling satisfactions to impaired minds; none else can appreciate the reward of seeing reason returning to a mind long deprived of it; none else can fully know the value of diffusing comfort, and all the blessings of orderly life, among those who would either perish without care, or each of whom would, if out of the asylum, be tormented or a tormentor. Constant

intercourse and constant kindness can alone obtain their entire confidence; and this confidence is the very keystone of all successful management.

"Thus living, and thus occupied, the director will learn to love his people, with all their infirmities, which are their afflictions. The asylum is his world. The patients are his friends; humble, but not without even delicate consideration for others; wayward, but not malignant, except when cruelty exasperates them; capricious, but not ungrateful; distrustful, but to be won by candour and truth; disturbed and grievously afflicted, but not dead to some of the best and purest affections. He will almost regard his patients as his children; their cares and their joys will become his; and humanly speaking, his whole heart will be given to them." (pp. 143-4.)

ART. XI.

Die Krankheiten des Zwerchfells des Menschen. Von C. W. MEHLISS, M.D., &c.—*Eisleben*, 1845.

The Diseases of the Diaphragm in Man. By C. W. MEHLISS, M.D., &c.—*Eisleben*, 1845. 8vo, pp. 212.

THE diagnosis of disease of the diaphragm is attended with so much difficulty and uncertainty, that every attempt made in an earnest and philosophical spirit practically to advance our knowledge of the subject, or even to collect and arrange the scattered and fragmentary information afforded by medical literature, is deserving of praise. We ought, indeed, to consider ourselves the more beholden to Dr. Mehliss, that this task, which he has undertaken and fulfilled with great industry, and, upon the whole, with much good sense, is not altogether a very grateful one. We were really not a little curious, at first, to ascertain how it could be possible to fill 212 pages with a subject of apparently so little promise. Still, as we proceeded, our interest, we confess, soon became sufficiently excited to carry us very pleasantly to the conclusion of the treatise. It contains little, perhaps not any, information altogether new, but it has brought the scattered rays into such a focus as to render them, in some degree, available for practical purposes. It reteaches us a lesson which the progress and bent of anatomical pathology in the present day had almost made us forget; namely, that the diaphragm, far from being a mere blank leaf between two all-important classes of organs, is a muscle whose mere functional derangement may seriously, if not irreparably damage both the one and the other; in fine, that its condition ought always to be carefully investigated and duly weighed in our estimate both of thoracic and of abdominal disease.

The work commences with an elaborate account both of the anatomy and physiology of the diaphragm, in which, however, there is little that requires comment, or that is not to be found, in a condensed shape, in every good hand-book of anatomy. From this portion, therefore, we shall merely quote as useful the observation of Krause,* "that the highest convexity of the diaphragm, in the adult, is on a level with the upper margin of the middle portion of the seventh rib, or with the cartilage of the fifth; but that on the right side, it rises from half an inch to an inch higher than on the left."

* Handbuch der Menschlichen Anatomie, Vol. I. Hanover, 1838.

Since the publication of Dr. Mehliss's work—in short, quite recently—a most valuable and interesting paper has been read at the Medico-Chirurgical Society,* on the physiology of respiration, wherein it is shown by the author, Mr. Hutchinson, contrary to the generally received opinion, that, whilst ordinary, involuntary respiration is performed chiefly, if not solely, through the instrumentality of the diaphragm, this organ remains nearly quiescent during a profound respiration. Of the truth of this the reader may convince himself by experiment. He will find that every *ordinary* act of inspiration is accompanied by a relaxation of the abdominal muscles, denoted by a protrusion of the abdomen; every ordinary act of expiration by their contraction. Now it is to be remembered that, in breathing, the action of the abdominal muscles is inverse or antagonistic with that of the diaphragm. In the above movements the intercostal and other respiratory muscles participate only very slightly, so that a large proportion of the lungs does not become inflated. On the other hand, all the intercostal and other auxiliary muscles of respiration are engaged during a profound, or it might be more correct to say, a *full* inspiration, whilst the abdominal muscles are tightly braced up, the hypochondria drawn inward, and the diaphragm, if anything, slightly relaxed. Indeed, so long as the diaphragm is under contraction, it would seem to be impossible for the upper portion of the thorax to expand. We must, however, add to Mr. Hutchinson's statement, that after full expansion of the upper portion of the chest, and consequently inflation of the peripheral portions of the lungs have been effected, the diaphragm may, by a *voluntary* act, be still made to contract, by which means no inconsiderable amount of air may yet be inhaled. This double act of inspiration is, however, obviously a forced one, and not very frequently performed.

To remind the reader that both surfaces of the diaphragm have a serous investment would be needless, except, perhaps, for the purpose of remarking incidentally what is sometimes lost sight of, namely, that the base of the pericardium is firmly attached to the central tendinous portion of the diaphragm, by means both of cellular tissue and of fibrous bands, which close connexion, of course, tends very materially to promote a reciprocal reaction between the heart and diaphragm, both in health and in disease.

The position of the diaphragm, the importance and variety of its functions, the sensitive character of the momentous organs and parts by which it is compassed, or traversed, render it apparent how seriously disease of this muscle must impair the general health, but serve at the same time to explain both the obscure and the dependent nature of most of its ailments. The multiplicity of its sympathies, resulting not alone from juxtaposition, but also from the numerous excito-motory influences to which it is subject†, has gained for it a reputation for an extraordinary amount of inherent muscular irritability. No other muscle of the human body is so greatly under the influence of reflex action as this. Respiration, laughter, sobbing, coughing and sneezing, and again vomiting and defecation, are severally enacted by the whole or parts of the diaphragm sympathising

* Since published in the Medico-Chirurgical Transactions.

† The phrenic nerves are often derived from the anterior branches of the second to the sixth cervical nerves—always from the third and fourth—and are at their origin often connected with the cervical ganglia of the great sympathetic, with the pneumo-gastric nerve, and with the descending branch of the hypoglossus. Besides this, the diaphragm receives emissaries from the solar plexus.

indirectly; that is, through the medium of the nervous centres, with remote nerves directly stimulated. These are, for the most part, involuntary acts; for, although the muscle is to nearly an equal extent under the control of the will, this power is here more frequently exercised in arresting or subduing, than in promoting its action. Ordinary respiration would, however, appear to be the only function in which the diaphragm is singly active. In all its other functions it is associated with other muscles, and that not in consequence of any direct nervous consensus between them. All are here evidently set in motion directly from the sensorium, and not all simultaneously, but in the exact sequence in which they derive their nerves from the cerebro-spinal centres. Thus, in laughter, the muscles of the face receive the impulse first; and if the latter be slight, exclusively. Where the impulse is greater, the laryngeal,—then the pectoral muscles,—next the diaphragm,—and lastly, where the impulse is violent, the abdominal muscles severally, or all, become engaged.

Disease of the diaphragm. If we except the heart, no muscle of the human body is probably more often morbidly affected than the diaphragm. The variety of its affections is, however, not commensurate with their frequency, the great majority ranging between the extremes of spasm and paralysis. Of these two, spasm is by far the more common, the number and extent of its nervous sympathies causing the organ to participate very readily in spasm affecting other parts, whilst these identical relations at the same time prevent its entire innervation from becoming easily suspended. Thus, in general, convulsions of whatever kind, the diaphragm is always in a high degree implicated, whereas, in general states of paralysis, its activity is, for the most part, little impaired. The other affections of the diaphragm are nearly all due or subordinate to, or at any rate inseparable from, the diseases of adjacent parts. Upon the whole, thoracic disease is more readily communicated to the diaphragm than abdominal; the latter has, however, a much greater tendency to excite in it convulsive action.

The symptoms generally indicative of diaphragmatic ailment are divisible into those of *anomalous sensation* and those of *anomalous motion*. The former consist in a characteristic sense of tightness or constriction at the lower part of the thorax (which is also felt sympathetically, from organic disease of the cervical portion of the spinal marrow); and, secondly, in pain, which, in partial disease of the diaphragm, is referred to the affected locality,—in general disease of the organ, to behind the inferior portion of the sternum, and from thence across to the spine. This pain is never, as some writers affirm, very intense, and differs in no particular from muscular pain elsewhere.

Anomalous motions of the diaphragm. These are tremulous, spasmodic, or convulsive. The preternatural sensations they produce have a singular effect upon the imaginations of patients, who often fancy that some living animal has taken up its abode in their interior;—notions which may possibly have given rise to the fables recounted from time to time of serpents being extracted from the stomachs of men. The vibratory movements of a tremulous diaphragm are not unfrequently perceptible to the eye.

The secondary symptoms of diseased diaphragm consist in disturbance of those functions which are carried on through its instrumentality or aid.

Here, of course, respiration is principally concerned. In slighter affections of the diaphragm, ordinary breathing may proceed without obstacle, and the difficulty be felt only when the necessity for a deeper inspiration occurs. Where the affection is more considerable, ordinary respiration becomes more abrupt. In disease of the organ attended with pain and fever, both conditions are observable, respiration being short, and at the same time frequent, hurried, the patient feeling, on an attempt to inspire deeply, as if the lung were held fast. Under these circumstances, the breathing sometimes becomes so hurried as to exceed in rapidity the arterial pulse, when it may be likened to the panting of hounds heated with the chase. Where again the diaphragm is disabled by abnormal action, respiration is abrupt, interrupted, irregular in rhythm. Where its *activity* is altogether suspended, the results depend upon the circumstance as to whether it be or be not still susceptible of passive motion. In the former case, as, for example, in paralysis of the diaphragm, the abdominal muscles in some measure act for it, and we have for a while the phenomenon of so-called abdominal respiration, the real import of which we shall afterwards endeavour to explain. In the other case, as in tetanic states of the diaphragm, for instance, complete apnœa ensues, which would necessarily terminate in death, if the other muscles of respiration failed to take the entire office upon themselves. Other cognate functions are equally liable to interruption or disturbance from the same causes; laughing, coughing, sneezing,—speech itself—being thereby restrained, or at least modified in a very peculiar manner. Finally, the power of defecation is liable to suspension, the want under these circumstances being felt, but the ability to satisfy it withheld.

Changes in the position of the muscle, and in its relation to other organs and parts. When under the influence of abnormal contraction, the diaphragm withdraws the cartilaginous appendix of the sternum, and the common margin of the cartilages of the false ribs, to which it is attached, towards the centre of the thoracic cavity. By this means a groove is formed externally, about the hypochondria, which is rendered somewhat deeper by inspiration, but flattens during expiration. "The heart's pulsation," says our author, "then becomes less distinct." This is, however, chiefly the case during the former act, which, under these circumstances, is accomplished by the pectoral muscles, causing the thorax to expand depthwise (instead of lengthwise), and the heart therefore to recede momentarily from the pectoral walls. The conditions are, in fact, the same as during the full natural inspiration before described. Total inaction of the abdominal muscles during respiration is a positive sign of suspended mobility of the diaphragm. It is most apparent in painful affections of the organ and, when, protracted, commonly leads to vesicular emphysema of the lungs, owing to the inadequate performance of the act of expiration.

The ancients believed in a much closer intimacy between the brain and the diaphragm (the *φρένες* of the Greeks) than the present standard of our knowledge of neuro-pathology suffers us to admit. It is, however, to be observed that this belief passed current up to a comparatively recent period, and we think it not quite improbable that the tricks of the earlier mesmerists may have been founded upon this traditional error. At the present day we are no longer on the watch for wild delirium to confirm

our suspicions of a severe lesion or other affection of the diaphragm, nor do we, on the other hand, take the latter for granted merely because the patient's facial muscles are seen to be distorted with the so-called sardonic grin.

In the rarer instances of independent disease of the diaphragm, the symptoms before specified will probably be sufficiently marked. Where this is not the case, however, the author sagaciously employs the method "by exclusion." Thus, in doubtful cases, he is in the habit, *provided there be no obvious counter-indication,* of trying the effect of sternutatories. The patient who can sneeze vigorously cannot possibly be labouring under any severe affection of an organ whose entire freedom of action is indispensable for the performance in question. Nor is the converse less true; the total incapability to sneeze, assuredly implies suspended mobility of the diaphragm.

Wounds and contusions of the diaphragm from external injury. The importance of these, for the most part, merges in that of the simultaneous lesion of contiguous parts. They are inflicted by means either of fire-arms or of some sharp instrument, and more often through the thorax than through the abdomen. Mere contusions of the diaphragm may occasion internal hemorrhage or inflammation, and eventual gangrene of the organ. Penetrating wounds are, however, for the most part of a much graver character. Where their locality and size are such as to admit of speedy cicatrization, they are, so far as they are alone concerned, seldom productive of much mischief. In the opposite event, either they cicatrize imperfectly, and eventually give rise to diaphragmatic hernia (strictly so called), or else they remain permanently open, affording to a greater or smaller amount of abdominal viscera a free passage into the thorax. The consequences are permanent disturbance to the thoracic organs, and, sooner or later, fatal asphyxia. This prolapsus of the abdominal organs is more frequent when the left side of the diaphragm is wounded; on the right side, the liver opposes a decided obstacle to such a consummation. To this part of the subject we shall afterwards have to recur.

Cases. An instance is cited from Schmucker* of a hussar who received two lance wounds, one in the left inguinal region, the other, immediately afterwards, in the epigastrium, which penetrated to the depth of an inch and a half, obliquely in the direction of the diaphragm. Violent diaphragmatitis ensued, being preceded by a night passed in mingled *laughter, crying, and vomiting.* Under vigorous antiphlogistic treatment, the patient recovered, so far as to appear able to return to his duty. At this epoch, however, he was unfortunately taken prisoner, marched away on foot, and lost sight of.

In this case, the diaphragm was supposed to have been wounded but not pierced. Instances of penetrating wounds of this organ are related by several of the older medical authors, amongst others, by Morgagni. In some of these the chief symptoms recorded, namely, orthopnœa and vomiting, only terminated with death; in others, seeming recovery was followed, after a longer or shorter interval, by deadly symptoms of incarcerated bowel. An interesting case is related by Boyle, in the 'Edinb. Med. and Surg. Journ.' in 1812. A man, about 40 years of age, was,

* Wahrnehmungen.

after a meal, seized with shivering, nausea, and vomiting; the abdomen was retracted, sunken. He was largely bled, and took a dose of castor oil, which, however, the stomach rejected. The employment of a warm bath was followed by two copious stools; nevertheless the vomiting continued, and death ensued within twenty hours. The stomach was found so enlarged as to fill the entire abdomen. On its removal was seen the diaphragm, presenting a large convexity downwards, and in its fleshy portion a slit through which the jejunum, the greater portion of the ileum, and the transverse colon, together with the greater omentum, had penetrated into the left thoracic cavity. The intruding viscera were enveloped by the omentum as within a sac, and strangely intertwisted. The edges of the wound in the diaphragm were smooth and of a ligamentous character, the transposed intestines not incarcerated, but, in the neighbourhood of the wound, attached by means of fibrous threads to the mediastinum and costal pleura. Eleven months previously, the man had received a wound between the sixth and seventh ribs on the left side, but which had quickly healed. Six months later, he had suffered disturbance of the digestive organs, which, though transient, left him less fit for labour, and constantly afflicted with dyspnœa. "Morgagni mentions a case in which the wound in the fleshy part of the diaphragm was scarcely large enough to admit a man's little finger; nevertheless the greater portion of the colon had become protruded through it." Transposition of abdominal viscera is, however, not an invariable consequence, even of extensive and mortal wounds of the diaphragm. Gunshot wounds are, for the most part, attended with such grave injury to other internal organs, that the share taken by the diaphragm in producing a fatal issue can with difficulty be estimated. A somewhat interesting case of imperfect recovery from a gunshot-wound is told by Bilguer.* A Prussian grenadier had been wounded by a musket-ball, which entering on the left side, between the sixth and seventh ribs, passed out again on the right side, at two fingers' breadth from the third lumbar vertebra. There were no symptoms of the injury of any viscus, and the patient did well. From that time forward, however, he was unable to walk erect, or otherwise than with his body bent forward, and he was discharged from the army in consequence. Every attempt to assume an erect posture was attended with a painful sensation of tension far back in the umbilical region. It does not, however, appear to us very certain that the diaphragm was here in fault.

The following case is pretty generally received, in Germany, as evidence of complete recovery from an undoubted wound of the diaphragm. It is related by Dr. Grabl, of Hamburgh, in 'Fricke and Oppenheim's Zeitschrift' (1838). A cavalry officer stabbed himself with his sabre, which entered at the inner side of the left nipple, and passed out again one and a quarter inch from the vertebral column, and two inches above the os sacrum. The neighbourhood of the latter wound forthwith became strongly emphysematous. Symptoms of wounded lung alone manifested themselves, and perfect recovery followed within a month. Dr. Mehliss justly observes, that as no abdominal viscus appeared to have sustained any damage, the sabre may very possibly have passed through the thick layers of dorsal muscles into the external skin, and the diaphragm have escaped injury altogether. This, therefore, as well as other cases recorded, by Larrey

* Wahrnehmungen.

for example, of alleged recovery from penetrating wounds of the diaphragm, can hardly be received as fully established.*

Rupture of the diaphragm. By this term we would designate a separation of the muscular fibres, with simultaneous rupture of the serous investments. Actual transverse laceration of the muscular fibres is rare. The extent, attachments, and manifold as well as forcible uses of this organ, cause it to be more obnoxious to this lesion than any other muscle; and this, not alone through violent contusion of or falls upon the abdomen, but also through the straining of difficult parturition. Instances of rupture from excessive vomiting are scarcely to be relied upon. Contrary to Devergie, our author declares the fleshy portion to be far more liable to rupture than the tendinous, which, he affirms, suffers only when the accident occurs close to its confines.

Both authorities agree that the left side is incomparably more often affected than the right. It is highly probable that the protrusion of the abdominal viscera through the wound takes place with violence at the very moment of the rupture, and to a much greater extent than in the case of penetrating wounds, which would account for its being fraught with more peril, and more frequently productive of asphyxia. A good selection of cases is given, each illustrative of rupture from a different accidental cause. The first, is that of a man labouring under ascites, who fell forward upon his belly, and almost immediately died asphyxiated. The dropsical fluid was found to have entered and filled the thorax through a diaphragmatic rupture, an inch long. We have next an instance of rupture through contre-coup, in a youth upon whom a wall fell, burying him beneath the ruins. A compound fracture of the leg-bones occurred simultaneously. Death took place through asphyxia, the patient previously feeling as if something were tied round the heart. The stomach, colon, and omentum were found to have passed into the thorax, through a rupture four inches long and one and a half wide, and to have compressed the left lung. Mr. Wheelwright's case, cited from the 'Medico-Chirurgical Transactions,' vol. VI, furnishes an example of fatal hemorrhage, consequent upon rupture of the left side of the diaphragm, produced by falling from a coach. The extravasated blood filled the left cavity of the thorax. Finally, we have an instance of very extensive rupture, caused by the effort to prevent a cumbrous body from rolling down a steep descent. In animals of burden, and especially of draught, the accident is frequent from a cause similar to that last mentioned. One case is cited from the late Sir A. Cooper, and another from Percy, of the rupture occurring during *labour*. In the latter, the parturient, suddenly alarmed at seeing her husband fall from his chair, uttered a loud shriek, then feebly muttered a few words, gave birth to a child, and immediately afterwards expired. The phrenic rupture was on the left side, and five inches in length.

The above are all cases of longitudinal separation of the fleshy fibres; nor is the author aware of more than a single well-authenticated instance of their transverse laceration, namely, in a person who died from the effects of a fall, and in whom the front and sides of the diaphragm had become torn away from their sternal and costal attachments.

The diagnosis of phrenic rupture is exceedingly obscure. Since the

* The escape (in any quantity) of air through a recent *wound in the abdomen* affords positive evidence of the diaphragm having been pierced.

time of Celsus, authors have done little more than ring the changes upon his sentence. "Si septum transversum percussum est, præcordia sursum contrabuntur; spina dolet; spiritus rarus est; sanguis spumosus fertur." But in this there is nothing quite distinctive of the affection. It is a matter of surprise how such men as Larrey and Percy should have revived the obsolete fancy of rupture of the diaphragm being necessarily associated with distortion of the facial muscles. Still a sunken expression of the countenance will always be observable, where a portion of intestine has become actually incarcerated. Upon the whole, the practitioner has little to depend upon, except the locality of the pain (when present) and the presumptive evidence of transposition of the abdominal viscera, such as a sinking in and apparent emptiness of the abdomen; permanent dilatation of one side of the thorax, mostly the left; a preternaturally clear, or preternaturally dull sound on percussion; respiratory murmur, feeble or null,—borborygmi heard instead; heart's sounds obscure. Not every case of rupture, however, is attended with immediate displacement of the bowel; and again such displacement may be not recent, but of long standing, perhaps congenital; considerations of no little moment in medico-legal practice.

Spasmodic action of the diaphragm. No other muscle is so liable to become influenced by spasm as this. It is, however, far more frequently implicated in general spasmodic states than independently affected.

Tremulous or vibratory motion of the diaphragm. Many of our readers may have experienced this in their own persons. It is felt in the anterior portion of the diaphragm, giving rise to a sickly and anxious sensation. It is often both palpable and visible from without, in the shape of epigastric pulsation. Sometimes it alternates with painful spasmodic contraction of the rectus muscle, which, however, ceases on the individual bending the body backwards. Of a like nature is the trembling of the diaphragm in the cold stage of intermittent fever. Dr. Mehliss refers the phenomenon to irregular circulation of the blood through the diaphragmatic vessels.

Convulsive action of the diaphragm. Singultus or hiccup is produced by the periodical and sudden contraction and depression of the entire diaphragm, and the peculiar and abrupt sound that accompanies it, by air suddenly and forcibly entering at the mouth or nostrils to replenish the void caused by the sudden retraction of the root of the tongue. The exciting cause is, for the most part, irritation from the organs or parts subjacent to and in contact with the diaphragm, less frequently from those within the thorax. In animals, it can be called forth by irritating the cardiac orifice of the stomach.* We have not space to enumerate all the various circumstances that may give rise to this convulsive affection. It has, however, a fruitful source in reflex action, as instanced by the effect of intestinal worms, and the like in producing it. It may have its origin in mental emotions, or be transmitted by imitation from child to child; or attend the development or exercise of the sexual functions, or their suppression. It is a common and obstinate symptom in hysteria. All these instances are abundantly confirmed: Dr. Mehliss is, however, aware but of one authentic case of the irritation emanating directly from the roots of the phrenic nerves. It is that of Watmough, recorded in the 'London

* J. Müller, Handb. der Physiol., vol. i, 3d ed.

Med. Gaz.' for 1840, of singultus following concussion of the spine, and being overcome after six months' duration, by the application of a seton in the neck.

It is obvious from the foregoing that, as a symptom, singultus is little to be relied on, inasmuch as in one and the same disease, it may arise from a great variety of causes. Being difficult to imitate successfully, its presence may be regarded as distinctive of real from feigned spasm. Again its occurrence disproves any very grave affection of the diaphragm, whilst its being associated with pain demonstrates either the diaphragm itself, or a contiguous organ or part to be the actual seat of disease.

In no fewer than seven of the aphorisms of Hippocrates, is the occurrence of singultus during disease declared to be of evil omen, and the same belief long maintained its ground. The truth, however, is, that although in fever, as also in inflammatory disease of many of the organs and parts subjacent to the diaphragm, singultus denotes a grave character of the disorder, it cannot, singly, form the basis of any certain prognosis. Still Dr. Mehliss believes that in puerperal peritonitis, and in dysentery, the supervention of hiccup is generally prophetic of a fatal issue.

It is almost needless to state that the treatment of singultus must for the most part resolve itself into that of the disease upon which it depends. It is only where, from the violence or obstinacy of the symptom, life is either endangered or rendered burdensome, that direct treatment is admissible. But as no remedy has proved generally successful in such cases, we shall omit to specify all that have been tried, and simply refer to the sternutatories recommended by the father of medicine, and by which again Aristophanes is said to have been cured of obstinate hiccup, which we suppose the gods had afflicted him with as a punishment for the mischief which his farces had wrought upon the diaphragms of others. We will, however, also mention a strange and somewhat rude, but perfectly successful, method adopted by Cruveilhier in protracted cases. Having caused the patients to seat themselves with their heads bent backwards, he poured a continuous stream of water, to the extent of a gallon, into their open mouths. To augment the energy of the pharyngeal contractions, he from time to time allowed the water to enter at the nostrils, producing cough, symptoms of asphyxia, and convulsive movements in all the muscles of respiration. The well-known domestic methods of treatment such as frightening the patient, or endeavouring to astonish him either agreeably or otherwise, or again urging him to suppress by a voluntary effort the involuntary movement, are scarcely applicable except in the treatment of children.

Convulsion,—clonic spasm of the diaphragm. This is characterized by anormal contractions of the diaphragm, which succeed each other so rapidly that the muscle does not become perfectly relaxed during the intervals. For this reason the normal movements of the organ are greatly curtailed, and its functions considerably impeded. "A painful sense of tightness with vibration in the region of the diaphragm, suspension of respiration and of the power of utterance, together with a drawing in of the hypochondria, are the subjective signs indicative of these attacks, which attend general convulsions of almost every kind, whether epileptic, hysterical, infantile, or puerperal." Dr. Mehliss is, perhaps, right in deeming this affection to be generally referable to irritation of the roots of

the phrenic nerves. But the irritation would be more often a reflected than a direct one.

Tetanus, or tonic spasm of the diaphragm. Prolonged contraction of the diaphragm, in consequence of preternatural irritation. The muscle forms a rigid septum between thorax and abdomen. Yet the degree of spasmodic rigidity varies; passive movement being, to a certain extent, still occasionally possible. Where it is not so, a deep inspiration is succeeded by complete apnœa, and this again by pain in the diaphragmatic region, and a progressive sense of anguish throughout the chest. In the former case the breathing is uninterrupted, but imperfect, occurring only through the instrumentality of the intercostal and auxiliary pectoral muscles. It appears as if the patients were forcibly and voluntarily holding their breath, or straining at stool. The seizure is for the most part sudden and unannounced by any premonitory symptoms. During the seizure the hypochondriac regions always appear furrowed. The patients are compelled to sit up and bend the chest forward, unable to utter a cry, and hardly able to make known in broken sentences their sensations. The face is pale at first, but becomes bloated and blueish if the attack is prolonged. The pulse is either natural, or else quick, small, and soft. Such are the main characteristics of tonic spasm of the diaphragm, a peculiar form of phrenic asthma, differing from "angina pectoris," in the absence of that agonising pain felt at the region of the heart in the latter disease, in which, on the other hand, the pitting at the hypochondria is wanting. Moreover, neuralgia of the cardiac plexus is rather a disease of advanced life, tonic spasm of the diaphragm one of youth.

The well-known transient pain occurring during violent exercise, and commonly termed "*stitch*," is justly ascribed by Dr. Mehliss to partial spasm of the diaphragm, and not to sudden gorging of the spleen with blood, or pressure from flatus, as many writers maintain. The speedy relief afforded by bending the body towards the affected side, supporting the part with the hand or a handkerchief tied round the body, and slowly inhaling air, all of which is practised instinctively by children confirm our author's assumption. That the right side is so seldom affected in this manner, is here again probably owing to the support derived from the liver.

That the crowing inspiration of infants is for the most part the work of reflex action can hardly be doubted, and it is probable that the motory nerves of the diaphragm are here almost simultaneously affected with the nerves of the muscles of the glottis. But there is little evidence in favour of Dr. Mehliss's assumption, that spasm of the diaphragm here precedes that of the glottis, much less that the latter is dependent upon the former. For this would imply the necessity that the diaphragm should become relaxed first, which is contrary to experience, the crowing character of the first returning inspirations and the heaving of the upper part of the chest during their performance proving that the diaphragm is not yet released. In this instance Dr. Mehliss reasons altogether from false data, for he attributes the crowing sounds, not to inspirations, but to air forcibly expired. It must be admitted, however, that much obscurity still prevails with reference to spasm of the glottis. It is certainly due to a variety of causes, and amongst others, perhaps, now and then, where

there is great enlargement of the thymus gland, to pressure upon the phrenic nerves in their passage along the outer margins of this gland.

Paralysis of the diaphragm. This may result either from interrupted or impaired innervation, or else from destruction of its inherent muscular irritability. The latter kind, usually termed atony, is perhaps the most frequent, and at the same time most complete; for the diaphragm has too many sources of nervous influence readily to admit of their being all intercepted. Still, division of, or ligature applied to the phrenic nerves, in animals, is followed by sudden and fatal palsy of the diaphragm, as shown by Lower,* and more recently by the late Sir A. Cooper. Atony of the muscle differs in degree; when slight, it is manifest only where extraordinary exertion is used, as by panting in ascending stairs, &c., or in febrile states of the system when more severe it is conspicuous during ordinary breathing; in its highest degree it is denoted by respiration seeming to be almost entirely carried on through the abdominal muscles, by suffocative symptoms of various kinds, and finally by a double act of expiration. This grade occurs only as the closing scene to acute diseases, and is often incorrectly termed "paralysis of the lungs." With regard to the signification of "abdominal respiration" it must be admitted that much error prevails. In *complete* paralysis of the diaphragm, inspiration is effected through the agency of the pectoral muscles, whilst the abdominal muscles, by firmly contracting, fix the base of the thorax, the diaphragm having no longer the power to perform this office itself. Expiration is then, so far as the abdominal muscles are concerned, simply a cessation of those contractions,—a momentary relaxation. Hence the action of the abdominal muscles is here only antagonistic with that of the muscles by which respiration is, in reality, still carried on.

The duplex act of expiration is caused by the passive diaphragm, mechanically depressed by the inhaled air, relapsing by two steps, as it were, into its natural position. Thus, at least, Dr. Mehliss seeks to explain it. It seems more probable, however, that the first act is referable to collapse of the upper part of the thorax; the second to a return of the diaphragm to its place, through the propelling aid of the abdominal muscles.

Atony of the diaphragm may be produced by inordinate exertion of the muscle, such as climbing lofty mountains, by anæmia, by pulmonary disease, by parturition, or by spasm; or, mechanically, by the pressure of dropsical or other accumulations within the abdomen; or, as Stokes has shown, within the thorax. Where the abdomen is their seat, the evil consequence to the diaphragm is scarcely felt until after the removal of the offending matter, and the danger from this source of incautiously instituted paracentesis of the abdomen is well known.† Rapid delivery of a full-grown fœtus is not rarely fatal, for the same reason. The permanent relaxation of the diaphragm, from accumulations within the abdomen, is sometimes a source of angina pectoris, owing to the position which the liver is thereby made to assume relatively to the heart.

After the *cautious* removal (where possible) of that which caused the

* Phil. Trans., vol. i.

† Persons in whom there is a great accumulation of fat about the abdomen are for the same reason often much injured bp the use of drastic purgatives.

paralysis, and the employment of effectual means to prevent its return, we have little more than a dietetic course to rely upon for the further treatment. This should, however, include a bracing air, and the frequent use of the cold bath. For a length of time the bowels will probably not act without the aid of aperients,—in the selection of which, however, the practitioner should bear in mind that *tone* is the element most needed, —reduction of strength the evil most to be avoided.

Interrupted or impaired innervation, as a cause of paralysis of the diaphragm, has been as yet little investigated or even thought of. The subject is altogether a very obscure and difficult one, and the few cases cited by Dr. Mehliss, are either too imperfectly related, or too complicated, to admit of any conclusions being built upon them.

Rheumatism of the diaphragm. This, when occurring during the course of acute muscular rheumatism, is often extremely painful. It is, however, more frequent as a mild, independent affection, in males of a nervous, but more particularly in females of a hysterical habit. It is most common in wet seasons, especially towards the latter end of autumn, when its occurrence is sometimes epidemic. It infests ill-ventilated underground tenements, and especially the low, damp dwellings of the poor. It consists in a dull pain, extending from the ensiform cartilage and the inferior extremity of the sternum to the spine. A somewhat deep inspiration sensibly increases the pain, which, though never very acute, occasions much distress, as denoted principally by dejected spirits and a marked expression of anxiety depicted in the countenance. The tongue is generally clean, but of a blueish cast; the appetite not deficient, but even a moderate meal invariably occasions several hours of increased suffering. The bowels are sometimes obstinately costive; at other times tolerably regular in their action, which latter is, however, attended with great augmentation of the pain. The complaint, if not interfered with, will persist for weeks, if not months, and then gradually subside; in which case, however, there remains a great liability to its frequent recurrence. The heart's action is, for the most part, turbulent, insomuch that we have repeatedly seen cases which had been mistaken for organic disease of the heart. In the majority of these cases we have found it requisite to confine the patient for some days to bed, and to cause a succession of mustard poultices to be applied at intervals, alternately to the epigastrium and to the back. Warm diluent drinks, with occasional doses of Dover's powder, and enemata of warm water (purgatives being here decidedly injurious) have generally sufficed to effect a tolerably speedy cure. With respect to diet, it is necessary to restrict the patient, both as regards the quality and quantity of his food. But as transgression in either respect is attended with immediate punishment, the temptation to it is not great. Such is the disease in its uncomplicated form. We have sometimes observed the patient's urine to be quite clear, often to deposit lithate of ammonia, but scarcely ever to be so charged with the purpurates as Dr. Mehliss would teach us to believe. And here we may remark, (without intending any play of words) that whenever Dr. Mehliss details *cases* of his own, they give one the impression of being too highly coloured to resemble Nature very accurately. Portraits of disease will seldom admit of all the minuteness and finish of a Gerard Dow. There is, perhaps, no source of error either more fruitful or more common than working up to preconceived opinions!

Inflammation of the diaphragm. Little reliance can be placed on the accounts given of this affection by ancient authors. There is little doubt that it does occur now and then, in consequence both of wounds of the organ and of internal disease. In the latter case, however, it either originates or else merges in inflammation of neighbouring parts, so that its individual features are not sufficiently marked to offer a distinct portrait.

Diaphragmatic pleurisy has no other features to distinguish it from ordinary pleurisy, besides the locality of the pain and its violence during cough, the extreme dyspnœa, and, finally, the occasional accompaniment of singultus. Andral, however, mentions also the presence of nausea and vomiting, even in certain cases in which the stomach was found quite healthy. Sometimes a very distressing hiccup attends convalescence, —or adhesions take place between the lung and diaphragm, giving rise to permanent shortness of breath. When the right side is affected, the liver often becomes implicated, as is then manifested by icteric symptoms, &c. and the disease is often mistaken for hepatitis. Diaphragmatic is more perilous than costal pleurisy, on account of the greater embarrassment to the respiratory function. The treatment is that of ordinary pleurisy. The singultus attending some cases during convalescence, is said to have been successfully combated with the tincture of stramonium.

It is scarcely likely that the *peritoneal investment* of the diaphragm is ever generally and independently inflamed. This coating is not a continuous one, but is interrupted by folds, thrown off to several of the abdominal viscera, so that its extensive inflammation must needs involve that of one or more of the said viscera. Singultus, dyspnœa, phrenic pain, are common symptoms of the inflammation of organs subjacent to the diaphragm; if, therefore, we even admit the occasional occurrence of simple peritoneal diaphragmatitis, we are familiar with no symptoms distinctive of it. The disease is not unfrequently reflected from the pleura to the corresponding portion of the diaphragmatic peritoneum, which is then found, after death, to have undergone thickening and adhesions; the latter sometimes inclosing tuberculous deposits. In almost all these instances, the intervening muscular fibres appear perfectly healthy. Is not the singultus, before stated to occur now and then during convalescence from diaphragmatic pleurisy, probably dependent upon this implication of the peritoneal coat?

Malformations of the diaphragm. We shall confine our notice to such as would seem at all instrumental in engendering disease. *Narrowness of the foramina* deserves to be mentioned in the first place; that of the aortic foramen, recognized by Haller, may occasionally have some share in the production of thoracic aneurism. Of narrowness of the other foramina no authentic cases have been recorded; at the foramen quadrilaterum it is not very likely to occur, owing to the strong tendinous fibres by which that passage is protected. There are better grounds for believing dysphagia to be now and then a consequence of this malformation at the foramen œsophageum.

A more important vice of organization (where it can be deemed such) is that denominated *diaphragmatic hernia,* and which consists in the formation of a diverticulum or hernial sac, the short end of which is directed upwards, within the thorax, whilst the base opens into the abdomen. This sac is, for the most part, diminutive, but occasionally exceedingly

capacious, and it always contains a smaller or greater proportion of abdominal viscera. The parietes of the sac sometimes consist of the serous investments of the diaphragm only,—the muscular fibres having merely undergone separation; at other times the walls include the muscular fibres, and the hernia is simply a partial distension of the organ along with its serous coats. In the majority of cases these herniæ have been accidentally discovered during the dissection of bodies, without any symptoms having even denoted their existence. In a few of the recorded instances, however, the enormous mass of abdominal organs, both hollow and parenchymatous, contained in the sac, cannot have failed to produce the greatest disturbance to the organs, both of circulation and of respiration. There are examples of a violent blow struck against the abdomen, sufficing to produce fatal hernia at the anterior portion of the diaphragm, situate between the sternal and costal portions, where no muscular fibres occur, and the two serous sacs therefore meet. Hernia occurring in the body of the diaphragm is probably, in most instances, dependent on congenital malformation.

Ectopia, or the displacement of abdominal organs through an opening in the diaphragm is discussed by Dr. Mehliss at considerable length, with a view to discriminate between the cases dependent on arrest of primary development and those resulting from accident or disease. Total absence of the diaphragm is only met with in the instance of acephalus. Four marked cases are cited of infants who did not long survive, and in whom the greater portion of the left half of the diaphragm was found wanting. In each the left thoracic cavity was found encumbered with an enormous mass of abdominal viscera. In the adult the inference is in favour of congenital malformation: 1st. Where the opening is very wide, and the edges of the wound smooth and even, presenting no marks of cicatrization. An acquired rent of the same extent must needs have proved speedily fatal. 2d. Where there is actual want of substance. Mere rupture could not occasion this. 3d. Where the opening occurs in the tendinous portion, where the fibres are least of all likely to give way.

The passage of pus and other fluids, or of acephalocysts through the diaphragm, is generally followed by collapse and cicatrization, or else by adhesions between the muscle and some contiguous organ, so that no permanent opening becomes established. It has been before stated, that an important symptom of extensive displacement of the bowel, consists in a notable sinking in of the abdomen. This is not, however, invariably perceptible; for it is remarkable that Nature appears bent upon compensating for the dislodged viscera, by causing the dilatation or enlargement of those that still remain. The liver, in particular, is then sometimes found to have attained a prodigious size. Of the descent of thoracic organs into the abdominal cavity, through an opening in the diaphragm, we have no well-authenticated record. Such an opening did not appear to exist in Vetter's interesting case of a vigorous man's heart being situate in the left hypochondrium, where it could be grasped with the hand, and its contractions and dilatations distinctly felt.

The question of how the opening originated is chiefly interesting in a medico-legal point of view; since the lesion itself, however produced, is obviously beyond the reach of art. Still the mere suspicion of its ex-

istence,—(for certainty there cannot be)—offers sufficient grounds for the immediate employment of those precautionary measures into which the treatment of this grave affection must resolve itself. Of gastrotomy, for the purpose of replacing the extruded viscera, though seriously suggested by Laennec,—there can surely be no further question at this time of day.

Adhesions of the diaphragm with neighbouring organs are frequently met with as a consequence both of superficial and latent inflammation, and of other destructive diseases. They of course seriously interfere with the functions of the diaphragm, more especially when occurring with the subjacent organs. Nothing, however, with certainty denotes their existence during life. Dr. Mehliss infers from a case observed by himself, that the circulation may become seriously embarrassed by these adhesions involving the foramina through which the great blood-vessels traverse the diaphragm.

Parasitic formations in or upon the diaphragm. The various kinds of eruption stated to have been found on the diaphragm, are all referable to an earlier period of pathology. We must restrain the temptation we feel to give our readers the benefit of a full-length portrait, drawn with medieval circumstance by Lanzoni, of a lascivious female, (salacissima quædam muliercula), who, being alarmed during the act of parturition (?) fell into a convulsive state, with alternate fits of hiccup and of laughter, which, after forty hours' duration, terminated in death. The diaphragm was found "undequaque vesiculis polyporum colore protuberans, quarum nonnullis phlebotomiâ adopertis, emanabat serosus liquor, colore aqueo, sed acidissimo odore imbutus." Dr. Mehliss regards this as a case of vesicular eruption,—Joseph Frank, with more verisimilitude, as one of hydatids. A remarkable case of hydatid cyst is recorded by Pohl, (Diss. Lipsiæ, 1747). It was twenty-six inches long and fifteen wide, and situate at the inferior surface of the diaphragm, which it had forced far into the thorax. A longitudinal incision gave escape to some 200 acephalocysts, of various sizes, from that of a fist downwards. The narrator believed the principal cyst to have formed through distension of the cellular tissue situate between the muscular fibres and the peritoneum. The subject was a female, aged 26, and far advanced in her third pregnancy. The trichinia spiralis has occasionally been met with in the muscular substance of the diaphragm.

But we have already somewhat surpassed our proper limits, and must needs conclude. Those amongst our readers who are familiar with the German language, will do well to procure Dr. Mehliss's work, which, upon the whole, cannot but be regarded as a valuable contribution to pathological literature.

ART. XII.

1. *Erste Gründe einer Physiologie der eigentlichen thierischen Natur thierischer Körper: entworfen von* Dr. JOHANN AUGUST UNZER.—*Leipzig,* 1771.

Principles of a Physiology of the Animal Nature, peculiar to Animal Bodies. By Dr. J. A. UNZER.—*Leipsic,* 1771. 8vo, pp. 734.

2. JOHANN AUGUST UNZER'S *Physiologische Untersuchungen. Auf Veranlassung der Göttingischen, Frankfurter, Leipziger und Hallischer Recensionem seiner Physiologie, &c.—Leipzig,* 1773.

UNZER'S *Physiological Inquiries: in reference to the different Reviews of his Work on the Physiology of the Animal Nature.—Leipsic,* 1773. 12mo, pp. 149.

THE Principles of a Physiology of the Animal Nature peculiar to Animal Bodies, propounded by Unzer, is a work on Vital Dynamics generally, but specially on the Dynamics of the Nervous System.

In our original prospectus, issued twelve years since, we announced our intention of giving *retrospective reviews:* our readers, therefore, ought not to be altogether unprepared for the present article. But in truth it was not to fulfil our original intentions that we undertook the examination of Unzer's book, but rather that having our attention called to the volume during our investigation of the doctrines of Prochaska, we were struck by the lucid views and close reasoning of the author; and indeed, as to some points, by the novelty of the opinions propounded. We learnt too, that while anatomy had advanced so largely, vital dynamics had made little progress; or if it might be allowed that in some points of doctrine the science had advanced, in others it had really retrograded. In short, we felt that on the whole the movement onwards of the philosophy of life was "*cum exili et quasi contemnendo progressu,*" and that good service would be done to science by reproducing doctrines like those of Unzer, and giving them that wider scope of application which an improved anatomy admits of, and which, indeed, the loud claims to a newer and truer philosophy of certain modern neurologists imperatively demands. Nor is the review of such a work less interesting and amusing than it is instructive. We witness a powerful mind extending its *palpi* into paths which we now traverse freely, and trying to penetrate a gloom which to us is effulgent light. We anxiously watch his approach to some truth now familiar to us; and then see his steps turned away just when about to seize it and emerge from the surrounding darkness. But we see, too, that if he have turned away from truths lying at his feet, he has overlooked them because he has been engaged in straining his eyes towards the far horizon; and that it was the wide extent of his view which led him to overlook the near and the obvious;—the grandeur of his conceptions, which wiled him from the full enunciation of those principles on which others have since hoped to establish an exclusive reputation.

In introducing the subject of his book to the reader, Unzer sets out by placing an imaginary corpse before himself. Regarding this attentively, he sees nothing there but matter subject to the ordinary forces of matter. There are solids with the property of cohesion; fluids subject to the laws of hydrostatics. The arteries are tubes contracting on the finger in virtue

of their physical elasticity, and receiving the injections of the anatomist simply according to the laws of hydraulics. The heart beats not; the contracted muscles maintain the limbs in a motionless position; the machinery of the body is still.

In the living organism matters are far otherwise. It is true the elementary forces of matter act on its components, but other forces are seen in operation upon them, acting according to other laws, and in a definite order. The stimulus which excites no motion in the lifeless heart, in the dead muscle, or in the devitalized arteries, maintains the circulation in the living body, alters the pulse, and excites contractions of the muscles and movements of the limbs. These forces which the living animal possesses, in addition to those of the dead, are "the proper animal forces or powers," (die eigentlichen thierischen Kräfte,) and give it that nature which the author terms "the proper animal nature of animal bodies," (die eigentliche thierische Natur thierischen Körper.)

The reader will better understand the analysis of Unzer's doctrines if he will form a distinct and definite conception of what the author means by the word *Kraft*. This word corresponds to the Latin *vis*, and to the English words force and power. The *force* of gravity is the *power* of attraction inherent in matter; the centrifugal *force* is the *power* of repulsion. The two words are continually used as synonymous, but they are not strictly synonymous. Force is power in the abstract; power is force in action, according to law and arrangement. The elastic force of vapour is the power of the steam-engine. The term power has been used also to express the property or quality of a body; as, for example, the "conducting power" of various substances has been ascertained by experiments in thermotics and electricity; how incorrectly, if the two words, force and power, be synonymous, is made manifest by simply substituting the word force for power, for we have then the conducting force of iron as regards caloric, the conducting force of water as regards the magnetic imponderable. That Unzer means to use the word *Kräfte* to signify primary powers or forces of living matter, just as we speak of the force of gravity, the power of attraction, &c., is manifest from the context and the sentiments of the whole work:

"The ordinary science of physiology considers the forces or powers (Kräfte) of animal bodies in their natural condition and as they act together in connexion with each other, but without distinguishing the simply physical and mechanical from the proper animal forces or powers (Kräfte). This presupposes that we know the laws according to which each of these separate forces or powers (Kräfte) operate separately, and indeed as to the physical and mechanical, whose laws are known, there is generally in reality no difficulty. The physiological works of Haller teach us, in a manner almost impossible to be surpassed, the mechanism of all parts of the animal body, of which the structure develops the functions according to the laws of mechanics, hydrostatics, hydraulics, optics, acoustics, &c. But do we know those laws by which the proper animal forces or powers (Kräfte) govern the body when acting separately from the physical and mechanical, and independently of them? Truly no! or at least very imperfectly. The thoughts and desires or emotions of the soul or mind [Gedanken und Begierden der Seele] are animal moving forces or powers (Kräfte) of the animal body. But do we at this moment know anything of the laws by which these forces regulate their appropriate mechanism (Maschinen)? or have we hitherto troubled ourselves to observe the operation of these laws in each particular class of ideas and desires? We have disputed stoutly enough, whether the soul be matter or brain; whether

thought be an electric fire or a movement of the vital spirits; whether souls and bodies exercise a real or an ideal influence on each other; whether souls form their bodies, or whether they are diffused through them or dwell only in the head; whether an instinctive impulse (Trieb) or a passion belongs to the body or the soul; or whether the vital spirits be elastic or inflexible, electrical or etherial, &c.; and all this has been done without any beneficial results whatever, but rather the contrary." (Preface.)

It appears, then, that Unzer had in view a clear and definite object, and that was to rival Newton and the physical philosophers (the physiciens, as they are yet termed in France), by demonstrating the existence of a new and independent class of forces or powers inherent in living matter, and by definitely fixing the laws by which they act. To facilitate the comprehension of his views, we will designate these forces or powers as the *vital*, —or, if that term be too hackneyed, as the *biotic*—forces or powers.

Seeing how much philosophers had erred in their inquiries, on the one hand, by referring, with Stahl, all the animal forces or powers to the mental force only; or with the mathematical and mechanical, by excluding the idea of vital forces, and referring everything to the laws of matter only, our author was induced to reconsider the question, to separate the vital forces from the physical, and to analyse and arrange the former according to their manifest phenomena; in short, to consider *the physiology of the proper or peculiar animal* (*biotic*) *nature of animal bodies*, as distinct from the physiology of the whole animal mechanism. He acknowledges the imperfect nature of his views, but modestly expresses a hope "that some other and better students of animal nature, of which our time affords so many, and with whom he is not to be put in comparison, stimulated by these researches, may draw up and produce a much more comprehensive system," he being contented with the honour of having struck out the idea; or, if he might flatter himself that some much more eminent person than himself, adopting his plan of a physiology, would bring out a pathology of proper biotic nature, he would esteem himself fortunate in having the honour of paving the way to such an improvement in the art of medicine.

Plan of the work. Unzer having explained his general views and objects, gives a sketch of the plan he proposes to adopt in developing the laws of the biotic forces or powers. Their seat or origin is in the so-called proper animal machinery or mechanism, namely, the brain and nerves, furnished with the vital or animal spirits, or nervous fluid (Lebensgeister—Nervensaft), through which they are communicated to the mechanism or machines of the body. These "machines" of the body are in fact the *organs* of the body as displayed by anatomists, and are divided into two kinds, according to their uses. Those are simply mechanical in which the laws of physical science are seen operating, as the bones, considered as levers; the tendons as pulleys, the arteries as tubes, &c.; while those are "*animal*" which are directly subservient to thought, and in which the forces of life are seen to act. The term *maschinen* is evidently used as correlative with the biotic forces or powers: these put in motion the "*machines*" just as the elastic force of vapour puts in motion the steam-engine. As, however, Unzer considers muscular fibre "a machine," the term *organ* is scarcely synonymous with "*maschinen*," we shall therefore translate it literally,—machines, thinking it better to sacrifice ele-

gance to a truthful expression of the author's meaning. The animal or biotic machines are the brain and nerves, or the whole nervous system; the mechanical machines are the muscles, tendons, skin and membranes, blood-vessels, glands, viscera, &c. Many of these latter are, however, considered to be endowed with biotic forces, but only secondarily, by means of the nerves ramifying through them. This will be referred to again.

Biotic forces may act either with or without the consciousness and will of the animal—or, literally, the thinking force or power—*Vorstellungskraft*. Now, this is a new force, and it will be well to ascertain the precise meaning our author wishes to express by it, since it plays an important part in his physiology. Looking into the context we find it is used in two or three senses. It means soul or mind, since it is used synonymously with "Seele." It further means sensation, or consciousness of an impression, or of the changes induced thereby in the brain, so that an animal with this kind of "Seele" (*unvernüftige*) is a sensational animal as opposed to an animal with reason—*vernüftige Seele*. It is also used to signify the power of thought and will, or mind, in its highest meaning, and in this sense the *Vorstellungskraft* can excite or originate those material changes in the brain, which are necessary to thought and volition, as well as be conscious of them when excited by impressions only. Thus sensation, or consciousness of impressions, is not, in Unzer's views, a primary force, since it cannot act on the brain or system generally without being excited; on the other hand, the mind or soul is a primary force, inasmuch as it can originate material changes in the brain and nervous system.

There are a few other terms, whose precise signification it will be subsequently necessary to define, but the preceding is sufficient to enable us to understand the author's plan.

The First Part considers the physiology of the brain, and includes the operations of the mind, whether sensational or reasoning. In the Second Part the physiology of the spinal cord and nerves is expounded, and *reflex phenomena* are fully defined and analysed. In the Third Part we have the economy of the *whole* of the *animal* forces or powers considered; a definition of animated nature and an arrangement according to leading types, and especially indicating animals without consciousness; animals with sensation only; and animals with reason. And lastly, an essay on the origin, life, perfection, decline, and death of animal nature. Such is the plan of Unzer's work as laid down by himself; and, after again modestly acknowledging imperfections, inviting his readers to do better than he has done or can do, declaring his willingness to profit by fair criticism, and his determination not to notice personalities, he addresses himself to his task.

In the first chapter of the first part, he states his views of the nature of the machines subservient to the biotic and psychical powers of animals, or in other words, of the anatomy of the nervous system in man and the lower creatures; these are principally taken from Haller. The brain—the seat of mind, or "the soul"—and the nerves, are the two great portions of the nervous system, the latter all ending in the former as the great centre; the spinal cord being considered according to the current doctrines, as in some degree a bundle of nerves. With regard to the ganglia, he thinks that, like the brain, they are endowed with the property of reflecting impressions.

"It is also a settled fact that the ganglia are always larger than the nerve arising from it, and sometimes very remarkably so. It is also certain that almost all the ganglia give off more nerves than they receive, and that they also as well as the brain give origin to nervous twigs; whence it is very probable that they also, like the brain, reflect certain impressions that they have received, to the limbs they govern along the new nerves that spring from them, although the impressions do not in this case become objects of consciousness, as in the brain. But more of this afterwards." (p. 30.)

The nerves are motor and sensory, and are endowed (in common with the brain) with the vital spirits or nervous fluid, (Lebensgeister—Nervensaft) the office of which is to carry impressions to and from the brain along the nerves. It is formed from the blood-vessels in the gray matter of the brain, and may be increased or diminished, and the functions of the nervous system changed accordingly. Whatever impairs its production in the brain, or hinders its transmission along the nerves, interrupts the action of the powers or forces of the mind and the nerves. Thus congestion, or compression of the brain, or division of the nerve, will stop the operations of the vital spirits or nervous fluid. On the other hand, sleep, wine, and alcoholic stimulants, agreeable passions, pleasant mental stimuli, &c., increase the quantity of the vital spirits and correspondingly exalt the functions of the nervous system. All this, however, is stated by Unzer in deference to authority only—that of Haller. Having propounded the current opinions, he dismisses them with the remark, that little is known respecting the nature of these vital spirits, and that little is very uncertain and at the best only probable; and further, that they really are not necessary to the study of the laws of the proper animal forces. Henceforth therefore, he says no more about them. He seems glad to get rid of them, but influenced by conventional feelings, buries them in forgetfulness with ceremonial respect. In his answer to one of his reviewers, he again maintains that the theory of "vital spirits" is not necessary to his views.

Physiology of the brain: its biotic forces considered as bio-psychical forces (thierische Seelenkräfte). After observing that the alternate elevation and depression of the brain is a mere mechanical movement, and dependent on physical forces, Unzer proceeds with the proper physiology of the brain as a vital or biotic "machine." First, he observes that the brain is the seat of mind or "the soul,"—that is a fundamental proposition which all experience proves, conflicting statements to the contrary notwithstanding. It is also a compound organ, for certain parts being injured certain conceptions or sensations do not occur, and the ideas, desires, and instincts connected with them are prevented or disappear. The property or power which Unzer attributes to the brain in particular, is that in virtue of which those changes that are necessary to every act of thought are excited; but he shall speak for himself.

"When a thought arises in the mind, a change must necessarily occur concurrently therewith, without which the conceptive faculty (Vorstellungskraft) cannot act; and when this change occurs in the brain, the conceptive faculty is necessarily excited into action. Whatever may be reasoned on the matter, a change in the brain must consist in a movement (Bewegung), and the medullary matter must also be endowed with a power of exciting movement (Bewegendekraft), which acts in harmony with the mind (Vorstellungskraft). So that each distinct class of perceptions is always connected with a certain animal (biotic)

movement, and with these movements a certain class of perceptions, for it is ascertained from numerous observations that after certain injuries of the medullary portion of the brain, especially that part from which nerves of sensation arise, certain kinds of perceptions, as for example, certain sensations (Empfindungen) are prevented or disappear, and together with them all the ideas, desires and instincts dependent thereon, as well as other faculties of the mind. This power of the brain to excite movement (die Bewegendekraft) which is thus connected with the mind and power of thought is an animal soul force (thierische Seelenkraft), and hence arises *the fundamental general principle in the doctrines of the connexion between body and mind, that the medullary matter of the brain possesses an animal soul-force* (thierische Seelenkraft), *by means of which at every act of mind* (Vorstellung in der Seele), *whether it be a sensation* (Empfindung), *imagination* (Einbildung), *desire* (Begierde), reflection (Betrachtung), *or influence* (Einfluss), *it* [the brain] *produces in itself a certain animal movement necessary thereto, without which the act of thought can neither arise nor continue, and with which it infallibly arises and continues.* This animal soul-force is peculiar to the brain, and is the property of no other animal machines, because in none other except in the brain does an animal movement develop perceptions. The medullary matter of the brain can also with propriety be designated the only instrument of the conceptive power of the mind, by means of the animal movements of which it [the mind] puts its force into action, and continues it [in action] and without which [the animal movements of the medullary matter] it would absolutely remain inactive. Philosophers have already introduced the phrase *material ideas* to express those movements in the brain that are necessarily connected with each act of thought. (Baumgärtner's Metaphysics, § 416.) A psychological materialist considers these material ideas as the ideas of the mind itself. But since it must be firmly maintained that the thinking faculty is an immaterial substance (Substanz), it cannot certainly be granted that these changes in the brain are really ideas; but since the two are inseparably connected with each other, and the mind never acts without these movements nor can act in animals, it is fully established that every act of thought presupposes and causes a movement in the brain (material idea), and every such movement in the brain presupposes and causes an idea in the mind (Vorstellung in der Seele.)" (pp, 41-2.)

Unzer's doctrine of "material ideas" differs in little respect, if any, from that of the most esteemed metaphysicians. All acknowledge and maintain that a change takes place in the brain with each act of thought. Reid, for example, says, "In perception the object produces some change in the organ; the organ produces some change in the nerve; and the nerve produces some change in the brain." Newton, Dr. S. Clarke, Locke, Malebranche, Hooke, Descartes, Bonnet, and others held similar doctrines. In adopting it Unzer expressly cautions his readers in a special note against a misapprehension of his meaning. He has no intention of giving the notion that there are hieroglyphical figures on the brain, or impressions literally made upon it. Unzer's "material ideas" are altogether different from Dr. Laycock's "psychical substrata." Dr. Laycock wishes to express by that term, certain changes in the molecular constitution or arrangement of the neurine induced by frequently recurring *impressions communicated from without*, and corresponding to those impressions and to none other. These "psychical substrata" are therefore part of the constitution of the body; consequently like other organic properties, are transmitted from parents to offspring, and constitute the foundation of characteristic or acquired instincts, propensities, &c.*

Unzer uses as synonymous with "material ideas" the term "impressions,"

* Essay on the Reflex Function of the Brain.—By T. Laycock, M.D.—British and Foreign Medical Review, Vol. XIX, p. 308.

inasmuch as by the latter he wishes to signify the continuous changes which continued operations of the mind excite in the brain. He also uses, but once or twice only, the term Bilder der Vorstellungen—images of conceptions, to express the same thing. However, he expressly maintains that the strength of a perception or conception is in proportion to the activity of the movement, or in other words, to the intensity of the material idea. A large material idea comprises or is made up of smaller ones, and this constitutes a large conception (grössere Vorstellung). For the term "animal soul-force," used to designate the efficient cause of these changes, and which is peculiar to the brain (quotation, p. 186), we shall substitute the term cerebral power.

Conceptions may be of two kinds. The one are dependent on movements in the brain (material ideas), which are induced by impressions on the senses; the other are dependent on movements of the brain (material ideas), induced by the conceiving force or power of the mind. The former are termed sentient or sensational (sinnliche) conceptions, or simply natural conceptions; the latter are termed arbitrary, or spontaneous, or "physiologically free." These terms are derived from Baumgärtner, who appears to have followed Locke. Unzer specially distinguishes between these two classes of ideas or conceptions. Those of sensations are altogether dependent on impressions made on the brain; the ideas of the intellect are dependent on the immaterial essence—the soul. As no conception can occur without this change in the brain termed a material idea, Unzer proceeds to inquire how these changes are excited in the brain, and what actions they induce in the animal economy; and this leads him to the consideration of the biotic force of the brain, for that is the force which develops the material ideas, and his first chapter is on external sensual impressions.

The biotic properties of the nerves. The nerves, Unzer observes, have two points; the one is that connected with the brain, the other is the termination in the body, or on the surface. If either end be irritated, the irritation is propagated to the other extremity. If a nerve be irritated in any part of its course, irritation is propagated in both directions; and if it be cut, and the cut ends irritated, if that next the brain be touched, the irritation is propagated to the brain; if that next the periphery, the irritation goes to the periphery. When an animal apparatus (a nerve) is so influenced, either by a touch or a transmitted movement, or by any other impression of whatever kind it may be, that animal actions (Wirkungen) are excited, the change thus induced in the "machine" is termed a sentient impression (sinnliche Eindrück) or nerve-feeling (Nervengefühl). Thus, a sentient impression made on a nerve will travel from the cerebral to the peripheral extremity, and *vice versâ.* If it be made on the peripheral extremity or on the trunk of the nerve, and goes on to the brain, Unzer distinguishes it as an *external* sentient impression: or as a synonym, a "nerve-feeling *from without inwards.*" But the irritation must excite changes in the animal "machine" to become an impression. Light may be applied to the most delicate and most exposed nerves of the skin, but it excites ordinarily no external sentient impression. The excitation of the requisite change is purely animal, and quite unconnected with the mechanical or physical laws of matter. When the medulla of the nerve is touched, and the most energetic animal actions follow, as conceptions of the mind; or movements are excited by the cerebral forces, or

simply by the biotic forces, not the slightest change is observable in the nerve itself. Very often indeed it happens that the physical force of the impression has no relation to the amount of action excited, for the slightest touch is often followed by greater results than a strong pressure, thrust, &c. Nor is there anything in the mechanical structure of the nerve itself which can explain the propagation of the change or impression.

It is an unfortunate circumstance that we have no English word which expresses this faculty or property of living matter to receive and react on impressions, and which is as much a property of the microscopic cell, as of the nerve of special sense with its highly organized apparatus. By Unzer it is termed their Sinnlichkeit, which may be translated sentientness, or sentiency, and the change itself, which the nervous matter undergoes on the application of a suitable stimulus, a *sinnliche Eindruck*, which we have translated as *sentient impression*. The phenomena resulting from that change would therefore be properly characterized as *sentient* phenomena.

The great objection to this and all other expressions derived from the Latin verb *sentio* is this, that, let the true meaning be guarded by the context ever so strictly, as not implying consciousness or any mental act, still they are continually considered to imply the latter, because in ordinary language these terms invariably do imply consciousness. A sentient being is a percipient or thinking being. Whytt attributed these phenomena to a "sentient principle," as opposed to a "rational principle," but the use of this phrase, although expressly defined by him as not implying consciousness, led to a complete misapprehension of his views, as we have formerly shown (Vol. V, p. 524). *Sensation* and *sensibility* have also been very commonly used by numerous writers to express the cause of these phenomena, as by Prochaska (Vol. XXIII, p. 212), Alison, and many French and German physiologists of the present day. These terms have, however, given rise to the same misunderstanding as the preceding.

Some writers have designated these phenomena from the mode in which they presumed them to be produced. They have thus been termed sympathetic actions, involuntary, excited, excito-motory, reflex acts. Several objections have been necessarily raised to such terms. In the first place, they do not sufficiently mark one of the leading characteristics of the phenomena, namely, their adaptiveness to the well-being of the organism. Secondly, the vital mechanism in which they take place is incorrectly limited either to a "sympathetic system," or a "true-spinal system," &c. Thirdly, the sentient-like characteristics of the phenomena are pressed as strongly opposed to the more material causation advocated by this class of physiologists; thus, the Stahlians referred them to the operation of the unconscious *anima;* Whytt (influenced, doubtless, by the Stahlian doctrines) to an unconscious sentient principle; and others to sensation or sensibility.

It is certainly impossible to leave the leading or chief characteristic of adaptiveness out of consideration, in devising a name for the class of phenomena referred to. Adaptiveness is seen throughout the whole of creation—in the planetary system as well as in the terrestrial organisms. The vital changes and actions resulting in organized matter from suitable stimuli are *like* those of sentient beings, but it is plain they are not the result of individual thought or mind. On the contrary, they are manifestly the result of a power of adaptiveness in matter, acting universally,

and therefore in all organisms without exception, and subject, like the universal property of gravity, to general laws. Of this power we can know nothing but by its effects; we can only say, they are *like* those of sentient beings, and the property is sentient-like in its operations. We will therefore venture to translate this into an unappropriated Greek term, namely, homæsthetic, from ὅμοιος, *similis*, like to, and αἰσθητικὸς, *sensu præditus*, sentient.

The property of living matter, then, whereby adaptive actions and changes are excited in that matter, independently of volition or consciousness or thought, is homeoæsthesis, or abbreviately, homæsthesis, and the actions and changes themselves are homæsthetic, and this without reference to the class of organisms in which they occur, that is, whether they be animal or vegetable.

To recapitulate. When by rays of light, a change in temperature, or the contact of matter in any form, a vital change is induced in a nerve or ganglion,—that is an impression; if mere adapted actions result without consciousness, the impression, actions, and changes are homæsthetic; if there be consciousness of any of these, it is a sensational impression, and the actions are psychical. That the reader may the more easily comprehend Unzer's views, we shall substitute these terms for his own polysyllabic and uncouth phrases: for example, the phrase *sinnliche eindrücke* will be properly rendered by the term *impressions*, simply, as that includes the idea of adapted actions expressed by the word *sinnliche*. We allow that as few new words should be introduced into science as possible, but if any of our readers will improve on our coinage, they will do the literary state some service.

When a nerve of an animal possessed of mind receives an external impression, it is either propagated onwards to the brain, or is arrested in its course thereto. If it arrives at the brain it there excites through the nerve "material ideas," and their corresponding conceptions or perceptions. These latter are termed, when thus excited, external sensations (aüssere Empfindungen), and the power of the nerves to excite them is termed the sensational power (empfindende Kraft), or simply *sensibility* (Empfindlichkeit).

Unzer explains that the word Empfindung has three meanings. First it designates the involuntary perceptions to which impressions made through the senses give rise; these are external sensations (äussere Empfindungen). Secondly, the consciousness which the soul has of its own existence (Bewustseyn); this class of perceptions he designates internal sensations (innerliche Empfindungen). Thirdly, the verb Empfinden—to feel, is used indefinitely to express the perception of the existing condition, whether excited by an external impression or not. Impressions, then, *with* consciousness are sensations, or sensational impressions; those belong to the psychical actions. Impressions *without* consciousness, are *homæsthetic* impressions, or simply impressions. The nerves appropriate to the unconscious or reflex acts are *homæsthetic;* those connected with the mental acts are *sensible*.

Unzer next notices certain phenomena connected with the nerves, as for example, the power which we possess of referring sensations to the point of contact of the impression, and the circumstances that when a nerve is touched in any part of its course, we refer the point of sensation

to the extremity of the affected nerve. He illustrates this by the sensations experienced as if in amputated limbs. He also maintains that the nervous twigs are isolated throughout their whole course, and that each have a point in the brain in which the external impressions they convey to it develop "material ideas." He notices the difference in our sensations, as of warmth, cold, hard, soft, dry, moist, &c., and the differences in painful sensations, as, for example, a blow so severe as to fracture a limb, or a slashing of the flesh gives less pain than a caustic or burning application; but he sees no means by which these differences can be explained.

The circumstances that determine the transmission or non-transmission of external impressions to the mind are specially noticed by Unzer; and he deduces from them the general functions of the brain and nerves, and sympathetic ganglia. He observes specially, that many external impressions are made on the nerves which excite movements, but which never arrive at the mind. Thus, the mind does not perceive the sensation of things sweet, bitter, &c. received into the stomach, but the result of anything sweet or bitter which is irritating, is to excite vomiting. So the blood streaming into the heart, excites as the originator of an external impression on the nerves of the heart, the movement proper to that organ, but the mind *perceives* no impression. That the heart's action is independent of the mind is proved by the fact, that its movements may be excited when removed from the body. The seat of these motive powers he places in the ganglia, and he looks upon these as natural hinderances or obstacles to the transmission of impressions to the brain. Unzer also reviews other causes, which will prevent the production of external sensation. First, continuity of the nerve is necessary; then continuity of transmission; then a proper condition of the brain for the reception of the impression and the formation of material ideas. One hinderance to these is sleep, and another the habitual reception of external sensations. He enumerates five modes in which the habit of sensations render their resulting action weaker. On the other hand, the external impressions may be transmitted to the mind with greater facility, or may induce more powerful results, or the contrary; and this condition is termed being sensible, or insensible, and is the foundation of the doctrine of idiosyncrasies and temperaments.

After a short notice of the senses, Unzer proceeds to consider the sensational conceptions (sinnliche Vorstellungen.) No animal thinks without sensation (ohne zu empfinden.) But the conceptions of the mind may be of various kinds. They may be dependent on the material ideas excited in the brain by external impressions, or on material ideas excited by the mind, independently of external impressions. He adds, however, that upon a more careful consideration it will be found that the most "arbitrary" or "spontaneous conceptions" are dependent upon external impressions; but the connexion is so remote in some, and so immediate in others, that a discrimination is necessary. When spontaneous conceptions arise (which are none other than imperfect external sensations,) in so far as they belong to former external sensations, they are Einbildungen, imaginations, ideas of memory, and in so far as they belong to the future are Vorherſehungen, foreseeings—anticipations.

Unzer proceeds to illustrate these and other views (which we omit to notice), as to the nature and formation of simple notions or conceptions,

and abstract ideas by a reference to examples. Thus, dreams are the Einbildungen—the sensational conceptions of the sleeping person. In sleep-walking, and in insanity, these become so vivid as not to differ from those excited and developed naturally. This view of the identity of the psychical states of dreaming and insanity, was stated to be the principal object of M. Moreau's work, in our review of the book (vide p. 220 of this Volume). When a conception is reiterated or renewed, and the mind recognises it as one that has occurred before, that is memory.

Unzer thus theorizes on the origin of a set of ideas opposed to those of memory—ideas regarding the future.

"The sensational foreseeings (Vorhersehungen) and expectations (Erwartungen) arise out of existing external sensations, and sensations of memory, each having a part in common with the other. The mind in these considers that as belonging to the future, wherein both are different, or *foresees;* and when it considers this as the same with that which is felt as actually coming, *it expects* (erwartet). Simply sensational anticipations are forebodings—ahndungen." (p. 85.)

All these conceptions have their corresponding "material ideas" in the brain, and are subject to the general laws regulating simple conceptions and their material ideas. They are often developed in dreams and in insanity. *Wahrsager*, or Seers, are persons who have the faculty of anticipating or expecting the future. The understanding, acts of attention and of abstraction, &c., are all explained by Unzer in accordance with the preceding doctrines.

Nature of desire and aversion. Passing from the faculties of the mind, Unzer takes up the question of pleasure and pain. He first observes, that we are conscious of a difference in our conceptions, sensations, &c.: many we designate as pleasurable, many as painful. These he argues are essentially different in their action on the brain. If at any time a painful conception becomes pleasurable by being placed in relation with another conception, it becomes by that act a new conception. He further maintains, that there is the same difference in our perception of external sensations, and that they have a different action on the brain according as they are pleasurable or painful. When the mind foresees (vorhersieht) a pleasant thing, or what is the same thing, anticipates the conception of a thing which is pleasing, it exerts its power—it strains itself—to produce or obtain the subject of this anticipated agreeable conception, or to make it present, or to exist; that is, to experience it or feel it (empfinden) in the third sense or meaning of the word (vide ante, p. 188), or to make the anticipation a reality, or if the anticipation be displeasing to produce the contrary condition. In the one case, the condition or effort of the conceiving force is DESIRE, in the other AVERSION. The satisfaction of these impulses, propensities, or anticipations, requires the development of external sensations excited by external impressions. Since pleasure and pain are the moving causes of the efforts, they are consequently the grounds of the desires and aversions; thus considered, they may be termed the exciting motives, momenta—springs—(Triebfedern) of the mind (Gemüths). The impressions of pleasure or pain excite an anticipating sensation, and in so far an effort of the cerebral power sufficiently powerful to induce the material ideas of this future sensation, and this is the "material expression of the desires and aversions in the brain." He who desires,

foresees a future pleasant thing or conception, and endeavours to realize it so that it may be actually felt. In each desire three things are to be distinguished: 1. A foreseeing and expecting of a future sensation. 2. The exciting motives of the mind, delight and pleasure; these communicate the impression of pleasure to the "imperfect material sensation" in the brain, excited by the preceding. 3. The spontaneous effort of the mind to realize the entire anticipated sensation; this is connected with the exertion of the cerebral powers to complete the "imperfect material sensation" which the soul foresees. The action of the desires on the animal economy is in proportion to the strength of these phenomena. Aversion is entirely analogous to the preceding, except that the efforts (in the third proposition) are directed to attain the converse of the anticipated unpleasantness. The conceptions which excite pleasure and pain may be either sensational or intellectual; that is to say, may be distinctly connected with the sensations or with the understanding. The former, which excite desires and aversions, are to be carefully distinguished from mere impressions, or impressions without consciousness—the intellectual excite "desires or aversions of the will."

Nature of instinct and its modifications. A strong and strictly sensational desire which arises from obscure sensational stimuli, and the material ideas of which are little developed in the brain, is termed a 'blind impulse,' or synonymously 'instinct,' 'sympathy,' 'sensual propensity,' 'sensual inclination,' generally 'natural instinct,' and an abhorrence arising in like manner, is termed an 'aversion,' 'antipathy,' 'sensual disinclination,' 'enmity.' Both classes of instincts are termed animal, and divided into self-preservation, self-defence, the continuance of the species, and love of offspring. Stronger desires and aversions arising out of complex sensational stimuli, of which the person is conscious, and of which the material ideas are much more developed in the brain than those of the sensational instincts, are termed passions, affections, emotions. Both these are subject to the same laws as the simple desires and aversions, and are divided in their operation into the same three stages. The conceptions (Vorstellungen) which are required for the excitement of the desires and aversions, when they excite the conceiving force, or acts of mind, are either sensational or intellectual. The former belong to the instincts and passions; the latter are termed, motives to action (Bewegungsgründe), and excite "desires or aversions of the will." These psychical desires and aversions are subject to the same laws, and act in like manner as those of the instincts. The material ideas of the anticipations and expectations they excite, arouse the cerebral power to efforts directed to the object of realizing the perfect material ideas of the anticipations, or counteracting the aversions. We pass over the remaining purely metaphysical portion of this part.

Action of material ideas on the brain. Reference has been continually made to material ideas. Unzer next proposes to consider the operations of these in the proper animal or biotic machinery, the brain and nerves. All occur either through the one or the other, or they set simply mechanical, as opposed to biotic, machinery into motion. Unzer confines the consideration of them to the two classes which act through the brain and nerves. All material ideas are movements (Bewegungen) in the brain, consequently their action can be no other than to excite movements. They are biotic forces. It is the function of the cerebral power to pro-

duce material ideas for the conceptions of the mind. Farther, the immediate results of the operation of the material ideas that act on the brain only and do not extend their influence to the nerves, or to the mechanical machines, are simply other material ideas, which give rise to other conceptions, and consequently are developed at other points of the brain distinct from the first. Thus complex ideas are produced.

Action of material ideas on the nerves. These material ideas are next considered in their action through the nerves. And first, the impression they make on the brain is carefully distinguished from the impression from without, made by and through the nerves. To distinguish the former class of impressions from the latter, he terms them, inner sentient impressions, "innere sinnliche Eindrücke," while the other are external or outer, as already explained. We shall omit the term sinnliche, in characterizing the inner as well as the outer impressions. The inner impressions are propagated from within outwards, while those received by the nerves are propagated in a contrary direction.

Two sets of fibrils in each nerve, and their function. In prosecuting the consideration of this part of the subject, Unzer takes occasion to show that each nerve must run an isolated course from the periphery to its termination in the brain, and vice versâ, and that the impressions of material ideas, or inner impressions, must act upon the point of the nerve at its junction with the brain. This transmission of impressions in the two directions is, he argues, to be attributed to two sets of fibrils in each nerve, just as there are two sets of blood-vessels in connexion with the central organ of the circulation. It is remarkable that his Göttingen reviewer (Haller?) notices this doctrine of the existence of two kinds of nerves in the same trunk, to oppose it. "Further on Unzer considers it probable that there is a difference between the nerves proceeding *to* the brain and *from* it, and thinks the arguments to the contrary invalid. He forgets that the proof rests with himself, and that neither experiments nor anatomy favour his hypothesis." To this Unzer replies by repeating his doctrine of two classes of fibrils in one trunk, and observes, that it is not necessary to his views; nevertheless, certain phenomena can only be explained by the hypothesis; and, therefore, although he cannot prove it to be a fact, it may still be useful as a hypothesis. In fact, his fundamental doctrine is, that there is a reflexion of the impression from the cerebral end of the sensory nerve to the cerebral end of the motor, occurring in the brain itself. (Answer to Rev., p. 25.) Unzer also reviews properties of the nerves now well known and thought new, as for example, the reception of impressions made along the trunk, and the reaction or radiation of inner and outer impressions on the central ends of the nerves; first, in exciting the functions of other nerves, inducing *sympathetic actions*; and, secondly, by frequent repetition, developing the phenomena of *skilled actions*, or what have been usually denominated *habits*, or habitual motions. The influence of attention, or anticipation and expectation is discussed; and it is shown that an influence is sent along the nerves from the brain during this act. He mentions as an example, amongst others, that the papillæ of the tongue in which the nerves are distributed, are excited with the expectation of sugar, salts, &c. The action of attention on the terminal fibrils in the brain may also develop impressions analogous to those made on the periphery of the

nerve, and thus imperfect external impressions are formed. These are known only by the abnormal conception excited by them, and which are spectral illusions, false sensations, as of touch, hearing, and smelling, and vertigo. It is impossible not to be struck by the lucid views detailed by Unzer under this head, nor to incline to the opinion that the transference of an influence from the brain outwards is not confined to the motor nerves, but that the sensory or sympathetic constitute a channel of communication in the same direction. If the hand of a nervous female be pointed at she complains of a sensation of coldness; what is this but an "external sentient impression," excited by the action of attention on the cerebral termination of the nerve of touch, with probably a real change in the peripheral termination? That some such influence is sent to the nerves of secretion, as of the salivary and lachrymal glands, kidneys, &c. is certain, and renders the preceding supposition more than probable, and that Unzer's view is the correct one.

Operations of the mental and cerebral powers on the mechanism or machines of animal bodies. The *mechanical* machines of animal bodies are, in virtue of their structure, capable of many movements; but they are much oftener excited into action by vital, than by simply physical or mechanical forces. The influence of the soul on the body takes place through the material ideas in the brain, or through the sensational impressions on the brain of their corresponding conceptions; movements of the mechanical machines operated through these media are psychical actions (Seelenwirkungen.) They are effected either mediately through the nerves, or immediately by the material ideas. Now these mechanical machines are the muscles, tendons, membranes, vessels, glands, bones, cartilages, viscera, &c., and not the brain or nerves, which are the proper *animal* machines. The former possess animal forces it is true, but in virtue of the nerves by which they are supplied, and not otherwise.

Unzer then takes a review of the machines of the body, beginning with the brain. The cortical substance, he observes, is not the seat of the psychical forces, nor the instrument of the mind, but only a tissue of blood-vessels and canals—a secreting apparatus—in which the vital spirits are elaborated from the blood; from thence they are communicated to the medullary substance, and through the latter distributed to the whole body; so that the cortical substance is in fact a secreting viscus situate within the cranium. The medullary substance is the seat of the material ideas, but he argues that the latter have a close connexion with the cortical substance, and the secretion from the numerous blood-vessels it contains, inasmuch as so large a quantity of blood is sent to the brain for the maintenance of its functions. Further, that material ideas may have an influence upon the cerebral capillaries, is rendered probable by the fact of their direct and immediate action upon the heart itself and the capillaries distributed elsewhere. It may certainly be stated that mental acts do often induce a disturbance of the circulation within the cranium, but this is inconclusive as to the *direct* action of material ideas situate in the medullary matter upon the capillaries of the cortical substance, because it *may* be induced indirectly through the action of the nerves on the central force-pump, the heart. So that Unzer says the opinion is probable but not certain; nor can it be demonstrated.

With regard to other machines, Unzer observes, that the nerves are in the

strictest connexion with them, inasmuch as they form loops at their ultimate termination within the intimate tissues of the machines, as is seen in muscles, and around the blood-vessels. The physiology of muscular action is then set forth, partly by extracts from Haller, and partly in his own words and style. He points out the well-known laws of muscular actions very distinctly, and their relation to the nerves and brain. Material ideas make impressions on the origin of the motor nerves in the brain, and thus excite corresponding muscular actions. If a muscle be immediately excited to action by external impressions, the nerve which receives them excites the action; of course the anatomical distinctness of motor and sensory fibres in the same trunk was not known at the time he wrote, but we have seen that he maintained such distinction as a theory. Perhaps it is also worthy of notice that Unzer considered the biotic powers of the two classes of nerves identical in their nature, and that the difference in their action is dependent upon the difference of the apparatus in which the impression on the nerve finally terminated and excited changes. An impression on a motor nerve or on its trunk is propagated to the machines of movement—namely muscles; an impression on a sensory nerve is transmitted to the seat of the biotic forces and the "machine" of thought, namely, the cerebro-spinal axis; and there develops material ideas and sensations, and is thence reflected as an inner impression on the motor nerve. He adds, action may be transferred to other and far separated roots of nerves, and this is sympathy, and the actions are sympathetic. This kind of communicated action is not, he observes, at all unusual, and is seen when a painful injury that excites spasms in one set of muscles extends its influence to another set. With regard to the action of the material ideas, whether of conceptions of the understanding, or of the instinct, he observes that the muscular action is forcible in proportion to the force of the impressions made on the origin of the motor nerves by the material ideas. The frequent repetition of these impressions determines the form and size of muscles, so that those of the face are at last so formed as to express the thoughts, and a knowledge of their meaning is the science of physiognomy.

The vascular and secreting systems are subject to nervous action, but specially the heart. It has nerves fitted to receive external impressions, to transmit these to the brain, to develop there external sensations or homæsthetic impressions, which, provided no material hinderance occur, are reflected along the same nerves, and thus not only exert a biotic influence, but develop psychical actions. Thus also sensational conceptions, desires, and aversions will act on the heart. Nevertheless, the heart will act even out of the body and separate from it.

Action of external impressions through the nerves, or reflex acts. We need not follow our author through his review of the action of material ideas on the other hollow muscles, as the pharynx, œsophagus, intestinal canal, bladder, &c., as it is in all respects analogous to the preceding. We therefore pass on to an interesting chapter of Unzer's work—a chapter, indeed, which may be considered as containing the first elements and original enunciation of the doctrines of reflex action. After pointing out the distinction between acts resulting from impressions only, and those which are true psychical actions derived from the brain, Unzer observes:

"These two really distinct things have always hitherto been considered as immediate psychical actions resulting from external sensations, and thereby the physiological doctrines regarding external sensations have been shockingly confused (greulich wirwirret). It is desirable that we make this important distinction as clearly as possible here, whereby, however, we must presuppose that which in another part of this work *we are the first to demonstrate*, namely, that the external homæsthetic impression on the nerves (the external nerve-feeling) really possesses in itself a biotic moving force before it comes to the brain, to excite these external sensations. The clearest proof that the motion excited in mechanical machines by the touch of the end of a nerve is simply an action resulting from the external homæsthetic impression (feeling), or the external sensation, is to be found in the fact, that the same contact of the nerve which excites the movement may be repeated after its connexion with the brain is broken, or for greater certainty against sympathetic acts, after the whole head is severed from the trunk. The result is, that so long after decapitation, as traces of animal life continue, there are the same movements from a touch of the nerves, although the external impression can no longer be propagated to the brain and excite a material sensation; consequently, it is not the psychical action through an external sensation which this external impression otherwise excites through the brain in the soul, but an animal [biotic] action proper to the peculiar biotic moving force of the external impression. If this experiment respecting the apparent psychical action of external sensations on mechanical machines be investigated, it is found that the biotic motive force of an external impression develops the majority of those movements in the mechanical machines which we have considered to be solely the psychical action of its external sensation, &c." (pp. 179-80.)

Unzer terms these mere mechanical movements dependent upon impressions on the nerves "Nerve-actions," (Nerven-wirkungen, actiones nervorum) to distinguish them from the "soul-actions" (Seelen-wirkungen, actiones animæ). As the majority of the latter in the mechanical apparatus are really the former, he observes that it is wrong to state, that the one cannot be the other. Thus the action of the heart is ordinarily a "nerve-action" or reflex act, for we are not conscious of the external impression which excites that animal apparatus.

If an external impression be not felt, and nerve-actions or reflex acts follow, the action itself may be felt, or become an external impression giving rise to other actions; these Unzer says are also nerve-actions.* It may not be easy, he observes, to say what use sensation is of in nerve-actions, but the result seems to be, that when they are psychical as well as reflex, they are more regular, perfect, and complete. If we were not made sensible of spasm or cramp in the muscles by pain, the "machines" might thereby suffer injury and we not exert ourselves to prevent it, &c.

Looking at the psychical actions in the mechanical apparatus from another point of view, Unzer cautions his reader against confounding the psychical actions resulting from conceptions, and those resulting immediately from external sensations. Thus, if a certain food taken into the stomach has caused us at some past period to vomit, and on seeing or smelling it we vomit again, Unzer argues that it is not the sight or smell which excites the vomiting, but that there exists an idea previously formed (Einbildung), and this being re-excited, acts on the origin of the

* We must request the reader's indulgence for the use of these truly uncouth terms and Germanisms, inasmuch as we cannot substitute others of a more elegant construction without injury to the sense of the author, or without coining new words.

motor nerves, and causes vomiting. Again, if we see a stone falling upon us, we get out of the way; but this is no direct psychical action of the muscles excited by the external sensation of the stone, but a fear of impending danger which the appearance of the stone excited.

Physiological nature and operation of pleasure and pain. Unzer next considers the nature and action of pleasure and pain, and lays it down as a fundamental proposition, that all external sensations are either agreeable or disagreeable, and that if in greater strength they cause "Kitzel" and "Schmerz," or *tickling* and *pain*. He thinks that we cannot possibly know in what consists the difference between the impressions causing them; we can only judge by the phenomena. He adds, however, that it appears that that external impression or sensation is agreeable, which is conformable to the natural structure of the nerve, and on the contrary, that that is disagreeable which does not act conformably to the animal structure and force of the nerve, but rather in some degree does violence to it. Further, all movements, of which a mechanical apparatus is capable in virtue of its structure, are either conformable to the natural destination or functions of the apparatus in the condition of health, or they diverge from this conformity. All psychical actions arising from external sensations, stimulate the mechanical machines to which they extend, either to movements conformable to their destination, in which case the external sensations are agreeable, or to the converse, when they are disagreeable. Pain is curative—is the sentinel of the organism,—and excites the movements proper for the removal of its cause; namely, movements opposed to the natural and pleasurable movements. In the hollow muscles, for example, as the gullet, an ordinary external sensation excites the natural propulsive movement, a powerful sensation, or pain, induces a violent morbid movement or spasm. In like manner, pleasing or unpleasing sensations, or injurious or beneficial impressions act upon the vessels, and cause an effusion or efflux of fluid, and so on through the various mechanical machines of the body, as the pharynx, œsophagus, and alimentary canal; the skin and mucous surfaces, (quoting sneezing, excited by irritation of the mucous membrane of the nares, as an example;) the glands and glandular organs; the organs of the senses and the sexual organs. With reference to all the excited phenomena manifested continually in these "machines," Unzer observes, that the law of external sensations which Krüger has laid down in his physiology holds good, namely, that the movement which results from external sensation is proportionate in extent to the force of the sensation, provided there be no natural hinderance, and the condition be that of health. But movements may follow on external impressions as simply nerve-actions, and which impressions are not objects of consciousness. But the law of proportional reaction and action holds good in these impressions also, as well as in sensations; the force, however, of the impression must not be estimated by physical laws, for a slight touch of a nerve may be and often is a forcible external impression, and vice versâ.

But besides these psychical actions resulting from external sensations, there is a class incidental thereto, which must be taken into consideration. For example, we connect with an external sensation the conception of another resembling it, which we have experienced before, and so there is an idea or image (Einbildung) in immediate connexion with the former,

that commingles its psychical action in the mechanical machines with that proceeding directly from the external sensation. Thus we sigh if we see a person resembling another with whom we have experienced sorrowful feeling; the sigh is the psychical action of an "Einbildung," and only mediately of an external sensation. Further, we connect expectations or "Erwartungen" with our external sensations—expectations of others formerly connected with them, and there arises an anticipation (Vorhersehung) which commingles its influence in the mechanical machines with those connected immediately with external sensations. A certain person is accustomed to faint when bled; meeting the surgeon in the street he faints again. This faintness Unzer describes as a psychical action resulting from a "Vorhersehung" of a venesection, and only incidental to the external sensation of the surgeon.

Since all our external sensations are accompanied with pleasure or pain, and are associated with "Einbildungen," or inscribed ideas, and "Erwartungen," or expectations, so they consort with desires and aversions, and the psychical influence of these operate on the mechanical machines conjointly with that proceeding directly from external sensations. Thus the sight of food excites the sensation of hunger in a fasting person; peculiar tones or odours excite the sexual instinct, &c. On the other hand, strong sensational conceptions excite imperfect external sensations. Thus when we see a figure in the dark and take it for a ghost, and by this imperfect external sensation are affected with terror, and attempt to grasp it or repel it, the arm becomes spasmodically fixed, not directly by the true external sensation of the shadow or form, but incidentally by the imperfect external sensation caused by the appearance of the thing we take to be a ghost. If we reflect upon our external sensations, then conclusions of the understanding commingle in action with the former.

We pass over very reluctantly the physiology of mental action in its healthy and morbid states as detailed by Unzer. We can only remark that his explanations of the phenomena of dreaming, insanity, somnambulism, &c., and of the excitement of functional action by conceptions of the mind, whether of imagination, memory, anticipation, reasoning, &c., are quite as ingenious and plausible as any we have read, and what is curious we find that his illustrations are almost identical with those used by more modern neurologists.

Action and nature of the desires and aversions. A good deal of space is devoted to a consideration of the actions of the sensational desires and aversions through the nerves. The immediate psychical action of these is an effort of the cerebral power to produce the material idea of a foreseen sensation, if pleasurable, or its contrary, if painful; and it is the impression of pleasure or pain in the brain by the foreseen sensation which causes the effort. The psychical action of pleasant instincts and actions (joy) take place in harmony with, and in fitness or adaptation to, the animal machines; or if the impression be painful (sorrow), they are opposed to the natural function of the latter. Consequently, all pleasing instincts and passions are beneficial, or contribute to healthy action, and the painful the contrary. This proposition is of fundamental importance in understanding Unzer's views as to the instincts, propensities, and passions. Without defining pleasure or pain, he steadily adheres to the doctrine that those ideas and acts are pleasurable, which arise from

impressions having a beneficial influence or beneficial results; and on the contrary, those ideas and actions are painful which are induced by impressions injurious to the organism, and opposed to the natural and healthy function of the part affected. Subsequently, he shows that the *feeling* of pleasure or pain is not requisite for the necessary actions in many animals, but that the law still holds good as to their excitement by the two kinds of external impressions, namely, the beneficial and the injurious. In illustrating the mechanism in the excitement of the instincts, he observes, that instinct for food, the satisfaction of which is the taking of food, saliva is poured forth: in the instinct to suckle, the satisfaction or contentment of which is the pouring out of milk through the nipple, by the act of sucking, there is an efflux of milk and an erection of the nipple. In infants the instinct to suck is exhibited in the action of the lips, which suck even at the air. So also ferocious animals, on the excitement of their instinct (rage), prepare their weapons of offence; in terror, the contentment of which is the escape from a great impending danger, the efforts are in accordance.

Nature and operation of the sensational instincts. Unzer's arrangement of the instincts is this: 1. The instinct of self-preservation which, consisting of strong desires, arising out of obscure (dunkeln) sensational impulses, and the object of which is the preservation (Erhaltung) and well-being of the animal. 2. Self-defence, which consists of strong aversions arising out of obscure sensational impulses, and the object of which is to avert death, or remove an unfavorable condition. 3. The propagation of the species, consisting of strong desires, and having the object of propagation of the species by union of the sexes. 4. The parental instinct, consisting of strong desires and aversions, and having for its object the maintenance, well-being, and defence of the offspring. In considering these in detail, Unzer classes the first two together, and breaks them up into minor or secondary instincts, such as love of life, instinct for food, or of hunger, thirst;—of dislike for certain food, or nausea; the instinct of motion of voluntary muscles to perform specific muscular acts, as singing, whining, leaping, &c.: the instinct for rest, the war-instinct (Wehr-trieb), instinct of destruction. We subjoin an account of the instinct of motion of voluntary muscles, as being novel, and also as illustrating the mode in which Unzer treats the subject.

"Nature has constituted bodily exercise of animals, a means of their maintenance and well-being; consequently, they are thereby in the healthiest state. If, therefore, to their disadvantage they neglect bodily exercise, a number of unpleasant external sensations arise, which we term indisposition, or sickness, and which all animals detest. This is the sensational stimulus of the *instinct of bodily exercise*, a stimulus which causes the vital actions to be more or less morbid according to its intensity, the pulse becoming feverish, and the breathing embarrassed. By this unpleasant sensational stimulus (which too much repose, or sleep, or nourishing diet, and many other causes may occasion) is the animal motived to strive for the opposite agreeable sensations, which, as it foresees (vorhersieht), must be attained by the movement of its muscles and limbs, and in this instinct, this is the object and peculiar design of nature, although it is ignorant of it [the object], and knows not for what ulterior purpose it must move its limbs. The psychical action of this sensational stimulus, insomuch as they constitute a foreseeing (Vorhersiehung) of bodily exercise, are displayed in the muscles of voluntary motion, that is, in those which an inner sensational impression from conceptions can move without the concurrence of another sensational im-

pression being necessary; thereby they stimulate the same muscles to the movements which they accomplished perfectly during bodily exercise, and in the striving of the psychical forces after these same imperfect movements consist the psychical actions of the instinct to bodily exercise itself. This instinct to bodily exercise operates therefore in the machines which it formerly affected, namely, the muscles of voluntary motion of the limbs, whilst it impels the same to the natural actions of which they are specially capable, and which in particular, produces fully in them the satisfaction of the instinct, namely, bodily exercise. Consequently, in this instinct it is seen that the muscles jerk the limb, and the animal often seeks to hop, spring, soar, raise itself up, &c., in such manner as may happen perfectly by actual contentment of the impulse to bodily exercise, and if no extraneous hinderance occurs, commonly happens instantaneously, while the conception is only sufficient to move the voluntary muscles. In such cases, there follows through the connexion of physical, mechanical, and animal forces of the muscles set apart for voluntary motion, the further actions of the animal economy, namely, the changes in the composition of the fluids, the promotion of the secretions, the increase of muscular power, and of the sensational forces themselves, so far as they are in accordance with the objects of nature, which are treated of in detail in the physiology of the proper mechanism of animal bodies. (See Haller's Physiology, Second Discourse.)" (p. 268-70.)

Taking these views as a basis, Unzer proceeds to explain the various movements of animals, as the singing of birds, the cries of animals, laughter in man, the periodical migration of animals, the cleaning, pluming, &c. of birds and quadrupeds, the domestic architecture, the laws and practices of republican animals, &c. All these are allied to the instinct of voluntary movement, have certain obscure pleasing or displeasing external sensations and beneficial or injurious impressions, as a basis, by which the animal is motived to perform certain movements without knowing their object, but which, according to the plans of nature, through the assistance of external impressions, are really in the position to accomplish adaptively the maintenance and well-being of the animal. After discussing the respiratory instinct and showing its double character as a voluntary act, and a reflex act or nerve-action, Unzer discusses the instinct to repose and stimulation (zur Ruhe und Ermunterung); yawning, extending, stretching, are instinctive movements belonging to the latter, and not to the former. He then reviews other instincts, as the instinct of attack and defence (Wehrtrieb), of reproduction, of suckling—a branch of alimentative instinct, &c.; and next shows that there are general instincts, as the love of life, of pleasure, of food, of the propagation of the species; and special instincts proper only to certain animals, as respiration, care of the young, brooding, &c. Instincts may be unnatural or perverted, as self-destruction, sodomy, &c.; they may be also general or principal, and give the character to an animal, or a race of animals. The principal instinct is that which is most necessary for the well-being of the animal, as the Wehrtrieb or destructive instinct in carnivorous animals.

Nature and action of the emotions and passions. The affections, or passions, are analysed by Unzer, and he shows, that they differ from the instincts in this, that in the latter the animal is not necessarily conscious of sensorial stimuli, whereas in the former it is. Every animal is by nature impelled by a love of life, without knowing why. Nature has given them the means of protecting and maintaining their life without their knowing it, or its object, and they use those means unconsciously;

but when they become conscious of the danger of death, they then experience the emotion termed fear of death (Furcht des Todes), and through this, other conceptions, desires, impulses, and emotions, are excited, all which combine for the preservation of life; so the *appetite* for food is distinguished from the *instinct* for food, and gives rise to other passions, as voraciousness and rapaciousness. So the *instinct* of attack and defence gives rise to the *emotion* of anger or revenge; the sexual instinct thus gives origin to the passion of love, and this to other instincts and emotions as combativeness, jealousy, &c. An agreeable emotion is joy, and Unzer draws the distinction between joyousness, contentment, or satisfaction, and hope. Sorrow, or unhappiness, on the contrary, is the emotion or feeling consequent on a disagreeable passion, and comprises the painful or injurious emotions. The whole of these are gone through *seriatim*, and their action on the mechanical machines pointed out. Then the animal dynamics of the understanding and of the will are discussed, and Unzer makes a remarkable distinction between movements which are purely spontaneous (freywillig), and those which are simply volitional (willkürlich) or dependent on sensations as well as on the will. This distinction is requisite to understand some of his views, as for example those regarding the instinct to voluntary movement. "There are," he observes "innumerable sentient volitional (willkürlich) movements which are not spontaneous (*freywillig*);" meaning apparently, that the movements are excited *in* the muscles of volition but not *by* a volition. A concluding chapter is devoted to the communion (gemeinschaft) of the body and the soul. In this short chapter, he separates the principles already expounded in the previous chapters, arranging them consecutively and referring each to the paragraph where it may be found evolved. The whole is too abstract for our pages, but is a singular example of lucid, profound, and logical thought. He adopts a medium course between the Stahlians and the mechanical physicians and materialists.

II. LAWS OF REFLEX ACTION. The second part of the book is devoted to a consideration of the nerve-actions, *actiones nervorum*, or in other words, of reflex phenomena.

"Animal bodies become capable of movements by means of the forces of the nerves, which can neither be explained by the physical and mechanical laws of motion, nor deduced from the laws of the cerebral power, but are developed by the vital spirits, supplied to the animal machines, according to their own peculiar laws. To these belong those simply animal motions excited by external impressions on the nerves before they cause a material external sensation in the brain, with which the muscular irritability of Haller must be classed; and also those excited either by inner impressions on the medulla of the animal machines which develop no mental conceptions, or by other stimuli independently of ideas." (p. 343.)

Action of centric and peripheral impressions and of the vis nervorum. The introduction to the first chapter is entitled, "Of the forces of the nerves, and of the nerve-actions in particular." There are only two forces which act on the animal and mechanical machines, namely, the two kinds of impressions on the nerves. The external impression supplies to the mind all its external sensations but on this condition, that it reaches the brain, and excites therein material external sensations: and

the inner impression produces all its psychical actions in the body under the condition that it be excited by conceptions. But should these conditions fail,—should the external impression not be propagated to the brain, or at least excite no external sensations therein; and should an inner impression on the origin of the nerves in the brain be not originated by conceptions, but by some other cause, are we then to conclude, asks Unzer, that the two kinds of impressions develop no animal action in the body?

"If a nerve, accustomed to be excited to the production of certain animal movements in the body, by certain external impressions that are perceived, be cut off from all connexion with the brain—the seat of the mind,—as for example, either by being tied, divided, or even by the entire separation of the head from the body; nevertheless (as we are taught by indisputable facts) so long as traces of animal life are left in the separated portion, the same external impressions from the point of impression to the point of division are the stimuli of the same animal movements, and actually develop them, although in such cases the external impression is never propagated to the brain, but reaches only to the point of division [of the head from the body], and consequently, no material external sensation can be excited in the brain, nor be perceived by the mind. This is the first of the fundamental principles upon which all depends, that will be propounded in this division of the physiology of animal nature." (p. 346.)

Unzer then proceeds to mention in rapid detail the experiments, which show the action of the vis nervorum or nerve-forces, in developing this class of movements. He mentions the contraction of a separated muscle, of a portion of intestine, of the heart; the secretion of the glands and the movements of viscera excited by a stimulus when separated from the body; the sexual actions excited in decapitated crickets when the organs of generation are irritated; the full performance of the sexual act by decapitated butterflies on the application of a proper stimulus, so that ova are deposited; and the retraction of the foot by a decapitated frog when pinched or injured. For an abundance of such facts, Unzer refers to Haller, Zimmermann, and Geder. He then in like manner shows, that inner impressions made on the nerves in any part of their course as far as their origin in the brain, are propagated to their peripheral extremities, and there excite animal actions just the same as if they had been excited by conceptions. The Italics in the subjoined extract are the author's.

"From these two fundamental principles, is deduced the irrefragable truth in the physiology of animal nature, that the animal machines are not destined by nature solely for the propagation of external impressions to the brain, to excite there the material ideas of sensations; nor destined to receive all their inner impressions from conceptions only: *but that they possess in addition another faculty altogether distinct, and one destined by nature for the production of those animal movements in the body by means of external impressions, which may be propagated to the brain, and be perceived, or which may not;* movements which are produced by the cerebral power when the impressions arrive there and are perceived; and further; inner impressions which they [the animal machines, or nervous system] derive from a touch or a stimulus, but which excite no act of the mind (Vorstellung), induce the same animal movements in the body, that are produced by the cerebral power, when an inner impression, identically the same, excites an act of mind. These animal machines are endowed by the Creator with the special animal motive-forces of the impressions, in a mode to us unaccountable and mysterious, partly to put the cerebral power into action, partly through these to move the body.

In fact, the greater portion of animal movements are so very much dependent upon these, even when they are at the same time psychical actions, that *it (the vis nervorum) must be received as the most general and the first principle of life, and of the whole organism.*" (pp. 352-3.)

Analogy of the brain, spinal cord, and sympathetic ganglia. In pursuing his subject, Unzer lays down the fundamental principles of reflex action, and carefully distinguishes between a reflex action and a psychical action. He points out how the stimulus of conceptions (the internal impressions) acting on the terminating fibrils of the nerves in the brain, and producing mental acts, may be superseded by a mechanical stimulus operating on the same fibrils, and developing the same results. And contrarily, the external impression on the nerve or on its trunk may or may not excite conceptions, but in either case movements are developed which are either nerve-actions only, or these and mental actions conjointly. He also points out that an external impression may excite reflex acts on its way to the brain, and when arrived at the brain it may there excite changes and psychical actions result. The independence of the vis nervosa of the brain is demonstrated, and a remarkable feature in Unzer's views is, that he includes the sympathetic or organic system of nerves in his doctrine of reflex action, the ganglia being centres of action. We think it far from improbable that his doctrine may be hereafter fully established. It is worthy notice too, that he considers the actions of those animals which are endowed with a ganglionic system only as displaying these reflex acts only, and that the mental acts are peculiar to the higher orders.

"This is the case most probably with all those animals in which there is no brain, but only nerve-like fibrils, capable of receiving impressions; as for example, the polypes whose nerves are not united into a brain, but interwoven with many ganglia. In animals possessed of a brain, each external impression which is perceived, goes direct to the brain, and excites therein a material idea, and a conception. But when in the brain it is inverted, or reflected as it were [gleichsam reflektiret] and returns back as an internal impression of the conception to the same nerve-fibrils that move the limb, which this external impression has the power to govern through a psychical action of its sensation. At the moment of this reflection when the external impression is changed into an internal, the act of thought takes place, and thus the movement which the external impression excites in the mechanical machines becomes a psychical action. When the polype receives external impressions, they pass directly forwards as far as the first ganglion, and here are altogether reflected as in a brain, or if only partially reflected then they partly reach to other ganglia, and so may be reflected many times. It is enough that at these points (the ganglia) the external are changed into internal impressions, and are propagated from the ganglia outwards to the mechanical machines which they set in motion without it happening, that during this change, a thought is excited, since there is *no brain*, for in that *only* a conceptive faculty is seated; and without the inner impression becoming a mental action, and operating as such. In such a manner, polypi may be able to perform all their animal movements, simply by external impressions on their nerves without consciousness on their part, or thought, and without either brain or mind." (pp. 360-1.)

We think this as explicit an account of the reflex acts of lower animals, as has ever been given. The existence of a special class of nerves may or may not be proved anatomically, but here it is shown that their

anatomical distinctness is unnecessary. *The anatomical and physiological distinctness is claimed for the nervous matter in which they terminate.* In succeeding paragraphs Unzer propounds several modes in which mental actions may be changed into reflex actions. The retention of the urine is an act with sensation, but if the impressions exciting it do not arrive at the brain, they still act so as to close the sphincter. Thus the external impressions of worms in the stomach are not perceived; nevertheless, they pass to the central axis, become inner impressions, are reflected upon the motor nerves of the limbs, and excite convulsions (as in epilepsy) without any act of consciousness. But just the same epileptic convulsions may be excited by fear, &c.

In his answer to his Göttingen reviewer, Unzer brings forward a remarkable extension of his views. The reviewer, criticising Unzer's theory of the origin of corporeal or material ideas, observes—

"Corporeal ideas are developed at the origin of each nerve in the brain. It is, however, worthy notice that nerves arise also from the spinal cord and its gray matter, consequently their external impressions ought to be developed in it, yet it is quite certain that the soul is not seated in the cord, nor perceives by it."

Unzer's answer is very explicit.

"It is not a change only in the origin of a nerve excited by impressions which develops sensation and thought, but the cerebral structure is ever necessary to which the nerves must reach, if a sensation is to arise. Since new twigs frequently proceed from the ganglia, all may be sensible; but if that particular kind be wanting, which develops ideas in the brain, then the change which the nerves experience in the ganglia from external stimuli *is only a moving power* (Bewegendekraft), *a reflection of impressions upon other fibrils* which are regularly determined to action. *Just so is it with the nerves which arise in the spinal cord and have there probably the double function partly to transmit their impressions to the brain that sensations may be excited, and partly to be reflected on other nerves, so that thereby certain movements are developed which otherwise would not result from these impressions.*" (Answer to Review, p. 24.)

Nor is he less explicit as to the reflex function of the sympathetic ganglia. The reviewer observing that Unzer considered the ganglia to be small brains, Unzer explains in what sense he meant this. He ascribed to the brain various functions; in one only of these do the ganglia resemble it, namely, in having a reflex function.

"When an impression is developed in a nerve from without, it goes on to the brain, and is there felt. But this impression is forthwith reflected and is continued outwards along the nerve to the muscles. Consequently the perceived impression excites movements which are necessarily accompanied by sensation, or belong to the mind. The ganglia can in like manner reflect upon other nerves the external impressions which are propagated to them, and thereby move certain muscles. Now it is in this that they have a function similar to that of the brain, although during this reflex action no sensation is felt in the ganglia, as is the case during the reflex action of the brain. This is my real meaning." (Answer to Review, p. 59.)

Unzer devotes some space to a consideration of the question, whether the brain is endowed with the vis nervosa, or nerve-force, and whether it can give rise to nerve-actions, or reflex acts, without consciousness. He decides in the affirmative from several arguments: the most conclusive is

founded on the fact, that injuries of the brain may take place without exciting sensation, and yet develop reflex acts, for a certain individual suffered from an exostosis, which penetrated into the brain; for some time there were no morbid phenomena, but at last death supervened on convulsions. The convulsions were caused, he argues, by the irritation; yet the person was not conscious of the cause nor of pain. The seat of the vis nervosa in the brain is the cortical substance, with which, he observes, that of the spinal cord is continuous, and the fact of its existence there is a fundamental fact. These facts have now no weight whatever, but it is worthy of notice that the phenomena of etherization corroborate in many instances the views lately propounded as to the occurrence of reflex cerebral action without consciousness.

Analysis of reflex phenomena. In further considering the reflex acts or "nerve-actions" Unzer cursorily notices those of the sensitive nerves, evidently thinking that a central impression is reflected to the periphery, and then the return of this to the brain excites a conception or sensation. But he dwells little on the subject, for this reason, that in general it is impossible to say *when* animals feel. The reflex-motor acts are therefore the most instructive, because they are visible. Unzer commences with the consideration of muscular irritability, which he firmly maintains to be a "nerve-action," in opposition to Haller, principally on the ground that it is impossible to separate the minute motor nerve-fibrils from the muscular, and that whenever a muscle or a portion of one is irritated nerve-fibrils are necessarily irritated too. And, as it is well known that irritation of a nerve-fibril will and can excite muscular action, there is no need for the doctrines of a vis insita. He, however, takes up Haller's arguments one by one and combats them. The contraction of a muscle from stimulation is a *direct* nerve-action; cardiac and vascular action is of this kind. That from the irritation of a nerve an *indirect* or "reflected." The extension of the doctrines of homæsthetic action to cell-structures, will give a new phase to the discussions carried on from time to time regarding the *vis insita*.

Having disposed of the muscular fibre, Unzer proceeds to show the reflex acts of the heart, arteries, alimentary canal, diaphragm, glandular structures, sexual organs, and limbs; and with reference to the latter he refers to experiments on decapitated animals, as insects, reptiles, and birds, up to man. "A decapitated man struggles for a few moments to undo his hands from the cords, to raise himself up, to stamp with his feet; a dove whose head has been struck off in flight, still runs on until it comes in contact with an obstacle: a frog leaps forward without a head," &c. The action of stimuli on the central axis, as the spinal cord of a frog, is also considered, and lucidly explained; and he shows that *certain acts excited by stimuli, may in their turn excite other reflex acts.* If the spinal cord of a decapitated frog be irritated by a needle, certain muscular acts are excited; but, he adds, "these acts now become external impressions and excite other reflex acts, and the animal raises itself up, leaps, swims, &c. according to circumstances."

The incident-excitor and reflex-motor nerves and their functions. Unzer defines the property of the nerves to receive external impressions, and excite reflex acts, and he distinguishes it from ordinary sensation or feeling

by terming it "the external feeling? (Gefühl) of the nerves." The word gefühl cannot be translated by the term *feeling*, as is manifest from the context; the word homæsthesis (as defined before) would be the better expression. He observes that if Whytt and others had carefully distinguished the two kinds, they would never have renewed the old error of the ancients, that the soul was diffused through the body. The soul is that which conceives and thinks and feels, but the nerves (apart from the brain) can neither feel nor conceive. In like manner, there is an "inner sentient impression without consciousness," or an inner homæsthetic impression, acting upon the nerves at their point of connexion with the brain onwards to the muscles; or acting upon any part of the trunk of the nerve in the same direction. Unzer points out the injurious pathological errors which have resulted from a want of proper knowledge as to the functions of the nerves and the nature of reflex actions. Convulsions, epilepsy, paralysis, and, in particular, all diseases of the nerves have been thought to arise from morbid action of the brain, and their cure has been sought by remedies which act upon the brain; whereas they very often arise from reflected external impressions, of which there is no perception in the mind, and which operate independently of the brain; consequently the cure of the diseases noted should be attempted by removing the causes of the reflex action. After a most lucid detail and explanation of the phenomena of reflex actions from external and internal impressions, as produced by the vis nervosa and its laws of action, Unzer says that, on a consideration of all the phenomena, it is impossible to explain them otherwise than by the theory, *that in each nervous trunk there are two classes of fibres, the function of the one being to transmit impressions* TO *the central axis, the other to propagate them* FROM *the central axis.* There are examples of limbs totally paralysed to the will, which, when the nerve going to the limb is irritated, become convulsed, and contrariwise, an impression made on the periphery is transmitted and reflected without consciousness, with the effect of exciting convulsive movements on the powerless limb. These two classes of nerves he subsequently distinguishes as *afferent* and *efferent* (ableitenden and aufleitenden).

Centric reflex action. After devoting a paragraph to an announcement of "*the great truth that these impressions act in animals not endowed with consciousness or mind, and are the stimuli to their instincts,*" Unzer proceeds to notice the various modes of action of the "inner sentient impression without consciousness," and the nerve-actions, or reflex acts, that result. Thus the spinal cord can receive these impressions, for if that of a decapitated frog be irritated with a needle, the animal leaps, provided the continuity of the cord and of the nerves be perfect; but if they be divided, or crushed, no nerve-action results. An inner or centric impression, caused by the reflexion of an external impression, is subject to the same law. Such an example of nerve-action occurs, he says, when an external impression, caused by the external homæsthetic irritation of pinching a decapitated frog's foot, excites it to leap. "The external sensorial impression in this case is propagated uninterruptedly from the point touched in the toe to the point of injury in the spinal cord, and from this is reflected, and returns again uninterruptedly, and so the same nerve-actions are developed, as if another primary *internal* impres-

sion without consciousness had excited the spinal cord." Farther, the internal or centric impressions without consciousness are shown to act in the same way as conceptions themselves upon the origin of the nerves, and through the nerves upon the mechanical machines. Examples of centric reflex action illustrate this law, derived from the actions of the muscular and visceral systems. Thus, the repression of hemorrhage from the nose, by the application of cold to the neck or other part of the surface, is a reflex action upon the arterial capillaries. The act of respiration in the animals endowed with consciousness is partly voluntary, partly excited; but in the newly-born, in whom the brain is imperfect, and the mental faculty not developed, it is altogether an excited act. Vomiting from injury of the brain is a centric excited act; the injury acting as an internal or centric impression. The action of the lungs, and specially of the bronchi, is an excited act, as also is erection of the penis, and the emission of semen in epilepsy.

Centric diffusion of impressions. External impressions may be diffused, so as to induce convulsive action in muscles in other parts of the body of a decapitated animal than that in which the impressions are made. The bodies of animals, he says, are so constructed, that the "external sentient impressions with conceptions" may in their course to the brain be reflected upon other nerves and nervous twigs, and this also occurs with "impressions without conceptions," and the same movements result with them as with the former.

Analogy between instinctive, reflex, and emotional actions. The impressions of pleasure and pain have their analogues also in homæsthetic impressions, and the actions resulting from the former result also from the latter. For example, a tickling or a pain is caused by an external impression, and certain motions follow the sensation; the same impression without consciousness will produce the same results. Thus a still-born child or a suffocated person may have the respiratory act and the action of the heart renewed by irritating the nostrils; the same irritation would, if perceived, have caused a convulsive action of the respiratory machinery. In the same way, the acts characteristic of the instincts and emotions may be (and often are) only excited acts, and take place independently of conceptions, being excited by mere impressions, internal or external; consequently the great principle of self-preservation is manifest in the reflex acts, or nerve-actions. He argues that the impressions on the stomach of the decapitated turtle will cause automatically the acts which characterize the instinct of hunger on desire for food, namely, a creeping about as if in search for food, and a peristaltic action of the intestinal canal, although the *sensation* of hunger is not felt. The acts characteristic of the instinct to movement of the voluntary muscles, (see *ante*, p. 198) may also take place automatically, when the same impressions are applied which develop them normally. A living acephalous abortion will draw up its legs when they are pricked, and he says it is related that when a decapitated man was thrust through the heart by a sword, the arms were violently brought together; further, as soon as the headless trunk of a criminal falls to the ground it tries to undo its hands and struggles to get upon its feet; and that the same thing may be seen in a decapitated animal every day. He adds other examples, and says that

they might be multiplied to any extent, remarking, however, that many of this class of movements, as the movements of sighing, crying, shouting, &c., are rendered impossible by decapitation, the "machines" suitable thereto being removed with the head.

In this manner Unzer passes in review the whole of the instincts, passions, and emotions, showing that the same law is in operation with all. With regard to the emotions and feelings, he observes, however, that only those acts can be excited by homæsthetic impressions which are excited by external sensations. The material ideas dependent on the operation of the conceiving force cannot be produced, that force being wanting, nor consequently can the movements corresponding to those ideas be excited. Thus, the acts characteristic of self-defence and of rage may be excited, and the weapons put into action automatically, by means of external impressions only.

Having considered what reflex act or nerve-actions replace voluntary actions, Unzer next shows that the latter may take the place of the former. He remarks that there are three principal kinds of reflex acts or nerve-actions: the first, are those arising from primary internal homæsthetic impressions; the second, from unperceived impressions reflected outwards; the third, from those impressions applied directly. He gives examples of these different kinds: when the spinal cord of a frog is irritated, movements are produced which in the natural state are psychical actions; this is an example of the first kind. Of the second kind is the leaping of a decapitated frog, when its foot is pricked. In discussing the mode in which this reflex impression operates, Unzer takes occasion to point out analogous acts. A man dreams that he is swimming, and acts in his muscles as if he felt that he was actually in water. A decapitated frog thrown into water will do the same thing. The effort of the man to swim is the imperfect psychical action of an imagined sensation (eingebildeten Empfindung), and may be an indirect nerve-action or reflex act of the external impression. So if a man dream he is touching burning iron he withdraws his hand from it, or if a youth experiences a venereal dream, the phenomena of the sexual instinct result from an external impression, just as decapitated animals will perform the same instinctive acts from a similar external impression. Examples of the third kind are muscular contractions, &c. on the direct application of stimuli, or when an intestine or other hollow muscle is stimulated to contraction. This part is closed with a chapter on "the communion or mutual action (Gemeinschaft) of the cerebral forces with the nerve-forces in the natural condition." It concludes with the following aphorisms:

"They err who infer that, because an animal performs animal-adapted actions (Handlungen), it must necessarily possess a soul, or the conceiving force, or volition, since it is undeniably possible that the nerve-forces (vis nervosa) alone can produce the greater number of animal-adapted actions. In consequence of this error, most philosophers have hitherto considered that all animals, without exception, have souls; nevertheless, according to all probability, many of them neither feel nor are conscious.

"It is not correct to infer, that those animal movements of animals endowed with thought and consciousness, which accompany their sensational conceptions, in-

citements, desires, aversions, &c., are psychical actions of the same, and cannot be nerve-actions; or vice versâ, because they are nerve-actions, they cannot be the psychical actions of these sensational conceptions. From this error, taken in connexion with the second mentioned in the previous section, the old error has probably arisen, which the author of the article 'Sensibilité,' in the Dictionnaire Encyclopédique, has revived, that there must be two souls in animals endowed with consciousness, namely, a rational, by which the psychical actions are produced according to physiological laws, and a sensational, from which the psychical actions of external sensations and other sensational conceptions proceed, and which are produced according to the laws of the nerve-forces. Nevertheless, a soul is requisite for the production of that which the nerve-forces effect.

"Lastly, those also err who conclude that, because an animal performs animal-adapted actions which are effected by the cerebral power alone, without the co-operation of the nerve-forces, that all animal movements are the actions of the psychical force. This is the error of the Stahlians, who maintain that all animal movements are psychical movements, nay, the adapted actions of a will, of which the soul itself is unconscious. On this same erroneous proposition depends the old error newly revived by Whytt, that the souls of animals must be diffused through the whole body along the nerves, because in decapitated animals the same animal movements can be produced, which previously were considered to be psychical actions," &c. (pp. 605-6.)

The THIRD PART of the work is devoted to a consideration of Animal Nature as a Whole. Unzer first defines the difference between vegetables and animals: he then divides animals into two principal classes, the one comprising those with souls, the other those without. The latter must be endowed with nerves, or with other machines having similar functions, and with nerve-forces, but not possessed of a brain, sensation, or consciousness. The animals with souls are divided into two classes, the one comprising the reasoning, the other the unreasoning or simply sensational animals. Both are endowed with nerves, vis nervosa, a brain and animal soul-forces, but the soul of the former is a spirit. The faculties of these various classes are then summarily stated in accordance with the principles demonstrated in the previous parts of the work.

In a second chapter, the principal genera of animals are marked out. Referring to the structure in certain insecta, arachnida, &c., which resembles a brain, he observes, that according to all probability it possesses only the vis nervosa, and *being analogous to the spinal cord and ganglia, like them reflects external impressions, receives internal without conceptions,* and by means of both, puts in motion the animal machines. The light, sounds, savours, &c., which in animals endowed with sensations, excite a thousand conceptions and voluntary motions, excite in these animals the vis nervosa directly without the intervention of sensation, and the animal performs automatically the actions proper to various instincts. This position he defends through several pages, and explains the actions of republican insects, as ants, bees, &c., in accordance with the doctrine.

The third chapter is on the origin of animals, and is in fact a discussion of the phenomena of reproduction. The fourth is on animal life, and traces the development, growth, perfection, or period of puberty, and decline of the body. The fifth is on the system of forces of animal life, and is devoted to a consideration of the co-ordination and subordination of the various "machines" which they set in motion, the doctrines being

in accordance with what has been previously laid down. Finally, the sixth and last chapter investigates the phenomena of death, or the cessation of action in the organism.

III. In this analysis, we have endeavoured to do justice to our author, but the reader will readily comprehend that the tone, spirit, and meaning of a book so comprehensive in its scope, and so profound in its principles cannot be fairly given in the space we have allotted to it. We can only say, that it has rarely been our good fortune to read and learn with so much pleasure and profit as on this occasion.

Perhaps a critical estimate of the merits of Unzer's system would properly close our analysis, and especially a comparison of it with those of more modern date. Such of our readers as are conversant with neurology, will have no difficulty in perceiving that Unzer was the teacher of Prochaska. They will also perceive that the doctrines of reflex action are clearly and lucidly propounded under another designation; that views brought forward in recent times as new are here perfectly expressed; and that if the doctrines advanced were adapted to the later discoveries in the anatomy of the nervous system, and the histological structures of the vital agents, they would constitute a more perfect system of vital dynamics than now exists. We will not, however, extend our remarks on this point, preferring to subjoin a biographical notice of Unzer from the pen of the distinguished Professor of Göttingen, Dr. K. F. H. Marx. We may add that this notice is the more valuable, and ourselves and readers are the more indebted to Professor Marx, inasmuch as no memoir of Unzer has been before published. A critical analysis of his works would be a curious addition to psychology, as it is evident that the truths he advanced when his judgment was mature, were only attained to after long and patient study.

John Augustus Unzer (or Untzer) was born April 29th, 1727, at Halle. In his thirteenth year, he exchanged school for the university of his native town, for the purpose of being educated as a physician. He attached himself principally to Professor Funker, who initiated him into the Stahlian doctrines and taught him at the bedside. Unzer himself studied more especially the works of Boerhaave.

When only eighteen, and yet a student, he tried his hand at authorship, and published anonymously, 'Neue Lehre von der Gemuths Bewegungen, Halle, 1746' (New Views regarding Mental Operations), in which he attributed the emotions to varying tension of the nerves. In the same year he wrote a defence of the theory of Stahl, entitled 'Gedanken vom Einflusse der Seele in ihren Körper,' 1746 (Thoughts on the Influence of the Soul on the Body). He also published 'Gedanken vom Schicksale der Gelehrter' (Thoughts on the Fate of Scholars), and also 'Gedanke vom Schlafe und den Traümer nebst einem Send-schreibe, dass man ohne Kopf empfinden könne' (Thoughts on Sleep and Dreams, together with an Essay, showing that there may be sensation without the head), under the somewhat mysterious signature of "S. C. I. S." [Sine capite inducitur Sensatio?] The year following (1747), he published 'Abhandlung vom Seufzen' (A Treatise on Sighing.) On taking his doctor's degree, 9th Sept. 1748, he defended his dissertation 'de Sternutatione,' and the following year the disserta-

tion 'de Nexu Metaphysices cum Medicina generatim.' In 1750, he published 'Philosophische Betrachtung des menschlichen Körpers überhaupt' (A Philosophical Consideration of the Human Body generally.) Towards the close of this year, he left Halle, and went to Hamburgh, where he immediately became connected with the 'Hamburgischen Magazin' (vide the 6th volume), but as he soon got into practice in the adjoining town of Altona, he resided there.

In 1757, he married Johanna Charlotte Ziegler, a poetess, who had the laurel crown conferred on her at Helmstadt in 1753, and who was also a philosopher. For the purpose of obtaining a proper estimate of physicians and medicine, of extending sound medical knowledge, of removing prejudices, and of checking the practices of ignorant practitioners, Unzer resolved to establish a weekly periodical, entitled 'Der Artz, ein Medicinische Wochenschrift.' Being edited in an excellent spirit, and with singular talent in the style and getting up, it exercised a beneficial influence in foreign countries as well as in Germany, for it was translated into Danish, Swedish, and Dutch. It appeared during the years 1759-64 in 12 parts, and to the first a portrait of Unzer, by Tuchbein, was appended.

In 1766, he published 'Sammlung kleiner physicalischer Schriften,' (the third appeared in 1769); in 1768, 'Grundriss einer Lehrgebäudes von der Sinnlichkeit des thierischen Körper;' in 1771, appeared his 'Erste Gründe einer Physiolgie der eigentlicher thierischen Natur thierischen Körper.' This work is probably his most distinguished performance. It is worked throughout in a philosophic spirit; is clear, precise, profound. He simply endeavoured to elucidate without too many general assumptions the obscure doctrines regarding the nervous system, particularly of the ganglia, and the *reflex actions*. He carefully modified his early unconditional assent to the views of Stahl, and specially sought to explain the stimulation of the nerves by natural laws. The animal movements are not mental actions.

As Unzer was in extraordinary request as a physician, on account of his skill, and as a man, on account of his agreeable sociableness, in 1772 he called Dr. I. C. Unzer, his nephew, to his assistance, who died in 1808 at Göttingen, on an excursion to the baths.

In 1773, Unzer edited 'Physiologische Untersuchungen auf Veranlassung der Recensionen seiner Physiologie.' In 1778, appeared 'Ueber die Anstechung, besonders der Pocken, in einer Beurtheilung der Neueren Hoffmannischen Pocken-theorie,' (this was translated into French, in extracts in Vickler's Mémoires sur les Maladies contagieuses. Strasbourg, 1786.) C. H. Hoffman considered certain glands of the skin to be pock-glands, whose function was to secrete a fluid, which by putrefying generated the contagion of smallpox. On the 29th January, 1782, his wife died in the 58th year of her age. In the same year, he published 'Einleitung zur allgemeine Pathologie der anstechenden Krankheiten,' (a copious and substantial work.) In 1783, appeared 'Vertheidigung seiner Einwürfe gegen die Pockentheorie von Hoffmann.' He died, a rich man, aged 72 years, on the 2d of April, 1799.

Art. XIII.

1. *Familiar Instructions in Medicine and Surgery, with Observations on the Means of maintaining the Health of Men on Ship-board, or when employed in Unhealthy Localities. Intended for the Use of the Merchant Navy, Commanders of Yachts, Travellers, and all Persons who may be away from Medical Aid.* By F. F. Sankey, M.D.—*Malta*, 1846. 8vo, pp. 273.
2. *The Scale of Medicines with which Merchant Vessels are to be furnished, in pursuance of Act 7 and 8 Vic., cap. 112; with Directions for their Use; Observations on some of the Accidents and Diseases to which Seamen are more peculiarly liable; and Directions for preserving the Health and promoting the Comfort of Merchant Seamen.* By Charles M'Arthur, M.D., Surgeon Royal Navy. Fourth Edition.—*London*, 1845. 8vo, pp. 76.

In 1844 many respectable London merchants became impressed with the necessity of their ships being supplied with proper quantities of necessary medicines, and the masters of the ships with clear and sufficient directions for their use, with information regarding the means to be employed after severe accidents. They accordingly applied to the Admiralty, and Dr. M'Arthur was appointed by Sir William Burnet, the Director-General of the Medical Department of the Navy, to draw up a scale of medicines with which every merchant ship not carrying a surgeon, was directed to be furnished by Act of Parliament. He also drew up the small work before us as a companion to the medicine chest. The fact of its having in one year reached a fourth edition is alone sufficient to show the demand which existed for some such work, and also, that it, to a certain extent, was calculated to supply that demand; but, on looking it over, we can readily believe, what we know to be the case, that many intelligent merchant captains have complained that it is not sufficiently full or detailed on the subjects treated, and that others on which they require information are altogether omitted. Dr. Sankey, another naval surgeon, practising at Malta, has endeavoured to avoid these objections in another very useful little work, the title of which is also given above.

Every ship which carries one hundred persons or upwards on board, and every ship having fifty persons or upwards on board, the voyage of which is likely to exceed twelve weeks, is obliged, by act of Parliament to carry as one of the complement, "some person duly authorized by law to practise in this kingdom as a physician, surgeon, or apothecary." But by far the greater proportion of our immense merchant navy do not fall within the provisions of this act, and therefore it is at once evident that the works before us are much more important than would at first sight appear, the commercial prosperity of our country so directly depending upon the health and welfare of our seamen. On these grounds we shall examine the works of Drs. Sankey and M'Arthur more closely than we are in the habit of examining works directed to the non-medical public.

It may not be uninteresting to our readers to look over the scale of medicines drawn up by Dr. M'Arthur, and sanctioned by act of Parliament. We therefore extract his table. The quantities are to be sup-

plied for ten men, and to be increased proportionally to any additional number of men borne:

	lbs.	oz.	drs.		lbs.	oz.	drs.
Castor oil	1	0	0	Balsam of capivi	0	8	0
Epsom salts	4	0	0	Opodeldoc	0	8	0
Powder of jalap	0	4	0	Spirit of hartshorn	0	4	0
rhubarb	0	4	0	Paregoric elixir	0	4	0
ginger	0	2	0	Essence of peppermint	0	1	0
Carbonate of soda	0	8	0	Alum	0	8	0
Tartaric acid	0	8	0	Blue stone	0	4	0
Senna leaves	0	6	0	Quinine	0	0	4
Sulphur	0	8	0	Calomel	0	4	0
Cream of tartar	0	8	0	Dover's powder	0	2	0
Nitre	1	0	0	Mustard	1	0	0
Laudanum	0	4	0	Mercurial ointment	0	8	0
Tincture of rhubarb	0	8	0				

"Bullin's blistering fluid, instead of blistering plaster; 2 yards of adhesive plaster; lint, 4 oz.; simple cerate, 8 oz.; basilicon ointment, 8 oz.

"*Pills.* 2 doz. Purg. (Pil. Col. Co. Ed. Phar.); 2 doz. Blue pills; 2 doz. Opium pills (Phar. Edin.)

"*Powders.* 2 doz. Purgative, each to contain ʒj of compound powder of Jalap and 2 grs. of Calomel; 2 doz. Emetic, each to contain 15 grs. of Ipecacuanha and 2 grs. of Tartrate of Antimony; 2 doz. Sudorific, each to contain 10 grs. of Nitre, 10 grs. of Cream of Tartar, and 5 grs. of Dover's powder; 2 doz. Injection, each to contain ʒss. of the Acetate of Zinc.

"1 set scales and weights; 1 marble mortar and pestle; 1 file; 1 graduated measure; 1 funnel; 1 small pewter cup; 2 pewter teaspoons; 1 spatula; 1 pair of scissors; 1 syringe; 2 lancets; 6 bandages of different sizes; 6 yards of calico; a paper of needles, pins, and thread.

"The pills and powders are to be carefully put up in tin or leaden boxes, and properly labelled."

Dr. Sankey has made the necessary additions of Diluted Sulphuric Acid, a Caustic Case, an Elastic Catheter, Enema Apparatus, Tourniquet, and a set of Splints for legs and arms. These are so obviously necessary that we are surprised they should have been overlooked in the official scale; but we cannot recommend some other additions of Dr. Sankey, such as Croton Oil, Creosote, James's Powder, Red Precipitate, Sugar of Lead, and Tincture of Colchicum, as it is of so much importance in a small ship only to take what is absolutely necessary, in order to avoid expense, confusion, and mistakes. Perhaps the Iodide of Potassium and the Muriated Tincture of Iron, in Dr. Sankey's list, might be advantageously admitted. For the reasons just stated, we also think the powdered Jalap, Senna Leaves, Tincture of Rhubarb, and Opodeldoc might be omitted from the official list. There are sufficient purgative medicines without the first two; a glass of wine and water with a little spice would answer every purpose of the kind; every ship carries some oil and turpentine, and with this a liniment can be immediately prepared equal if not superior to opodeldoc. Instead of these medicines we should increase the quantities of Cream of Tartar, Laudanum, and Quinine. Half an ounce of the latter is far from sufficient for a ship trading in tropical climates. Some cotton wool for burns, and oiled silk should also be added.

The first part of Dr. Sankey's work is a sort of medical dictionary, embracing the most important subjects which interest the parties he addresses. He has strictly avoided everything like pedantry or technicality, and has written in a most explicit and intelligible manner, adapting plain

and familiar language with great tact to the common sense of his readers. The few pages in Dr. M'Arthur's work, on Accidents and Diseases, contain a great deal of very useful information in a very highly condensed form, but he has altogether omitted some important subjects; retention of urine, for example, is unnoticed, although particularly common among seamen; nor do we find any mention of hernia, catarrh, chilblains, whitlows, boils, piles, jaundice, itch, tooth- or ear-ache, all of which are of very frequent occurrence at sea. The language, also is somewhat technical, and by no means so intelligible to uneducated persons as that of Dr. Sankey. On the whole, we think the work of the latter by far the best of the kind which has hitherto appeared, but it contains some directions which might be amended.

The general directions for the suppresssion of bleeding from wounds are very good, and the course of the principal arteries, with the spots for the application of the pad of the tourniquet are shown by a plate, but we very much doubt the propriety of telling a sailor, "you would be justified in cutting down upon the vessel, enlarging the wound, and searching for the cut artery." (p. 11.)

A great deal of useful information is given regarding dislocations and fractures, but we think a few simple woodcuts would have been much more acceptable. We are not sure if any description would render the appearances produced by these accidents recognizable, while it would be very easy to point out that when a limb is in the position shown by one plate, it should be replaced by the means shown in another.

As a general rule, we entirely depart from the practice inculcated in fevers. With regard to letting of blood, we are told that "a decided impression should be made on the disease by the quantity removed; and the pulse should be reduced in force and frequency by the loss." (p. 69.) Dr. M'Arthur holds much the same opinions; but we maintain that such treatment is much more likely to do harm than good, even in the hands of a medical man. Experience is daily more strongly proving that fevers are the result of some specific poison, and run as determinate a course as smallpox, or any other specific disease, and that bleeding only lays the system more completely under the influence of the poison. Of course it will sometimes be a question with a medical man whether he had not better relieve some local symptom by depletion, even with the risk of depressing the general system; but we maintain that no such discretion should be given to a sailor. It would be far better—and generally it would be quite sufficient—to insist on thorough cleanliness, and ventilation, as much quiet as possible, cold sponging, simple diet, and plenty of water or bland fluid to drink. Neither of the writers before us have neglected to point out the importance of these matters, but in our opinion both have been much too heroic in the medical treatment they recommend a merchant captain to pursue—bleeding, emetics, purgatives, calomel, tartar-emetic, quinine, cold affusion, opium, wine, and blisters, forming a case of edge-tools, which would be far better kept out of sight.

Under the head of inflamed windpipe, the merchant captain is directed to "throw the head back to put the windpipe upon the stretch, and then just below the gristle or round ringbone of the throat under 'Adam's apple,' you begin to cut through the skin and flesh in a line down the centre for an inch; then push the point of the knife through into the

windpipe, and make a cut in it, dividing two or three rings large enough to breathe freely through," (p. 160.) We fear, if this recommendation were attempted to be followed, we should have occasionally a literal "cutting the throat" in place of a sanative chirurgical operation.

In the treatment of wounds, Dr. Sankey leans a little to the seaman's weakness in favour of Friar's Balsam; but as he pours it *over* the bandage, it may perhaps be of use in forming a stiff covering to protect the wound from air and dirt. One merchant captain, to our knowledge, at once settled the fate of Dr. M'Arthur's scale and book by exclaiming "Why, sir, there's no Friar's Balsam in it!"

Dr. Sankey speaks very highly, and we believe with some reason, of the utility of an ointment, composed of a drachm of red precipitate and an ounce of resin cerate, in the indolent ulcers of seamen.

Both works contain very useful directions for preserving the health of seamen and avoiding disease, under the heads of food, clothing, bedding, cleanliness, ventilation, climate, intemperance, malaria, &c.; but we think that scales of diet in health and disease ought to have been given. Dr. M'Arthur speaks in high terms of the chloride of zinc introduced into use by Sir William Burnett, as a means of destroying the odour of bilge water, killing vermin, and preserving timber, canvas, cordage, and ship's stores. As it does not emit the disagreeable effluvia which form so strong an objection to the use of chloride of lime, nitrous gas, &c., it would be advisable to serve a regulated quantity with each medicine chest.

Dr. Sankey's book is concluded by an appendix containing a paper on Phthisis, some observations on Homœopathy and Hydropathy, with some useful general directions for travellers. As we have hitherto chiefly extracted what we considered objectionable, we think it but fair to give a specimen of the plain common sense of which the greater part of the book consists.

"*Homœopathy.* This new system of curing all diseases, is one of the novelties which occupy the world's attention for a time and make the reflective think. It is a plan to cure by influencing the imagination.....The medication by almost invisible globules ranks in the same class with fortune-telling and mesmerism. The conjuring part of the system by infinitesimal doses is founded on principles which are absurd and intolerable. But the manner of living to be observed by those who follow this cure is highly rational, and without which the doses of medicine would be utterly inert. [They are inert *with* the manner of living.] Persons are to abstain from all excesses, avoiding many things which experience proves to be hurtful, to live with great regularity as to hours and pleasures, to take air and exercise, to pass little time in bed, &c. &c. The whole treatment may be summed up in few words, 'To live moderately, and earn that living by your labour.' The cure of acute diseases can hardly be trusted to homœopathy. The nervous, the delicate, and the luxurious are the proper objects to be treated by this method. The system was invented by a man of genius, Hahnemann, and it has done infinite good by disabusing the world as to the necessity of employing in all cases powerful medicaments uncertain in their effects, rather than to assist Nature to remove the weight which is pressing her down. Every honest medical man must acknowledge that very many severe cases of disease are produced by impertinent interference, by rude medical discipline, and by the injurious exhibition of drugs; by which routine practitioners gain great reputation, first making the disease which they afterwards may have the merit of curing.

"No practitioner of any standing, but must be sensible of numerous errors of diagnosis made, and of ill-judged remedies prescribed, even by himself; errors

which it has taken years of experience in the course of a long practice to correct. At any rate homœopathy is not so dangerous a weapon as physic in the hands of the ignorant, yet less effective when the latter is regulated by skill and judgment." (p. 81, pt. 2.)

We are rather surprised that our author, holding these opinions, should have recommended the violent means we have reprehended.

It will scarcely be believed, that in order to make the above extract readable, we have been obliged to omit two full stops, one colon, seven semicolons, and twenty-three commas, and this is by no means one of the worst specimens of punctuation in the book, which in this respect is perhaps the most extraordinary ever printed. The author makes an apology for "colonial getting up"—and well he may.

If a future edition of Dr. Sankey's work should be required, we would recommend an English publisher, and some of the alterations we have suggested, with separate lists of medicine for merchant vessels, yachtmen, and travellers, instead of one general list for these different classes. Scales of diet should also be given, and the use of plaster-of-Paris, or starch bandage in fracture described. Perhaps also, as women are often delivered on board ship, a few directions to be followed in cases of natural labour might be given; but we confess the subject to be a delicate one. The use of the probang also ought certainly to be described, as threatened suffocation from hastily swallowing large morsels of food is not an unfrequent occurrence, and requires the most prompt assistance.

We regard both works as well planned but imperfectly executed.

ART. XIV.

Mental Dynamics, or Groundwork of a Professional Education. The Hunterian Oration, before the Royal College of Surgeons of England, 15th February, 1847. By JOSEPH HENRY GREEN, F.R.S.—*London,* 1847. 8vo, pp. 64.

HERE is a disciple of a philosopher in the antique spirit, uttering those truths which he has imbibed from the conversations of the sage. If there was a modern man who realized "the old masters of the Academy or Lyceum," who in these times when all instruction is conveyed in books or in public speaking, was yet more eloquent, and full and instructive in conversation, and to a band of affectionate and trusting disciples imparted lessons which they received as oracles, it was Coleridge, the guiding spirit of Mr. Green's mind. "Throughout a long-drawn summer's day," says one of these disciples, "would this man talk to you in low, equable, but clear and musical tones, concerning things human and divine; marshalling all history, harmonizing all experiment, probing the depths of your consciousness, and revealing visions of glory, and of terror to the imagination; but pouring withal such floods of light upon the mind, that you might for a season, like Paul, become blind in the very act of conversion. With a calm mastery over your soul, leading you onward and onward for ever through a thousand windings, yet with no pause, to some magnificent point in which, as in a focus, all the particoloured rays of his discourse should converge in light."* To have been the intimate

* Table Talk of S. T. Coleridge, by H. M. Coleridge. Preface, xvi.

friend, and the favoured disciple of such a man during the latter part of his life, and to have been left the literary executor of his unfinished and most valued work, was the high honour of Mr. Green; and, taking this public occasion in which he was called on, under somewhat peculiar circumstances, to deliver an oration to the honour of Hunter, he has paid the worthiest tribute to his own teacher in philosophy by enforcing those truths which it was the life's object of that great man to proclaim. There was something peculiarly appropriate in joining the names of Coleridge and of Hunter. For, as all readers of 'The Friend' remember, Coleridge at an early period of this century comprehended Hunter's idea, and the purport of his museum in unfolding it, and proving it, and he correctly estimated the effect of these physiological views on practical Medicine and Surgery, and on the progress of comparative Anatomy. (See 'The Friend,' vol. iii, pages 181 and 215, 2d edit.) That he should have expressed these conclusions, and many times repeated his discriminative and high eulogies on Hunter in a work, the object of which was to exhibit the true method of all investigation, scientific and philosophical, is quite a sufficient reason for bringing the names of Coleridge and Hunter together, and for introducing, even in an Hunterian Oration, the name of a poet and a philosopher as an authority for the principles on which a professional education should be conducted.

"Hunter's peculiar merit," says Mr. Green, "was to have first fixed the principles of physiology, by banishing hypotheses, fictions, and arbitrary assumptions, and by considering life as a law;—implying that it is a power anterior, in the order of thought, to organization, which yet it animates, sustains, and repairs—a power originative and constructive of organization, in which it continues to manifest itself in all the forms and functions of living being." (pp. 6-7.)

We are as ready and willing as Mr. Green to testify at all times to the surpassing merits of Mr. Hunter; but we must demur to the adequacy of the illustration selected: the "peculiar merit" here held up for our homage, we believe to be a positive error. But the orator's purpose was not so much to consider Hunter's merit as a physiologist or a surgeon, but as a pattern for the formation of a scientific character. And here the difficulty presents itself, that as the characteristic of Hunter's mind was genius, and if genius is looked upon as a special gift of Providence imparting to its possessor somewhat of the creative power of the Divine giver, it cannot then be acquired by mental culture, and is no example for our imitation. But instead of thus viewing genius, our professor regards it as the healthy balance and harmonious development of all the mental faculties, moral and intellectual, brought into full action by the persevering energy of a constant will. It is this energy of character, with genial power, which stamps an indelible unity on the works and acts of Luther, of Dante, of Milton. But not a mere lawless will, but a will under the influence of the conscience, a moral will, for the powers of the mind cannot be harmoniously developed unless the will is subjected to this imperative law. Such a man feels that his aim in life, the very purpose of his existence here, is to cultivate by strenuous and indefatigable exertion those powers, moral and intellectual, which are the birthright of our humanity: that this is the goal to which he must strive to approach—this is the ideal man. To attain or rather to approximate to this point, is the object of "a liberal education," and the purport

of this Oration is "to define the intellectual discipline which will best prepare the mind for the scientific cultivation of a profession, and aid the individual in forming his character in the light of a final aim." The golden rule of all systematic education, according to Mr. Coleridge and his pupil, is, that all living knowledge proceeds from within the man; that it may be trained, fed, and excited from without through the senses by instruction, but not infused or impressed by such means; that his real mental progress will depend upon his own exertion of the inward powers which have been thus roused and trained. Hence the necessity of disciplining the mind to habits of reflection, and to those branches termed Abstraction and Generalization. How early these faculties come into play is seen in the child, who, in his perception of things that are alike, abstracts what is like in any object from what is different, and by associating these like objects together, he generalizes.

Words are the earliest products of these powers, and the commencement of education. Hence the importance of grammar, which teaches the first lessons in thinking of the meanings of words, and initiates in the art of arranging words and sentences perspicuously, so that "it would be difficult to attach too much importance to a knowledge of words, their definite import, and right use, as grounded in grammar and evinced by a correct style." Such discipline leads to the acquirement of many kinds of knowledge which assist that great purpose ever to be kept in view, the harmonious development of the whole mind.

"Thus, as admirably suited to engage and exercise all the faculties, which may be supposed to be predominant, active, or capable of further evolution at an early period of mental training—we may mention natural history, considered here without especial reference to its scientific character—a study, which offers food for every digestion, knowledge for every capacity, and which tends more perhaps than any other to people, to enlarge, and to tranquillize the mind." (p. 18.)

Here curiosity is ever excited, attention riveted, and memory bribed by perpetual novelty, variety, and beauty. The comparing power is ever kept alive by an endless succession of similitudes and contrasts; and the reasoning power is finally evolved in order to trace and to explain the adaptation of means to proximate ends in instincts, which, when enlightened by reason, become in man the human understanding.

"Thus, as the student watches the ascension of nature into mind, he shall learn that, up the whole ascent, nature is a prophetic-hymn, heralding the advent of man, and proclaiming the wisdom and goodness of the Creator." (p. 19.)

As natural history is nearly conjoined with the history of man by a knowledge of the earth as his abode, a knowledge of geology prepares the way for that of the history of man.

"His primæval state, the separation of races, the gathering and swarming of hordes, the settlement of nations, and the formation of political confederacies and states—history, exhibited as the great scheme of Providence, which has been, and ever is, operative in the moral education of man, considered as the mind and soul of the planet." (p. 21.)

Looking back at the races of men we see one momentous difference—

"A certain number of nations, or people of the earth, retain the conditions of progress, while the remaining parts of the species bear the decisive stamp of their inferiority, their improgressiveness; communities, whose to-day is an everlasting yesterday, or varied only by the accidents of the day, more or fewer calamities, less or greater wretchedness. The proximate cause of this awful fact is evidently

this.—The nations that are historic and progressive, contain in themselves the means and instrument by which their progress may be effectuated, namely, literature;—literature as connecting one nation with others diversely though equally cultivated, and as connecting the present with the past and with the future. They possess that which gives a continuity to time, and emancipates both individuals and communities from the hard necessity of existing as successive fragments,—like a glow-worm in the scanty circle of its own light, all before and all behind and, save in the narrowest vicinity, all around—in impenetrable darkness. But literature, thus regarded in its high vocation of representing the total mind of a nation or people, no nation can have without a living intercommunion with the master minds of all ages and countries. Need I say that this communion, this intelligible converse, whether we speak of the nation collectively, or of the individuals of which it is composed, can only be the result of a familiarity with the language, which is the medium of communication, and with the words, which are the living educts wherein the productive thoughts are embodied? Any additional language, of which a man is master, is as it were a new limb without its deformity. And I scarcely need dilate on the importance of that which enables a man to collect into his own individuality the discoveries, the mental wealth, the ennobling affections, and the models of the wise and great of countries and states under the most auspicious circumstances; or on the moral utility of an instrument, which may enable him to confer a portion of his knowledge and power on the inhabitants of less fortunate countries." (pp. 22-3.)

The study, therefore, of languages of these "literæ humaniores," is indispensable to explain the process by which the present state of intellectual cultere has been attained, and "to arm the mind for future enterprise and further conquests." "How shall we correctly interpret the oracles of God without a critical study of the languages in which they were recorded, written as they were for our instruction in that which concerns every man who is not wanting to himself?" How shall we learn political science unless we read the historians of antiquity? "What reliable knowledge of philosophy or of speculative science, without invoking the genius of Plato, and Aristotle? What hope of a cultivated taste without a familiarity with the excellencies of the models of ancient Greece, with a view to their fruitful comparison with the master-works of modern genius?" Such knowledge cannot be acquired by all in equal proportion and in early youth, but the sketch may serve as a comprehensive survey to an education which has claims to the term "liberal."

The orator assumes that such knowledges obtained in public schools may be considerable. Their use is to educate the intellect for the real business of life, so that a mind thus furnished and where powers have been thus strengthened, may be used as an instrument for the attainment of that practical knowledge and skill which the future sphere or profession of the individual may require. And as grammatical studies exercise the powers of analysis and synthesis, and as all sound judgment depends on these mental functions, the connexion of languages with a practical education (the *cui bono* of Latin and Greek) is thus apparent.

But there are two other sciences more or less indispensable in this preparation for the concerns of life.—Mathematics and Logic.

Though by mathematics, or the sciences of figure and number, the natural world has been brought under the intellectual and manual power of man, it is not on this account chiefly that its aid is important in intellectual training and discipline. Well hath Plato said—

"Let no man enter the schools of philosophy, who has not previously disciplined

his mind by geometry. He considered this science as the first purification of the soul by abstracting the attention from the accidents of the senses." (p. 29.)

The proper province of logic is reasoning.

"Reasoning is the daily and hourly business of our lives, and, whatever our worldly calling—in the pulpit, at the bar, or at the bedside of the patient—we are unceasingly occupied in inferring or proving a something from what is already ascertained or taken for granted. And in the words of an eminent writer on logic, 'To learn to do that well, which every one will and must do, whether well or ill, may surely be considered as an essential part of a liberal education.'" (p. 31.)

To master, therefore, the art of reasoning conclusively, and to guard against those causes which interfere with sound reasoning, "we should especially accustom ourselves to think and reason in precise and steadfast terms."

And here we take the liberty of quoting a rule laid down by Coleridge in his 'Aids to Reflection,' as containing advice by which those who know nothing of logic as a science may avoid the sophisms of others, or of their own minds; an aphorism of constant application in that reasoning which all of us are engaged in silently at the bedside, or in consultations, or in conversations with the sick or with their friends.

"Whether you are reflecting for yourself, or reasoning with another, make it a rule to ask yourself the precise meaning of the word, on which the point in question appears to turn; and if it may be (that is, by writers of authority has been) used in several senses, then ask which of these the word is at present intended to convey."

The professor having thus reached this vantage ground from which can be viewed the requisites of a liberal education, rises to a higher theme, and suspects that already the student who has followed him will discover that among these means of self-culture no place has yet been assigned for truths "that have an especial relation to the moral and spiritual being." Truths which no self-inquirer can fail in discovering to have their "birth, growth and requisite foundation in the whole man, head and heart, and without the possession of which he will live in the eclipses of the better half of his being, moral and intellectual. Not mere truths of the intellect, which in its noblest offspring—Science, is still but a fragment of the whole man. Truths, not left to fortuitous choice, or uncertain proof; but realities, powers of truth, which actuate as well as enlighten, and which are coextensive with our humanity at all times, in all places, and under all circumstances;"

"—Truths that wake
To perish never;
Which neither listlessness, nor mad endeavour,
Nor man, nor boy,
Nor all that is at enmity at joy,
Can utterly abolish or destroy!

Hence, in a season of calm weather,
Though inland far we be,
Our souls have sight of that immortal sea
Which brought us hither,
Can in a moment travel thither—
And see the children sport upon the shore,
And hear the mighty waters rolling evermore." (p. 34.)

And what are these living truths, "which are the light of all our seeing?" Are they not the idea of the good—the idea of the beautiful—the ideas of eternity, immortality, freedom, holiness, happiness, duty,—the idea of our own soul as the reality of which the body is only the covering, "of God as the absolute cause of all reality, Supreme Being, containing all perfection, excluding all want, privation or negation, in the plenitude of goodness, truth and love?" These are the living truths which do actuate man at all times more or less unconsciously, in childhood as high moral instincts by which he is impelled and governed, and which, when reason has become his guide, are to his self-conscious mind, ideas, principles, or final aims, and when sifted by reason he asks for the source of these eternal truths

"that are the life and reality of his spiritual being, where shall he find it but in the Supreme Will causative of all reality, and how name it but the Living Truth,

'—whence the soul
Reason receives; and Reason is her being.'

"But thus conceived there neither is, nor can be, but One Reason:—and, in truth, it is a statement of the Christian doctrine, that the Word by whom all things were made, is essential Light and Life to his creatures;—it is the sublime doctrine revealed by St. John, that the Reason is the light and spiritual presence of the Logos: *τὸ φῶς τὸ ἀληθινὸν, ὃ φωτίζει πάντα ἄνθρωπον ἐρχόμενον εἰς τὸν κόσμον.*" (p. 37.)

Finally, if reason be the sole universal light of man, it follows that it is the essence of all science.

"There are indeed many sciences; still they must all retain the same inherency in, the same known and understood derivation from, the common trunk, of which they are living and growing branches. . . .

"I will conclude by comprising in a few short paragraphs the qualifications of a medical practitioner, considered in relation to the dignity and efficiency of the medical profession according to its ultimate aim.

"I need scarcely indeed dwell upon the possession of technical knowledge and skill; since it is evident that the medical practitioner, who aims at the performance of those duties, which his profession demands, will possess himself of the requisites for its practice, which no honest man would be without. But if I am asked, what are we to understand by the amount of skill and knowledge, which may be justly demanded of the members of a liberal profession, and here of the medical? It is evidently this, that each severally should be capable of applying all the resources of art, which the whole profession can supply. And in this country, and at the present time, in no small proportion of cases, every man, who has fairly and in good earnest availed himself of the advantages, which our medical institutions afford, will have the satisfaction of feeling that he is doing what every other regularly educated physician or surgeon would do under the same or similar circumstances. And where, from the imperfection of our knowledge, we are unable to refer the facts and phenomena to intelligible principles, it should still be our aim and endeavour so to arrange and combine them as to bring them more and more under the conditions, which facilitate the discovery of a principle.

"Hence then, in stating the qualifications of the members of the medical profession, we place as the second requisite scientific insight, or the possession of those laws, or rational grounds, which form at once the principles and ultimate aims of all professional knowledge. Disjoined from the patient and persevering details of observation, the search for facts, and the wakeful attention to them when presented, the healing art would soon fall back to the state, in which during a portion of the middle ages it actually existed, when medicine was little better than a fantastic

branch of logic. On the other hand, wholly separated from all speculative science it would necessarily become a mere collection of cases, of facts without any copula that might render them severally or collectively intelligible—nay, without any security that the supposed facts are actually such, or that the most important incidents may not have escaped the notice of the observer. But this is not all. It is not in the nature of the human mind to remain satisfied with the mere record of always imperfect cases. In the absence of insight the imagination takes its place; ostentatious affidavits supply proofs of the efficacy of the medicine and of the *methodus medendi*;

> 'And puffing quacks confiding dupes allure
> 'To swear the pill or drop has wrought a cure!'" (pp. 38-40.)

"Lastly;—in order to secure the enlarged education, which is to realize the ardent wish and deliberate scheme of the council of this College for the elevation of the surgical profession,—[a scheme which the reviewer takes the liberty of adding, would have met with the unqualified approbation of the members of that college if it had been carried out in the spirit of a kind and considerate justice to the older surgeons of the country who have grown gray in the actual practice of surgery]—I have only to express my unaltered conviction, strengthened indeed by the success with which the trial has been already attended,—that it is in universities and colleges that a medical education may be best grounded on those universal elements of science, which are the essential constituents of every liberal profession. I hold it indeed scarcely possible that any professional education can be fully accomplished except in such institutions;—where discipline both intellectual and moral, and a pledged direction and supervision of the studies, give the requisite security for its progress and completion;—and where the *Alumni* are induced habitually to regard themselves as members of one body, and to form among themselves a correspondent law of honour, of self-respect, and of respect for each other as fellow-collegians—with the cognate habit of despising the hollow, the tricky, and the ostentatious,—in short, to form that sentiment of honour and gentlemanly feeling, in which the moral life of the individual breathes as in its natural atmosphere, with an unconsciousness, which gives the charm of unaffected manners and conduct." (pp. 38-40.)

We conclude our pleasant task of giving a summary of an Oration so artistic in its conception and execution, so noble in its purpose, so elevated in its thoughts, so correct in its principles, so deeply founded on true science. Where we have not directly quoted we have often conveyed our own reading of the professor's views, often in more common and more intelligible language, in order to accommodate those unaccustomed to its specific phraseology, hoping that this recapitulation may serve as an inducement to many to read the original, which has an Appendix on several metaphysical points, the full understanding of which is necessary to the due comprehension of the lecture.

Such an essay requires careful and even repeated reading, especially by those not conversant with the writings of Coleridge. As an Oration delivered to a body who can hardly be thought to be thus furnished, its value must have consisted in the general impression it conveyed of an earnest religious-minded man, of cultivated intellect, and with the highest spiritual aims, enforcing with eloquent words, and with a chivalrous spirit, his deep convictions that the true way of elevating the medical profession, and making it indeed the calling of a gentleman, was by a comprehensive education that developed harmoniously all the faculties of the man.

If its effect were tested on the mere information each hearer had gained, or in his power of carrying away the line of argument, it might have

been pronounced to have failed; but those accustomed to the best examples of parliamentary eloquence, recognised in the orator one whose commanding person, voice, elocution, self-possession, proved him gifted by nature for his office, whilst the general effect of his full-toned and somewhat gorgeous eloquence was one of raised and higher feeling, with a justifiable satisfaction that such a man, so endowed both naturally and by mental culture, should have been found to represent the surgery of England, and to cast over this annual recognition of the genius of Hunter a certain nobility and grandeur, from a gleam of light proceeding from a loftier region of thought and aspiration than that to which the common ordinary man attains.

Art. XV.

Outlines of Structural and Physiological Botany. By Arthur Henfrey, F.L.S., &c., Lecturer on Botany at St. George's and the Middlesex Hospitals.—*London*, 1847. Fcp. 8vo, pp. 298. With Eighteen Plates.

The rapid advance of botanical science, and especially of that portion of it relating to the structure and growth of the primary tissues, renders the appearance of a new elementary treatise, from the pen of a gentleman who is well known to have devoted great attention to the study, extremely welcome; and a careful examination of the result of his labours has only increased our satisfaction, in almost every particular. The following quotation from his preface will show that he is fully alive to the importance of recent discoveries; and that he is by no means one of those writers of manuals, who take their cue from the antiquated treatises of their predecessors.

"In the natural sciences, especially, the accession of new information, whether from original investigation or from the persevering application of repeated examination and comparison to received views, is continually modifying the very fundamental doctrines on which our superstructure of knowledge temporarily rests; and thus, at certain stages, arises the necessity for the revision of the whole, the rejection of superseded and superfluous disquisitions, and for the assumption of new points of view, from which to regard much of that with which we have been previously acquainted. From such and analogous causes originates the production of new books of the nature of the present; and in them is to be found the justification of those who devote themselves to such tasks."

The first chapter contains a condensed account of the chemical constituents of plants; and in the second, the author proceeds to treat of their elementary structure, commencing with "the cell as an individual," and then proceeding to "cells in connexion, or tissues." In this manner, starting with the simple ideas derived from the consideration of the structure and actions of the single cell, he proceeds synthetically to build up (as it were) the entire plant, with its several organs and its distinct series of operations. Mr. Henfrey has bestowed considerable attention on the process of multiplication of cells, as it takes place in the plant, and has presented to the British Association more than one valuable communication on the subject. We shall endeavour to convey an idea of his doctrine, although this is difficult without the aid of figures. The wall of all young cells is lined by a very delicate membrane; which is termed by Mohl (who first pointedly directed attention to it as a general constituent of the cell)

the "primordial utricle." It is easily brought into view by tincture of iodine, which causes it to become brown and to contract, so as to detach it from the outer wall of the cell. We had ourselves observed this inner membrane long before it was described by Mohl; but had supposed it confined to the cells of the epidermis, in which alone we had met with it. Now, according to Mr. Henfrey, the division of a parent-cell into two (which is the usual method of multiplication, except when distinct germs are formed within it and set free by its rupture) is accomplished after this manner. The primordial utricle folds inwards, as if it were constricted by a cord passed around it; and the fold gradually grows inwards towards the centre, separating the cavity of the cell into two parts, which at last become perfectly closed and distinct sacs. Each lamella of the primordial utricle deposits a layer of matter on its exterior, constituting the permanent cell-wall; and thus a complete partition, continuous with the external cell-wall, is formed between the two newly-generated cells. This we believe to be the exact representation of the mode in which the multiplication of the cells of cartilage takes place; but whether it is the universal method of cell-generation in the animal and vegetable tissues, it would be premature yet to assert. The nucleus does not seem to perform any important part in these changes; and Mr. Henfrey believes with Mohl that it is intimately connected with the chemico-organic changes of the cells-contents, and especially in the elaboration of the mucilaginous granular matter, which has been well named by Mohl the "protoplasma" of the cell. Our readers will have noticed the recent tendency, among animal physiologists, to regard the nucleus as rather the essential agent in the chemico-organic changes, than as having any necessary connexion with the reproduction of the cell; and it is interesting to see how these two conclusions, founded upon such remote data, tend to confirm each other.

In the third chapter we find some "General Considerations on the Physiology of the Elementary Structures;" in which are embodied many facts of very great interest. From these we may quote the following concise statement of a phenomenon which has lately attracted much attention, and which will probably lead to an entirely new view of the character of the greater part of the so-called polygastric animalcules.

"The spores of some of the lower Algæ present some remarkable phenomena of motion. They consist of cellules furnished with appendages resembling cilia, which enable them to swim about in water like animals. Thuret and Kützing have found in these a red spot resembling the so-called eye of Ehrenberg's green monads. The fact of the development of these spores from undoubted plants like the Confervæ, goes far to prove that the Diatomaceæ, &c., and even Volvox and its allies, are plants. In this case, the existence of spontaneous motion in plants would be a well-marked character of vegetable life; but we are still in the dark as to the causes and nature of such phenomena."

The fact of the presence of cilia on cells of undoubted vegetable character, and of the performance of regularly continued movements by these bodies,—the absence of any definite character by which the movements of the so-called polygastric animalcules may be distinguished,—the strong resemblance between the general characters and mode of multiplication of beings of that class, and those of the undoubted plants on the one hand, and the animal ovum in its earliest stages on the other,—seem, when taken collectively, to warrant the doubt whether any adequate proof exists of the

presence of *consciousness* in the Polygastrica, and whether we ought not either to exclude them from the animal kingdom, or to extend our notion of an animal to beings unpossessed of sensation; and if the latter alternative be adopted, we cannot see how any definition shall be framed that shall separate the animal from the vegetable. Perhaps, after all, it would be better not to attempt to carry such a separation through the lowest forms of organic existence, which present such a striking resemblance to the early embryonic conditions of the higher; and to classify those beings, in which no definite nervous system can be discovered, but which exhibit determinate movements, rather in conformity with their general organization and with their mode of reproduction, than by any arbitrary assumption regarding their possession of consciousness.

The remaining portion of the work is occupied with the consideration of the structure and physiology of plants considered as individuals; and we cannot too highly praise the manner of treating the various departments of these subjects, which is as remarkable for its clearness as for its truly philosophical character. The Cryptogamia are assigned their true rank in the scale of physiological importance,—for the first time, we believe, in any English botanical treatise claiming for itself a scientific value; and the author has brought together a large amount of new matter regarding their structure and actions, from the observations of eminent continental botanists, which will be new to the greater part of our readers. The only fault we have to find is with the illustrations, which, though very numerous and well-selected, are deficient in clearness, and by no means keep up the credit of Mr. Van Voorst's beautiful series of publications. They have the advantage, however, of being drawn by Mr. Henfrey himself; and their scientific correctness (a point about which mere artists often care but very little) is thus guaranteed.

Art. XVI.

1. *An Essay on the Relation of the Theory of Morals to Insanity.* By T. Mayo, M.D., &c.—*London*, 1847. 8vo, pp. 49.
2. *Elements of the Pathology of the Human Mind.* By Thomas Mayo, M.D., F.R.S.—*London*, 1838. 12mo, pp. 182.
3. *Clinical Facts and Reflections: also Remarks on the impunity of Murder in some Cases of Insanity.* By T. Mayo, M.D., F.R.S.—*London*, 1847. 8vo, pp. 217.

The last of the three works of which we have transcribed the titles, is the only one coming legitimately within the scope of our duty in this Journal; but we have been led to refer to the other two, from the circumstance of our attention being particularly arrested by the chapters in the new volume relating to the Medical Jurisprudence of Insanity. To this last subject we shall exclusively confine our attention at present; hoping to notice the other very miscellaneous contents of Dr. Mayo's last publication, on some future occasion. The greater part of these papers have already appeared in the 'Medical Gazette,' and are therefore not of a novel character; but the members of the profession will thankfully receive them in their collected form, as the productions of an experienced physician, a man of science, and a scholar.

We have long been accustomed to think that a correct appreciation of the relation between criminal responsibility and insanity can only be formed by a more complete analysis of the psychological characteristics of crime and madness, than has yet been effected. No such analysis can be successful, which is not based upon a correct view of the normal operations of the mind, and especially of those which are commonly referred to the *moral* faculties. Both metaphysicians and theologians have been accustomed to speak of the moral sense or conscience as furnishing to every man an invariable rule of right and wrong, any departure from which constitutes a violation of moral or religious duty; and lawyers have taken for granted the existence of such a standard, and have made the test of the sanity or insanity of a criminal to depend upon his supposed ability or inability to distinguish right from wrong at the time of the commission of the offence. Into this doctrine we shall institute as searching an examination as our limits will permit.

The psychical nature of man is usually regarded as composed of intellectual powers, of moral feelings, and of propensities connected (more or less closely) with the animal wants: the first alone constitute the reasoning faculty, which may be exercised independently of the others, as in a mathematical demonstration; whilst the others in all the ordinary affairs of life are concerned in supplying motives to the reason, and are hence the mainsprings of conduct, from which they have received the designation of *active principles*. We have endeavoured on former occasions to show that a further analysis is practicable. All mental operations appear to us to be founded on *simple ideas*. Some of these are purely objects of intellect, exciting by themselves no feelings of pleasure or pain; such are the ideas of time, space, measure, &c. But others are necessarily associated with the simple feelings of pleasure or pain; and thus become (when habitually entertained) the active principles or motive powers, being referrible to the class of moral feelings, or to that of animal propensities, according to the *objects* to which the ideas relate. We cannot, however, see how a clear line of demarcation is to be drawn between these classes. We are accustomed to speak of the *selfish propensities* as differing in their character as well as in their object from the *benevolent feelings:* but we cannot see why amativeness or philoprogenitiveness may not be just as well called a *feeling*, and benevolence a *propensity*. Each is an *habitual idea*, which is dwelt upon and becomes a *motive* to the intellect, because associated with a pleasurable feeling; it is only because the object of the former idea is the gratification of self, and of the latter the happiness of others, that different places are assigned to them in the moral scale. Yet philoprogenitiveness may give rise to actions as disinterested as the purest benevolence; and benevolence may be so indulged at the expense of justice as to become nothing else than a refined selfishness. We consider, then, that such a distinction as the one just alluded to is altogether untenable as the foundation for a system of ethical philosophy; however convenient the terms moral feelings or sentiments, and animal propensities, may be as collective designations.

Now the Motive Powers which are brought to bear upon the Intellect, and which serve to govern the decisions of the Will, are as various as the different classes of objects which excite ideas in the mind; and they are continually liable, therefore, to come into antagonism with each other.

Hence the discussions which every man occasionally holds with himself as to the line of conduct which he shall pursue; his decision being formed according to the preponderance of this or that motive or combination of motives. This is universally admitted to be the nature of the mental operation which we perform, when we are discussing merely the *prudence* of a line of conduct; that is, when we are choosing between two or more lines of conduct into which the question of right and wrong does not enter. Thus we will suppose that we had arranged to take a ride at a certain hour, and the sky in the meantime becomes overcast, threatening us with a wetting if we persevere in our intention; the decision will be governed merely by our estimate of the probability of a shower, our desire to enjoy the anticipated pleasure, and our dislike of the threatened discomfort. But suppose that instead of a pleasure-ride, we are about to set out on a professional visit to a patient whose condition requires our aid; a new motive is introduced, which alters the aspect of the whole question, making our decision no longer one of prudence but of *morality*. Or if, instead of the benefit to another to be derived from our going, we introduce the risk to our own health which may be incurred by exposing ourselves to the rain; we then have another motive which gives the question a moral aspect. And if we suppose *both* these motives to be in action, we may have a very unpleasant conflict between our sense of duty to others and our desire to serve them, and our sense of duty to ourselves and our affection for those who may be immediately dependent upon us. Such a conflict, in regard to exposure to the severity of the weather, infection, or to risk of other kinds, has doubtless passed at some time or other in the mind of almost every one of our readers.

Now it is commonly supposed that, in all such cases, *conscience* or the *moral sense* should be the supreme guide, and that every other motive should be kept in subjection to its dictates. But the difficulty is to determine what conscience directs; and it is surprising to us that, in spite of the continual experience of such a difficulty, both metaphysicians and theologians, to say nothing of the so-called practical men or disciples of common sense, should continue to uphold the *autocratic* nature of this faculty. We have carefully examined the prevalent opinions upon this subject, and find none of them satisfactory. But we have recently met with an analysis of the supposed faculty by an anonymous critic, which perfectly accords with the views we had ourselves previously entertained, and which is so lucidly expressed that we cannot do better than present it nearly in its original form to our readers:*

"If moral good were a quality resident in each action, as whiteness in snow or sweetness in fruits, and if the moral faculty was our appointed instrument for detecting its presence, many consequences would ensue which are at variance with fact. The wide range of differences observable in the ethical judgments of men would not exist; and even if they did, could no more be reduced and modified by discussion than constitutional differences of hearing or of vision. And as the quality of moral good either must or must not exist in every important operation of the will, we should discern its presence or absence separately in each; and even though we never had the conception of more than one insulated action, we should be able to pronounce upon its character. This, however, we have plainly no power to do. Every moral judgment is *relative;* and involves a comparison

* See review of Dr. Whewell's Elements of Morality, in Prospective Review, November, 1845.

of (at least) two terms. When we praise what *has been* done, it is with the co-existent conception of *something else that might have been* done; and when we resolve on a course as *right*, it is to the exclusion of some other that is *wrong*. The fact that every ethical decision is a *preference*, an election of one act as higher than another, appears to us of fundamental importance."

We cannot, therefore, attach a moral character to the actions of animals that are performed under the direction of a blind undesigning instinct, which operates in them as the spring which moves an automaton, leaving them no choice between one course and another; nor can we say that a human action is in itself morally wrong as regards the individual, when it directly results from a violent impulse which he has no power to restrain. But suppose a hungry boy to give his dinner to a famishing suppliant, it is through the operation of pity or benevolence, which makes itself felt in his mind as possessing a higher claim to his attention than his own appetite. His own account of it, in the ordinary phraseology, would probably be that his conscience told him that it would be right to give away his dinner to those who wanted it more than himself. But we agree with the author from whom we have quoted, that, in such a case, pity speaks for itself, when confronted with appetite; and does not want a new power called moral faculty to speak for it. "If, indeed, it acted quite alone, without the presence and competition of any other principle,—if, for the time being, it occupied us wholly, like a solitary impulse, possessing a wild creature,—it would say nothing to us of its worth; but the instant it solicits us with a rival at its side, it reveals to us its relative excellence. And it is the irresistible sense we have, in this case, of its superiority, that is properly denoted by the word *conscience;* the *knowledge with ourselves*, not only of the fact, but of the quality, of our inward springs of action."

According to this view, then, what is termed conscience is nothing else than the *idea of right or wrong* character which becomes attached to an action, when we place in comparison the motives which prompted it; and this idea is entirely dependent upon the relative worth or value in the moral scale, which we have been accustomed to assign to the different classes of motives. The moral rule of action hence consists in the preference of a higher to a lower motive or combination of motives. But it will of course be asked, how are the relative values of these motives to be determined; and the answer is, simply, by the universal consciousness of mankind, which is found to be more and more accordant in this respect, the more faithfully it is interpreted, and the more fully the general mind is expanded and enlightened. It is this tendency towards universal agreement upon this fundamental point, which leads us to feel satisfied that there will in the end be as good a foundation for a science of morals in the psychical constitution of man, as there is for that of music in the pleasure which he derives from certain combinations of sounds. The decisions of the reason, founded upon observation and experience of the results of giving predominance to one or another motive, will aid in the estimation of their relative value; and such aid is most valuable as proving to us the accordance between the different parts of our internal nature and our external position. But it is on our *feeling*, not on our *reason*, that the fundamental axioms of moral science must be based; that feeling being the self-consciousness of the comparative worth of our different

springs of action, which guides the reason in the determination of the conduct, not as a distinct autocratic faculty, but as the preponderating result of the comparison which is involved in the very notion of right and wrong.

This is not the place to speak of conscience in its religious aspect; nor is there any necessity for so doing. The introduction of the promises or threatenings held out by Revelation as rewards for good or punishments for evil, simply operates by furnishing an additional set of motives to the determination of our conduct. The desire of the love of God and of the happiness resulting from it, and the fear of His displeasure with its awful consequences, act like any other desires and fears in deciding our will; and their relative force on any occasion will depend upon the degree in which they habitually influence the mind. The complete coincidence between the dictates of the Divine law and the highest principles of pure Morality, prevents one set of motives from ever being placed in antagonism with the other; and the contest generally takes place between these higher tendencies and those lower desires and passions which are in their unrestricted indulgence opposed alike to religion and morality. But there are a great many cases in which it is difficult to decide upon the right course, simply because it is difficult to strike the balance between contending motives; both sides including impulses whose claim to superiority, when acting against those of a low or selfish character, we should at once admit. Thus we have known persons in whose characters the love of *truth* and *justice* are the most prominent features, and who would shrink with horror from any violation of either of these principles for any selfish purpose, sorely perplexed when they have come into apparent collision; the motive to tell or to act a falsehood, being one in itself most just and praiseworthy, namely, the desire to secure the escape of a slave, or in some other way to protect a fellow-creature from oppression or suffering. Now, we do not pretend to decide upon the right course in such a case; we only adduce it in proof of our position, that there is no supreme dominating principle, either in morality or religion, that can invariably and indubitably indicate the rightness or wrongness of a particular action; the moral responsibility attaching to it being entirely determined by the character of the motives which led to the decision. Thus if a man, who might be urged to conceal a fugitive slave near the Canadian frontier, were to refuse to do so merely from the fear of unpleasant consequences to himself, he would be justly branded with the character of a cold-hearted coward; but if his refusal should proceed from the conviction that the divine law requires the preference of rigid truthfulness over every other motive, and that by concealing the suppliant he should be forced into a violation of that law, we cannot blame him, even if we believe that the law of compassion written on our own hearts is at least equally imperative. Similar difficulties beset the upholders of the non-resistance creed, which teaches that *love* is the all-powerful principle in the moral world, and that it should entirely supersede all those impulses of our nature which lead us to oppose force to force, and to resist an unjust and unprovoked assault. Now, here again, we might readily understand and sympathise with those, who consider that the fear of personal suffering does not warrant our doing a severe injury to another in warding off a threatened attack; but when the ques-

tion comes to be, not of *self*-defence, but of protection to *others* who are helpless dependents on our succour, we feel that the comparative nobility of the latter motive warrants actions which our individual peril might scarcely justify. And in justification of such a course, it might be urged to those who think themselves bound by the Divine injunction "resist not evil," that the context shows that such resistance had reference to individual suffering only, and not to evils affecting others whom the strongest feelings of humanity impel us to guard and protect.

If we are correct in these views, it must be admitted as a general principle, that the ethical judgments of men should be guided, not by any fixed and determined standard of right and wrong applied to *actions*, but by the balance of the *motives* which have led to the actions in question. Under ordinary circumstances it will be easy to strike this balance, when the true motives can be ascertained; from the concurrence of all men, whose minds have been expanded by intellectual and moral cultivation, in their estimate of their relative value. Thus we cannot imagine the circumstances which should induce a woman to sacrifice her chastity, even when the motive is to save the life of one most dear to her. And it is difficult, if not impossible, to put a case in which the violation of truth or justice shall be *clearly* demanded by duty. But notwithstanding the universal horror with which the crime of murder is regarded by all but the most degraded tribes of our race, it is not difficult to conceive the circumstances in which it loses its criminal character and even become justifiable; thus the desire to rid the world of a sanguinary tyrant, at the severest personal risk, has called forth actions which have been in all ages admitted to be noble in their character, even by those who would regard them as prompted by a mistaken principle of duty; and there are few who cannot sympathise with the stern resolve of a Virginius, when paternal affection led him to sacrifice his daughter's life to preserve her honour.

It must be evident, however, that in the construction of laws, the *actions* rather than the *motives* must constitute the foundation of criminal responsibility. It would be impossible for any human power to search the heart in the way that is open to the Divine Being alone; and in by far the greater number of criminal actions, the real motives are very much what they *primâ facie* appear to be. Consequently, in fixing an average of punishment, and leaving it to the administrators of the law to modify or rescind that punishment, in cases in which it can be clearly proved that the motives were of a higher character than the law presumes, enlightened governments adapt their system to the requirements of justice as much as is probably feasible.

Before, however, we proceed to apply these views to the question before us—that of the Criminal Responsibility of Lunatics,—we must offer a few remarks upon the ends of punishment. On this subject a very considerable change of opinion has been gradually taking place in the public mind; and we believe that we are entitled to affirm, that the great majority of those who have duly considered the subject have entirely abandoned the notion that society has any right to inflict punishment as an act of simple retribution—or more properly vengeance—for the offence committed; and are now willing to admit the sole ends of punishment to be the reformation of the offender and the prevention of crime in others.

Some indeed go so far as to assert that these two ends are identical, and that the latter may be disregarded altogether; but while we admit this to a certain extent, in the case of juvenile offenders more especially, we cannot but think that those who uphold the reformation of the offender to be the sole end of punishment, do not adequately estimate the force of the fear of punishment as a motive in determining the conduct of a large class who cannot be influenced and kept in check by higher considerations. Let any one concerned in the education of children attempt to dispense altogether with this motive, and we are very sure that he will find it impracticable to obtain universal obedience, even though he may succeed well with certain natures. Now the great mass of criminals are but "children of a larger growth;" having the higher moral and intellectual nature undeveloped or at least dormant, and acting only with reference to the present gratification of their lower propensities and baser feelings. And there are many more of such, we are well convinced, who are only prevented from becoming criminals by the fear of judicial punishment. It may be said that, where the desire exists, but is thus prevented from exercising itself in action, there is as much moral criminality as if the offence were actually perpetrated; and this must be admitted. But it does not hence follow that the fear of punishment is useless; for if it prevents the commission of meditated crime, society is a great and direct gainer; and it is a much greater indirect gainer, inasmuch as, according to universal experience, the first step in the actual execution of criminal purpose is the almost certain prelude to a downward career of constantly increasing wickedness.

We shall, then, regard the reformation of the offender, and the prevention of crime, as the two great ends of punishment, which are to be made coincident as much as circumstances will admit. And we believe that in the great majority of cases, the latter purpose may be attained without any real interference with the former; a slight modification in the means which experience shows to be most effectual in the reformation of the actual criminal, being frequently sufficient to give the treatment a sufficiently repulsive aspect in the eyes of the would-be criminal, to serve as an efficient motive in deterring him from violation of the law.

Now if it be admitted that there is no fixed and invariable standard of right and wrong, even amongst persons of the highest moral and religious cultivation, who acknowledge the same fundamental principles of ethics and theology, and who recognise the same guides in their application,—and this all experience shows to be the case—it must be further admitted that the standard of right and wrong is subject to much greater variations among those who have never had the higher part of their moral nature properly developed by cultivation, or who have, from original constitution, a want of power to feel the higher class of motives, or who are led by disorder of their intellectual faculties to form a wrong estimate of the respective values of the different impulses which act upon them.

Although the Oliver Twist of Dickens and the Fleur de Marie of Eugène Sue can scarcely be regarded as *impossible* characters, yet it is in the highest degree *improbable* that children brought up in nurseries of vice should remain so free from the most grievous moral pollution as these authors would represent. The moral faculties require no less education than the intellectual; and the early repression of all the higher feelings,

and the influences constantly at work to develop the lower, can scarcely do otherwise than fix the standard of right and wrong at a very different point from that which a better education would have determined in the same individual. The young "prig" is led to consider the undiscovered abstraction of a watch as the most creditable action he can perform; and to look at the criminal who "dies game" on the scaffold as his noblest ideal of humanity. "It is absurd," said a superintendent of police, "to talk about the effects of *no* education; for if *you* do not educate these children, the devil will." And if society leaves any portion of the rising generation to this devil's education, what else can be expected than that they should become a pest to the remainder; or how can those beings be regarded as criminally responsible who never knew that they had souls to be saved, or have never experienced such treatment from others, as to have any of the higher moral feelings aroused in their hearts? For such, the duty of the state in our apprehension is most clear; for these offenders against its laws cannot be regarded as subjects of criminal punishment, if they have no consciousness of moral responsibility; and the education which they are receiving, in which the braving of punishment is made to constitute the highest credit, tends to prevent the fear of legal penalties from having any operation in deterring from crime. We look, therefore, to judiciously devised plans for the reformation of juvenile offenders, as amongst the most important means of social amelioration. We believe that the instances will be found to be comparatively few, in which the higher moral feelings are incapable of being called forth by appropriate means, and in which the reason may not be trained into the habitual regard of them as the dominant motives to action. In cases in which, from long repression, or from original peculiarity of mental constitution, these higher feelings are found incapable of being thus developed, and the lower passions and propensities exert themselves without control, we can scarcely regard the individual as a free agent; his condition is more akin to that of brutes; and society has certainly a right to restrain him from the indulgence of his appetites and passions in a way which shall be prejudicial to others, whilst at the same time he has a right to such treatment from society as shall be curative as well as restrictive.

This is the state to which Dr. Mayo has drawn attention in his 'Elements of the Pathology of the Human Mind,' (1838); and it is not inappropriately termed by him *brutality*. He regards it as characterized by the original deficiency or the abolition of the moral sense; and distinguishes it thereby from insanity, in which he considers that there must be intellectual perversion. We cannot agree with him in his definitions; because we do not recognise the moral sense as a single faculty or element, but rather as the collective result of all the higher moral tendencies; and because, moreover, we by no means think that the term insanity ought to be restricted to cases of intellectual perversion. But as to the general features of the condition itself, and the light in which the actions of the individual should be viewed, we are quite in accordance with him. The case of Lord Ferrers affords a striking example of it:

"That nobleman was not *insane* in any customary use of the word; his intellectual faculties were good; and they were directed by a powerful will towards definite objects; neither did he exhibit that moral incoherency which we have described among the earliest phenomena of the insane state. The business-like

talents, indeed, which he displayed in his own defence, indisposed his judges to allow him the advantage of *that* plea. But his brutality made him unfit for *social* existence; the laws of this country did not reach him as a subject for confinement. Therefore he was hanged. This procedure was unavoidable under the circumstances of the case, and in the present state of our laws; but it constitutes a painful fact, considering that education at present affords no preventive to such criminality." (p. 132.)

In the Appendix to the same work, Dr. Mayo relates an interesting case, in which he was enabled to carry out his views of the proper treatment of this condition; and we shall quote the description of it, the truthfulness of which will doubtless be recognised by many of our readers from their own knowledge of similar unpleasing specimens of humanity.

"N. B., aged 16, was described to me by his father, who came to consult me in regard to his management, as a boy of singularly unruly and intractable character, selfish, wayward, violent without ground or motive, and liable under the paroxysm of his moodiness to do personal mischief to others; not, however, of a physically bold character. He is of a fair understanding, and exhibits considerable acuteness in sophistical apologies for his wayward conduct. He has made little progress in any kind of study. His fancy is vivid, supplying him profusely with sarcastic imagery. He has been subjected at different times, and equally without effect, to a firmly mild and to a rigid discipline. In the course of these measures, solitary confinement has been tried; but to this he was impassive. It produced no effect.—He was last in a very good seminary in a town in —, where he drew a knife upon one of the officers of the establishment, while admonishing him; and produced a deep feeling of aversion in the minds of his companions, by the undisguised pleasure which he showed at some bloodshed which took place in this town during the disturbances of 18—. He has not appeared to be sensually disposed, and he is careful of property. His bodily health is good, and he has never had any cerebral affection. This boy was, further, described to me as progressively becoming worse in his conduct, and more savagely violent to his relatives. Still I easily discovered that he was unfavorably situated; for his relations appeared to be at once irritable and affectionate; and the total failure of various plans of education was throwing him entirely upon their hands. As an instance of the miserable pleasure which he took in exciting disgust and pain, I was told that, when 13 years old, he stripped himself naked and exposed himself to his sisters." (p. 173.)

It is evident that in this case there was a deficiency of those higher moral feelings, which may usually, by a judicious course of education, be made to keep the lower impulses in check; and among these impulses we think ought to be classed a positive pleasure in giving pain to others, which is the exact opposite of benevolence. This is surely as elementary a tendency as benevolence itself; and is by no means sufficiently accounted for by a deficiency of that disposition and a predominance of destructiveness or other similar propensities. Of the uncontrolled action of this malevolence, in combination with other selfish propensities, we have a most characteristic example in one of the fictitious personages of Mr. Dickens's creation, namely, the diabolical Quilp, the undiluted malevolence of whose nature, perhaps a little overdrawn, manifests itself in almost every action recorded by this veracious biographer; who has not acted with any inconsistency in endowing him with remarkable power of intellect. Such a disposition, of which almost every one may recognise a trace in himself in certain conditions of his system, is peculiarly strengthened by early indulgence; and in a large proportion of the cases in which it is strongly manifested, we believe that it might have been repressed by

judicious education. This, however, had not been accomplished in Dr. Mayo's case; and the question had become a very difficult one in what manner the subject of it should be treated. Dr. Mayo recommended that he should be placed under physical restraint; coercion being employed in order to induce him to comply with reasonable requests, and to teach him the desirableness and superior value of self-control; and praise and encouragement being given, as his efforts to acquire it became more successful. Such discipline, as Dr. Mayo justly remarks, should be perfectly free from violence; but should possess that quiet and apparently irresistible force which may give it the passionless character of a law of nature. In the case in question, it was attained by confinement within a private lunatic asylum, the proprietors of which undertook to carry out Dr. Mayo's views; and as the visiting magistrates interposed no obstacle, the experiment was fairly tried, and with remarkable success. It is a curious feature in the conduct of the youth, that he received the magistrates in his apartment without expressing any dissatisfaction with the place, or making the slightest allusion to his *confinement* there: but how far pride contributed to this reserve, or how far the imposing character of the restraint to which he was subjected had impressed him with the idea that to struggle against it would be useless and that patience was his best policy, Dr. Mayo was unable to determine. We should like to know something of the subsequent history of this young man; the immediate result of the discipline was most gratifying, in strengthening his power of restraining himself, and leading him to feel the pleasure of living in an orderly and regulated manner, so that an occasional waywardness was the only remaining symptom of his former condition.

It is much to be desired that establishments could be expressly formed for the reception of such young persons as exhibit the indications of this brutality; whether it arise from early mismanagement or original peculiarity of moral constitution. At present the law requires that they should become qualified by the actual commission of crime for the discipline of a penitentiary; and we have a large number of persons annually arraigned as criminals, who can scarcely be regarded as responsible beings. A great proportion of those who have been remarkable for a long and reckless career of crime seem to belong to this class; but it must be borne carefully in mind, in all attempts to determine criminal responsibility, that the question of departure from that normal or standard condition, in which the higher moral tendencies keep the lower propensities and passions in complete control, is one of *degree* only. We have all of us our besetting sins, of which the world may know little enough, even the closest observer of our external conduct failing to discern their indications; and whatever may be the particular nature of each, it consists in a temporary predominance of some lower motive over a higher, and its permitted exercise upon the will. An action which all regard as criminal results from nothing else; the still lower character of the motive or combination of motives which prompted it, and its more complete opposition to the higher class of feelings, constituting the sole moral difference. On looking over the collection of narratives of remarkable criminal trials, lately translated from the German of Von Feuerbach, we have been forcibly struck with the extreme triviality of the motives which have led to the perpetration of the most terrible crimes; and this not merely singly, but in long succession.

Thus we find Andrew Bichel,—a man not given to the indulgence of violent passions, neither a drunkard nor a gambler, nor quarrelsome, living on excellent terms with his wife, and having a reputation for piety, though at the same time known to be a sneaking pilferer,—killing one young woman after another whom he had decoyed to his house; and for what? Not, as might have been supposed, for the gratification of his lust, that tyrant passion which is the provocative of so many crimes; but merely for the sake of the clothes they wore, which, from their position in life, could be rarely worth more than a few shillings. His master-passion seems to have been avarice; but he was restrained from the indulgence of it by the cowardly disposition which led him to shrink from risk; and it was only when opportunities seemed to present themselves for gratifying it in secret, at the expense of the weak and helpless, that he carried his criminal desires into execution. In describing the murder of one of his victims, he says—"I had a desire to see how she was made inwardly, and for this purpose I took a wedge, which I placed upon her breast-bone, and struck it with a cobbler's hammer, I thus opened her chest, and cut through the fleshy parts of her body with a knife;" and all this, as he subsequently acknowledges, before his victim was actually dead. We can scarcely fancy that a man who could thus commit one murder after another for the sake of a few shillings or pence, which he acknowledges that he did not want, and who could coolly gratify his curiosity about the conformation of the heart and lungs upon the yet living body of the poor girl whom he had just stabbed, after decoying her to his house under the pretence of showing her a magic mirror, could have been endowed with the ordinary feelings of humanity, in a degree that should lead us to regard him as criminally responsible, or at least as a fit subject (so far as he was individually concerned) for the same kind of punishment as that inflicted upon criminals who have received a much higher moral cultivation. The case seems to us to have been one of sheer *brutality;* for though the wretch exhibited a show of piety, it seems merely to have been a thin disguise to hide his real grovelling sordid soul; and his weak will, in the absence of higher motives, must have been completely subjected to the tyranny of a desire which had not in itself strength enough to brave the fear of punishment.

This case, we think a very instructive one; for whilst it leads us to question the justice of the system which inflicts the punishment of death upon offenders who can hardly be considered responsible beings, it also leads us to see the importance, as regards society, of not allowing them to pass scot free on the plea of insanity, and giving to the subsequent measures a character of *mere* salutary restraint. The fear of *punishment* operated as a powerful motive in restraining Bichel from the gratification of his avaricious desires. Had he been a man of more powerful intellect, he might have contrived means for exercising his propensity, without bringing himself within the grasp of the law. But his low cunning merely enabled him to contrive means by which he hoped to elude discovery; and if he had been certain that discovery would have succeeded, his crimes would have never been committed. We fully agree with Dr. Mayo, that "the extent to which the understanding of persons even partially insane may be made instrumental to their self-management, by supplying motives and fears, under dread of punishment, is not at present

appreciated." And we believe that the tendency of an enlightened system of criminal legislation, based upon a correct psychology, will be to break down the barrier which at present stands between those who are held excused from all responsibility on the ground of insanity, and those who are deemed fit subjects for the extreme penalty of the law. On the one hand, the treatment of those recognised as criminal lunatics, may be so conducted as to deter from the imitation of their example, instead of suggesting and even encouraging it, as it has frequently done. On the other hand, the treatment of criminals reputed to be sane, will approximate to that which is found most efficacious in the cure of madness. In this manner the courts will be relieved from some of the most trying questions which at present come before them; and the ends of justice will be far more completely satisfied. For example, why should not an insane ward be established in our chief prisons, instead of a criminal ward in our lunatic hospitals? At present, as Dr. Mayo well remarks, in regard to many of the criminal lunatics confined in Bethlem Hospital, "the enduring confinement is in itself a dreadful punishment. But in these cases there is a sad expenditure of unfruitful suffering; for the confinement being entirely and sedulously deprived of the character of a punishment operates unpreventively." And the preventive influence of the restraint is of course altogether lost, when the criminal, being regarded as cured of his insanity, is let loose again upon the world. Dr. Mayo refers to a case noticed in the 'Examiner' for June, 1838; in which the captain of an East Indiaman, who had murdered his crew of eight men in cold blood, but who was excused from capital punishment on the plea of insanity, and placed in the Cork Lunatic Asylum, was subsequently let loose upon the world again. We trust that such may not be the ultimate issue of the trial of Captain Johnson, which most of our readers will remember, who was convicted not long since of the murder of several of his crew, but who was sheltered from punishment by the plea of insanity—a plea chiefly sustained by the extraordinary atrocity of his conduct. Of this individual Dr. Mayo remarks in his visit to Bethlem:—"Captain Johnson's cold, close, subacid visage, and quiet gentle demeanour is awfully contrasted in our recollection with the demoniacal violence which drink and fear, operating upon cruelty, could produce in him, without the smallest warranty of insanity." In the justice of this view of the case we fully coincide. There seems to have been in this man's mental constitution much of the malevolence and vindictiveness which have been already noticed as the chief positive characters of the condition defined by Dr. Mayo as brutality; these were unrestrained by any higher motives; but on the other hand they were fearfully aggravated by habits of intoxication, which not only rendered him most furiously savage whilst under the excitement of alcohol, but doubtless increased the apprehension of personal danger, which he seems to have entertained in the intervals of his temporary madness. We have no doubt that if the fear of punishment could have been duly kept before his eyes, it would have operated as a sufficiently potent motive to self-restraint; but his whole conduct exhibits a coolly-devised scheme to take advantage of the protection which the law most abundantly extends to the captains of ships at sea, for the sake of maintaining the subordination of their crews; and he obviously hoped to be beforehand with his men, by instituting a charge of mutiny against them,

which fell to the ground, however, as soon as it became apparent from the concurrent testimony of the passengers and crew, that the captain was the first aggressor. We do not think, as Dr. Mayo seems to do, that this last circumstance "should be viewed as the indication of cunning wickedness, and inconsistent with the supposition of lunacy;" because we believe that in minds decidedly insane, in the ordinary acceptation of the term, very coherent trains of reasoning may be carried out, and a variety of ingenious devices practised, for the sake of gaining a particular end,—especially where that end is the escape from consequences unpleasant to the lunatic. But we do maintain that an enlightened and unfettered consideration of such a case as this, apart from the morbid sympathy for great criminals which has of late been so strangely manifested by the public, would lead to the conclusion that the prisoner was a most fitting subject for the severest punishment which could be inflicted consistently with reformatory discipline; and that this punishment should be of a kind to act as a motive in deterring others from similar conduct. The coincidence between the two cases to which we have last alluded is most striking; and it speaks powerfully of the fearful risk which is involved in the commission of such powers as those which are intrusted to the captains of ships, to individuals in whom the lower class of motives is predominant. The greater the temptation to the abuse of these powers, the more severe and certain should be the penalty for the misemployment of them; and even if an occasional injustice should be done to the individual, by punishing him as if he had possessed entire control over himself, it must be borne in mind that if the discipline, however severe, be curative or reformatory, no permanent injury will be done to him, and society will be the gainer in proportion as the punishment acts as a preventive to crime in others.

Our readers will have seen that we have hitherto avoided all allusion to the question of death-punishment in such cases; but we may now briefly explain our views upon this subject. We do not hold with those who maintain that society has *no right* to take the life of any individual; for if it were demonstrated that capital punishment had more influence than any other infliction in deterring from crimes of certain classes, we should feel justified in upholding it, even though we must thus abandon all idea of reforming the offender. We could be content to leave the fate of the criminal in another world to that Being who searcheth the hearts of men. But from the best evidence we have been able to collect, the punishment of death *has not* this deterring influence; and since the feeling of the intelligent middle classes has been strongly excited against it, so many instances have occurred in which the slightest deficiencies of evidence have been regarded as sufficient grounds for the entire acquittal of the prisoners, or the slightest suspicions or even suggestions of insanity have been held to shield them from responsibility, that the punishment has lost much of its former certainty, and the fear of it necessarily becomes far less operative as a motive for the prevention of crime. We could enumerate many instances of this kind; but they would lead us too far away from our present inquiry. The abolition of death-punishments would be, in our minds, a great step towards that improvement in our criminal legislation to which we look forward;—the treatment of all criminals to a certain extent as lunatics, that is, the devising of a system of discipline in which the reformation of the offender (in other words, his restoration to a well-

governed state of mind) shall be a no less prominent object than the prevention of crime in others;—and, on the other hand, the treatment of all such as are at present excused from responsibility on the ground of lunacy, as criminals, that is, carrying on the discipline necessary for their restoration to mental health in such a manner as to have the character of a punishment. We are well aware that there are vast difficulties in the way of such a scheme as we propose. Old systems and prejudices have too long interposed barriers to all improvement; and opportunity has scarcely yet been afforded for estimating what may be reasonably anticipated from a change of method,—still less, for determining what are the kinds of punishment which most completely unite the requisites we have indicated. But we are Utopian enough to have an almost unlimited confidence in the efficacy of education,—using that term in its most comprehensive sense,—as regards the prevention of the first crime; and in the efficacy of an improved method of prison discipline as regards the reformation of the offender, and the consequent prevention of future offences by the same individual. It must not be forgotten that the proportion of criminals to offences is really a small one; a very large number of those who have been once committed to prison being recommitted within a twelvemonth after their discharge; and the periods of confinement and liberation henceforth alternating, until some graver offence brings the criminal under the heavier penalties of the law. Even if the reformation of the offender, therefore, could only be accomplished by a system of discipline so mild as not to produce its due restraining influence upon the would-be criminals, we cannot doubt the beneficial effect of such an influence upon society at large. But we see no impossibility in the combination of the two objects; although experience alone can decide in what manner this may be best effected.

Having thus expressed our present views on the much-agitated question of the treatment of criminal lunatics, we may briefly return to some of those more abstract considerations regarding the state of mind that leads to criminal actions, with which we started in the present inquiry; since we wish not to lose the opportunity of recording what appear to us to be the real relations of insanity to crime. As we have already remarked, the emotions, propensities, affections, &c., appear to us all capable of being ranked in the same category; being composed of ideas, with which the simple feelings of pleasure and pain are linked; and being different from each other only in regard to the nature of the objects to which those ideas respectively relate. Any of them, when no longer ranging over the *abstract*, but excited in reference to some *particular* object, constitutes a *desire;* and this desire may be so violent as to constitute a *passion*. The proper province of the desires is to operate upon the intellectual processes, setting these to work to devise the means for their gratification; and the greater part of the actions of our lives are the consequence of motives thus originating in our desires, though they may appear to have been worked out as matters of simple calculation by our reason. But the will is not only concerned in carrying into effect the suggestions of the desires. In the well-regulated mind it ought to have a controlling influence over the desires themselves; so as to prevent them from exercising themselves with undue force. This is, in fact, the power known as self-control; a power which cannot be too early cultivated, or too habitually exercised.

Now we believe that much may be learned by observation of infantile life of the nature of this power. When a young child gives way to a fit of passion, the nurse attempts to restore its equanimity by presenting some new object to its attention, so that the more recent and vivid pleasurable impression may efface the sense of past uneasiness. As the child grows older, the judicious mother teaches it self-control, by calling up in its mind such motives as it is capable of appreciating; the act of self-control being the result of the overpowering influence of the higher motives suggested to it, over the lower or selfish emotions which we desire to bring into subjection. For a time this process needs to be continually repeated; but after a time, a very slight suggestion serves to recall the superior motives to the conflict; and in a further space, under judicious training, they present themselves spontaneously, whenever the evil spirit begins to rise within. We believe it to be a principle recognised by the best educators, that the development of this power of self-control ought to be the object of all nursery discipline; and that appeal should always be made to the *highest* motives which the child is capable of understanding, —punishment being only had recourse to, for the purpose of supplying an additional set of motives where all others fail, and never vindictively inflicted. Now in the exercise of self-control, when once it has been acquired, we believe that the process will always prove to be this:—that the will by a peculiar effort represses the vehemence of one class of motives, by forcibly withdrawing the attention from them, and directing it to another of a higher character. We doubt much the operation of *pure reason* in such a case; since we cannot see how any reasoning can have cogency in repressing a passionate desire, unless it is backed by another motive of the same kind. Thus a man may be deterred from the commission of a crime, by reasoning upon the consequences of the discovery of it with respect to his own worldly position, or to the degradation in which it will involve his family. But he must have a *desire* of the world's approbation, or an *affection* for his family, to give such considerations any weight in his mind. The mind thus swayed hither and thither by various motives contending for the mastery, is at last decided by those which present themselves most forcibly before it; and it is in keeping some in the background and bringing others into clearer view, that the power of the will seems to us to be exerted in modifying the decision,—rather than in simply opposing *reason* to *passion*, which is the ordinary idea of the process of self-control. The reasoning process must always be based upon *motives;* and these motives arise out of the desires, affections, &c., themselves.

But the habit of self-control, unless it is under the guidance of ingenuous impulses, is not unmixedly good. It may be turned to purposes of the deepest dissimulation. Its real operation, however, is the same,—the dominance of one set of motives which the will calls into strength, over another which would have otherwise operated without any restraint to produce an opposite result.

Most crimes result, however, from the undue predominance of some one motive or set of motives (almost invariably those arising out of the selfish desires), and from an insufficient exercise of the regulating power, in keeping these motives in subjection, and in bringing up others of a higher character within view of the mind. From want of education, or

from natural inaptitude, there may not be a power of appreciating the force of these higher motives, and the will cannot therefore be expected to decide in their favour. On the other hand we have too many instances of atrocious crimes committed by men who had enjoyed every advantage of education, who had made great moral and religious professions, and who had manifested no deficiency in self-control on all ordinary occasions. The recent murder of Sarah Hart, by John Tawell, is doubtless fresh in the memory of our readers. Now in most of these cases we believe that the *habitual dominance* of the motive which led to the crime may be traced in the previous conduct; and that the will had thus lost much of its controlling power. Thus it is obvious in Tawell's case that desire to stand well with the world by the concealment of his sensual habits, was the immediate cause of the murder of his mistress; and the influence of this desire is obvious in his preceding history, having been the mainspring of many of those actions which the world set down as results of benevolence. Such cases seem to us to border very closely upon insanity; the power of self-control having been gradually lost *quoad* the particular tendencies which at last produced the crime. But even if such a degree of mental disorder were admitted to exist, it could not be held as a sufficient plea in mitigation of punishments, since its *corrective* purpose remains in full force, and its *preventive* object must be equally kept in view, in order that others may not be encouraged in the habitual gratification of their lower desires by the hope of future impunity. So with regard to those paroxysms of anger or of lust, which have led to murders, rapes, &c. in open day and within sight of those who could witness, though they might be too far to prevent, the perpetration of the crime, we believe that they may usually be traced to an *habitual* want of self-control; and that although the individual at the time of committing the crime was so completely under the dominance of passion as scarcely to deserve the name of a responsible agent, he is a proper subject of punishment on account of his *previous* neglect of self-restraint. To treat such a one *merely* as a lunatic, and to absolve him altogether from criminal responsibility, seems to us to be a needless throwing-away of the motives afforded by the dread of punishment to that self-control which *may* be exercised when it is called into action at the right time. Tell a man who is subject to fits of dangerous and really insane passion, that he will be held criminally responsible for whatever injuries he may do to others in his paroxysms, and he will have an additional inducement to endeavour to dispel the storm at its first rising. But let him feel assured that he will only have to lead a gentlemanly quiet life within a lunatic asylum, should he happen to commit murder in one of his outbreaks, and he will have little motive to control himself. The homicidal monomania, as it has been called, passes by such fine gradations into a naturally violent and unrestrained temper, that we cannot see where sanity really ends and insanity begins; and the same may be said of many other mischievous propensities. Thus one of the inmates of the criminal ward at Bethlem is a young woman, who, whilst a nurse in a family at Greenwich, killed the infant which she nursed, and to which she was much attached, under the influence of a wayward and reckless state of mind brought on by a trifling disappointment relating to an article of dress,—just as a fanciful child would break a favorite doll in mere spleen. We have recently heard of a case in

which a girl set fire to her master's house no less than six times in one day, without any assignable motive; an act fully as insane as the preceding. And yet to excuse such criminals from punishment, and to subject them merely to restraint, seems to us in complete discordance with the principles upon which the educator and the medical man alike act, when they seek for motives to induce these hysterical subjects to exercise self-control over their wilful and mischievous tendencies.

It is perfectly clear, we think, that in the class of cases on which we have been dwelling, nothing can be more absurd than to set up the offender's knowledge or ignorance of right and wrong, at the time of the commission of the deed, as a test of his criminal responsibility. The clearest knowledge of what is right is not capable of producing abstinence from evil, in individuals in whom the motives supplied by the obligations of moral and religious duty have been habitually neglected, and in whom the baser passions have acquired almost undisturbed sway. On the other hand, individuals in whom the sense of right and wrong has never been sufficiently developed to exert a salutary control over their conduct, may be (in our apprehension) treated in some sort as subjects of criminal responsibility; since the fear of punishment must be held out as a motive to the avoidance of crime on the part of those who are not yet capable of being influenced by any higher motives. But then, let it be remembered, we would have *all* punishments reformatory in its tendency. The impossibility of holding by this standard becomes the more apparent, in proportion to the extent of the survey we take of the different circumstances under which crimes are committed, and of the different moral standards which prevail amongst different communities. Thus the Connaught peasant regards himself as fully justified in ridding himself of a tyrannical landlord by the agency of a musket-bullet; and holds himself bound to shelter and defend any one else who has committed the offence. That the law is against him he knows full well; but *his* principle of patriotic duty leads him to defy the rule of the Saxon, in whatever way it may be exercised; and we do not see how he can be said to do more violence to his sense of right and wrong, than the Briton who deems it right to defend his castle, *vi et armis*, against the midnight robber. Who could feel sure that Balfour of Burley did not conscientiously believe that he was doing God service in the murder of Archbishop Sharpe; or that Charlotte Corday was not animated by so noble a motive, that she has a claim to be regarded as acting up to her notions of right, when she plunged her dagger into the heart of Marat?

Surely all such considerations tend to break down any fancied barrier that the ingenuity of man can devise between sanity and lunacy, so far as relates to criminal responsibility. No general test can be applicable to all cases. In the great majority of the crimes committed at the present day, it is true, the question of insanity is not raised, and needs not to be, if the views we have expressed of the objects of punishment are carried out, as the present administration seems endeavouring to do. Yet if the *vindictive* system of punishment remained in force, we should unhesitatingly raise the plea of insanity in regard to by far the greater proportion of offenders brought to trial in this country; on the ground that their education has been such as to draw forth their worst passions instead of their purer feelings,—that their sense of right and wrong is

formed upon a standard so opposite to ours that they had much better have none at all,—and that their intellect, having been exercised only in the gratification of the most vicious desires, is not capable of being directed in any other channel, until an entirely new set of motives is aroused for its exercise. How could a child born in the garrets of St. Giles's, the offspring of a prostitute and a thief, who is brought up to his father's profession, and follows it with a dexterity which he regards as highly creditable, be placed in the same category with the son of moral and religious parents, who has been educated in the midst of domestic happiness, has been led to feel every conceivable motive for perseverance in the path of rectitude, and has all his reasonable wants gratified,—but who steals or forges to gratify some low sensual taste, which has grown into a dominant motive from the licence accorded to it? Surely if society feels it right to execute *vengeance* on the criminal, the *latter* is the one on whom it should fall; for what right has society to *punish* for dereliction of duty the criminal who has never received at its hands the opportunity of acquiring any higher standard than that to which he believes himself to have acted up?

Again repeating that all the difficulties of the subject appear to us to be removed by a due appreciation of the ends of punishment and a proper adjustment of it to the two great purposes of the prevention of crime and the reformation of the offender, we leave the subject for the present in the hands of our readers.

Art. XVII.

1. *Rapport à l'Académie Royale de Médecine sur la Peste et les Quarantaines fait au nom d'une Commission.* Par M. le Dr. Prus. *Accompagné de Pièces et Documents, et suivi de la Discussion dans le sein de l'Académie.—Paris,* 1846. 8vo, pp. 1050.
2. *A Treatise on the Plague, more especially on the Police Management of that Disease, illustrated by the Plan of Operations successfully carried into Effect in the late Plague of Corfu; with Hints on Quarantine.* By A. White, M.D., Deputy Inspector of Military Hospitals, late Superintendent of the Plague in Corfu, &c.—*London,* 1846. 8vo, pp. 342.
3. *Correspondence respecting the Quarantine Laws, since the Correspondence last presented to Parliament. Presented by Command to the House of Commons, in pursuance of their Address of May* 19, 1846.—*London.* Folio, pp. 48.

In October, 1843, and January, 1845, we drew the attention of our readers to the subject of the quarantine laws in their relation to plague. The appearance of the works whose titles we have transcribed, and recent changes in the quarantine regulations of the Mediterranean and English ports, render it advisable to give some account of what has been done since we wrote on this question. It will be well, however, first to give a short recapitulation of the leading views we advanced, and point out how they are affected by the facts and arguments in the works now before us.

In our first article we entered particularly into the inquiry, "How is plague propagated?" and by deductions from facts selected from the

works of authors of the most opposite opinions, and particularly from facts observed in 1813, at Malta, proved that the disease is propagated by contagion; that while *actual contact* with persons affected with plague is not necessary for its propagation, still as their exhalations are not injurious at any considerable distance, a moderate dilution with atmospheric air appearing to render them innocuous, *close approach* is necessary—that the question of propagation of plague by clothes or merchandise was still unsettled—that although the exact period of latency of the disease in the human system could not be accurately defined, all credible evidence tended to prove that it never exceeded ten days; and, lastly, that certain unknown terrestrial or atmospheric conditions are necessary for the extension of the disease as an epidemic, just as certain conditions of air, moisture, and temperature are necessary to the process of fermentation, we might have added as perhaps a more apt illustration, or certain qualities of soil and climate to the germination of plants. The necessary conditions of air, moisture, and temperature may be present, but in the absence of the fermenting principle, no fermentation follows, and on the other hand without these conditions the fermenting principle is inert. Just so with vegetation: a field may be in every way fitted for the growth of a crop, the season and climate may be favorable, but unless the seed be sown no harvest is expected; while if the seed be sown in an ungenial soil or climate, or out of its appointed season, it is equally unproductive. It would be just as reasonable to deny the propagation of plants by the germination of their seeds, because each has its peculiar season, soil, and climate, as to deny the propagation of plague by contagion, because its influence has similar limitations.

We gave reasons for believing that the origin of the plague is now invariably in Lower Egypt, and that a proper system of drainage, sepulture, burial of dead animals, improvement in the dwellings and customs of the people, and proper segregation of every case of plague as it arises, would gradually extirpate the disease as they have done in parts of Turkey, Russia, Greece, Asia Minor, Syria, and Barbary, where it was formerly considered indigenous or endemic. We further showed that plague had never been brought to any European lazaretto except in ships coming from places where it was raging at the time of their departure, and argued from this fact that quarantine should only apply to ships arriving from places so circumstanced. In our second article we brought forward other facts strongly confirming these views, and entered upon a question which has since been warmly agitated,—the possibility of eradicating or totally destroying the plague,—and gave our reasons for believing that the seeds of contagion were preserved in Egypt in certain places where sporadic cases occur throughout the year, by the peculiar manner of burying the dead. We showed that the receptacles for the dead, are in all probability nurseries of the disease, and advocated their destruction with the introduction of a new mode of sepulture.

It will be seen that the views we advanced were not those of others. Writers had been previously divided into two classes, contagionists and non-contagionists, the former advocating a continuance of quarantine regulations, the latter their absolute repeal. We, on the other hand, argue that the non-contagionists are the real supporters of these regulations, for it is owing to their advice that the plague is still existing in

Egypt, while the contagionist is the true enemy of useless quarantine laws, reducing them to the limits of utility and real necessity, and labouring to destroy the disease upon which their existence depends. Large extracts from our articles were translated into the journals of Malta, Marseilles, Athens, Smyrna, and Constantinople, and we have since seen that our views have been generally adopted by those journals; further than this, Captain Carolani, a most intelligent health-officer of Malta, the captain of the lazaretto there, previously urged them upon Mehemet Ali in a personal interview. We should be sorry to assume any unmerited credit, yet it is not a little singular that we find Sir W. Pym making the following statement in 1845.

"The towns of Alexandria and Cairo are remarkably clean, and Alexandria in particular is rapidly improving;—orders having been issued for widening streets, pulling down houses, and more particularly the huts of soldiers of the lower order, which are rebuilt upon a much better scale. The health regulations are not confined to the towns, but extend over all Lower Egypt, and particular attention is paid to the Delta, medical men being stationed in every district with most particular instructions for acting against any suspicious case of plague. They immediately separate the sick from the healthy, and cut off communication with the individuals of the family, and keep them in quarantine; they have the power of pulling down houses, calling in the military if necessary to their assistance, and in fact they appear to have declared war against plague, and appear confident that the disease may be eradicated. Mehemet Ali himself seems convinced of it, and when I mentioned to him that there was a very short quarantine in England against Egypt, his reply was, 'There ought to be no quarantine, it is our own fault;' he repeated, 'it is our own fault. We must get rid of the plague.'" (Correspondence, p. 17.)

In May, 1846, the parliamentary correspondence from which we have just quoted, and the report to the French Royal Academy of Medicine, by a committee of eleven of its members, were published. The former consists of correspondence between our Foreign Office and those of France and Austria, with regard to the formation of an European congress to establish a uniform period of quarantine for travellers and merchandise, and an account from Sir W. Pym, of visits made by him in 1844 and 1845 to the different lazarettos of the Mediterranean, Adriatic, and Levant. Nothing has resulted from the proposal for a congress, owing to delay and difficulties on the part of Prince Metternich, and it is not a little amusing to trace the manner in which he puts off from time to time the establishment of regulations which would check the rapidly growing importance of the Austrian port Trieste. Vessels have been for several years past admitted to this port with but nominal quarantine, and the consequence has been an increasing afflux of ships, to the injury of Marseilles and the Italian ports. A uniform period of quarantine would destroy this advantage, and therefore the clever Austrian has very politely thrown dust in the eyes of his diplomatic friends. We have already extracted the only important remark in the letter of Sir W. Pym, but shall hereafter notice a most important document he incloses, showing the periods of latency in all the cases of plague which have occurred in the lazaretto of Alexandria, from 1840 to 1843 inclusive. Some interesting papers are also returned from Malta, giving an account of all vessels which have arrived there with plague on board, since the island has been in the possession of Great Britain, but we have given the facts stated in the returns in our former articles.

Dr. White's work is divided into five parts: a dissertation on the nature and qualities of plague; its origin, history, symptoms, and progress; an account of the plague of Corfu in 1816, and its police management, with general remarks on treatment. Mr. Tully had previously published an account of the same plague, but Dr. White regarding it as defective in the "description of the extension of the calamity from place to place, and in the detail of the police treatment adopted on the occasion," has brought forth the present work, censuring Mr. Tully, with apparent justice, for disingenuously concealing the subordinate situation he held under Dr. White during the whole progress of the disease, and assuming the credit of originating measures which he merely carried out under the direction of his superior. The value of the work mainly depends upon the account of the police management of the plague of Corfu, which we think might have been usefully published alone in a smaller and less expensive form, as the remainder, although well written, is a rather common-place compilation from well-known sources of information, and deficient in exact accounts, which the author might easily have obtained, of cases of importation into lazarettos.

The report to the French Academy consists of three parts, the report of the committee, returns and documents in support of their conclusions, and the particular views of individual members, as expressed by them in the general academic discussions. The report states that the committee has especially studied the plagues "of Nimeguen in 1635, of London in 1665, of Marseilles in 1720, of Transylvania, in 1755, of Moscow in 1771, and of Egypt in 1798-99, and 1800." (p. 3.)

It is rather singular that the plagues of Malta in 1813, and Corfu in 1816, of which Hennen, Faulkner, and Tully had published ample accounts—plagues observed in our own time in countries where local origin could not be suspected—should have escaped the notice of the committee, while they have studied those which occurred two centuries since, over the accounts of which much more doubt must necessarily hang. The studies of the French physicians in Egypt since 1835; the work of Dr. Brayer, entitled 'Nine Years in Constantinople;' Dr. Gosse's account of the plague in Greece in 1828-29; Drs. Morea and Hemso on the plagues of Noja and Morocco in 1817-18; original manuscripts regarding all cases of plague observed in the lazaretto of Marseilles since 1720; with numerous despatches from the French consuls and ambassadors in the Levant, with other private communications, have furnished the body of evidence upon which the committee has founded the report. It is divided into four parts.

"In the *first part* we shall inquire in what countries the plague has been known to develop itself spontaneously; we shall then endeavour to determine the causes of the spontaneous plague; we shall show that when these causes have ceased to exist in Egypt and elsewhere, the plague has disappeared; we indicate the countries where the persistence of these causes renders the plague endemic, or, at least, makes us fear the return of the spontaneous plague; lastly, we insist upon the true prophylactic measures of the spontaneous plague.

"In the *second part* we shall answer the three following questions:

"1. Has the plague always shown itself with the principal characters of epidemic diseases, when it has broken out with violence in Africa, Asia, and Europe?

"2. What are the differential characters of the epidemic and sporadic plague?

"3. Is the plague propagated like epidemic diseases, that is to say, by the migration of certain atmospheric influences, and independently of the action that impested persons can exert?

"In the *third part* we shall consider the transmissibility of the plague: Is it transmissible by inoculation? Is it transmissible in and beyond epidemic foci, by the immediate contact of the impested, by the contact of goods, clothing, or merchandise, or by the miasmata exhaled by the impested, of which the air is the vehicle? We shall conclude the third part by the examination of the three following questions:

"1. Can persons affected with sporadic plague give rise to foci of infection sufficiently active to transmit the disease?

"2. Is the plague more or less transmissible according to the intensity of the epidemic, in relation with its first, second, or third periods, or with the organic dispositions of the individuals submitted to the action of the pestilential miasmata?

"In the *fourth and last part* we shall inquire what is the ordinary and exceptional duration of the incubation of plague.

"Then follow the conclusions of our report, and the applications of these conclusions to the questions of quarantine." (pp. 10-11.)

We shall follow the course of the report, giving a condensed statement of the most important results of the inquiries of the committee, with such comments as may appear necessary or desirable.

The following definition of the plague appears to be unobjectionable:

"The plague is a disease of the whole organism, in which the nervous, sanguineous, and lymphatic systems are especially affected, and which is generally characterized exteriorly by buboes, carbuncles, and petechiæ." (p. 11.)

With regard to the countries where the plague arises spontaneously, the committee clearly prove by a quotation only recently discovered in the work of Rufus of Ephesus, a physician who lived under Trajan, that the plague, such as is seen at present, incontestably existed in Egypt at least two centuries before Jesus Christ, the country which all modern research points out as its native seat. They go on to argue that it also arises spontaneously on the banks of the Danube, as well as those of the Nile and Euphrates, and their general conclusion is that "the plague has been known to arise spontaneously, not only in Egypt, Syria, and Turkey, but also in many other countries of Africa, Asia, and Europe." (p. 21.)

We dissent altogether from this conclusion, and can find nothing in any of the documents brought forward by the committee to warrant its acceptance. It is in direct opposition to the fact, that since the establishment of quarantine regulations in Europe, Barbary, and Greece, there has been no plague, the importation of which from Egypt has not been distinctly traced, and scarcely in accordance with the paragraph immediately preceding the conclusion: "at the present moment it is therefore almost exclusively from Egypt that we have to fear the importation of the plague." (p. 21.)

The only arguments at all favoring the conclusion are, that it has existed without proofs of importation near the Danube and in Turkey; but on examining the documents it appears far from certain that the epidemic on the banks of the Danube is really plague, or anything more than a severe remittent marsh fever; and the sanitary committee of Constantinople have satisfactorily proved that cases apparently spontaneous in Turkey, have really resulted from infractions of the quarantine regulations, and

are among the strongest proofs of the universal dependence of the disease in Turkey upon importation from Egypt. Giving every latitude to the facts brought forward by the committee, they can prove nothing more than *probability*, and therefore the conclusion should have been, that it is not yet determined whether the disease arises spontaneously in Egypt only, although our own firm opinion is, that this country is its sole original seat.

The next question is, whether in the countries where the plague spontaneously arises hygienic conditions exist to which its development may be rationally attributed; and after a most interesting account of the state of the people, their habits and habitations in Egypt, Constantinople, Armenia, and the banks of the Danube, this conclusion is arrived at:

"In all the countries where spontaneous plague has been observed, its development may have been rationally attributed to determinate conditions, acting upon a great part of the population. These conditions are especially: habitation upon alluvial or marshy soil near the Mediterranean Sea or the rivers Nile, Euphrates, and Danube; low, ill-ventilated, crowded dwellings; warm and moist atmosphere; the action of animal and vegetable matters in a state of putrefaction; unwholesome and insufficient food; great physical and moral distress." (p. 35.)

Yet in other countries there are marshy sea-shores, and great rivers with a large extent of alluvial soil continually deposited, and marshes where animals, insects, and vegetables putrefy under a burning sun, and we hear of intermittent and remittent disease as the result but not the plague. In other countries poverty, filth, and famine decimate our race, but not by the plague. All the hygienic conditions enumerated in the conclusion are doubtless predisposing causes of disease, and causes which lead to the devastating spread of the plague when it has once appeared, but we do not see any reason why *alone* they should rather produce plague than smallpox, scarlatina, yellow fever, or any other specific disease. The seed must be sown before the harvest can be looked for, and we shall hereafter show that the committee have scarcely alluded to the real cause of the continued existence of the disease in Egypt.

The next conclusion is that the plague is now endemic in Lower Egypt, and exists there every year in the sporadic form, and about every ten years as an epidemic, and that "the absence in Egypt of any pestilential epidemic during the long period that a good administration and sanitary police in the country victoriously combated the productive causes of the plague, justifies the hope that the employment of the same means would be followed by the same results." (p. 40.)

Now the statement of the committee as to the epidemic periods is contradicted by official reports furnished to and published by them, by which it appears that from 1700 to 1838, that is, during a period of 138 years, the plague has appeared in Egypt as an epidemic seventeen times in successive years, nine times at an interval of two years, twice of three years, once of four, six of five, twice of six, twice of seven, twice of eight, once of nine, and only twice of ten. So much for the accuracy of the deductions of the committee from the facts before them.

The question is of extreme interest, why Egypt, described by Herodotus as the most healthy country of the world, a country which was free from pestilential epidemics during 194 years' occupation by the Persians, during 301 years under Alexander and the Ptolemies, and during a great part of the Roman domination, which commenced 30 B. C. and continued until

620 A. D., has since the commencement of the Arabic rule been so often decimated by the plague. The statement of Rufus before alluded to, although proving that the disease was known, also proves that it was nothing more than a sporadic disease in his time, and a casual allusion only of Galen, who was of the school of Alexandria, would prove the same at a like period. The committee state that Alexandria, which was founded 331 B. C. was, *according to Galen*, attacked by plague for the first time as a pestilence A. D. 263. But they have made a strange mistake in chronology, for Galen was born 131 A. D., and if he spoke of the plague of 263, he must have written when he was 132 years of age. The fact is, it is Eusebius, an author by no means noted for accuracy, who describes the epidemic of 263, and the disease he describes appears to have been simply a contagious typhus; Galen, as we have said, only incidentally alludes to plague, while Celsus, Praxagorus, Serapion, Soranus, and above all Cælius Aurelianus, who lived in the fifth century, and practised in Numidia, have been quite silent with regard to any epidemic disease accompanied by buboes or carbuncles. It must therefore have been a rare disease until the great plague of 542, in the time of Justinian, broke out, which we know was regarded by contemporary writers as a new disease.

The ancient salubrity of Egypt must doubtless be ascribed in a great degree to the general prosperity of the people, the canals of Sesostris, the elevation of the towns upon artificial mounds, but we believe, above all, to the practice of embalmment. Ancient Egypt, the mother of the sciences, had recognised the effect of the periodical fertilizing inundations of the Nile, and of the burning heat of the sun upon the deposit left on the subsidence of the waters of the river, over spots where men or animals were buried. What was the result? Inhumation was forbidden, embalmment enjoined, and now, instead of tombs and cemeteries the traveller observes, along the ranges of hills which border the Nile immense subterranean cavities miles in extent, which are filled with embalmed organic remains. The living was thus protected from the dead, and to ensure the observance of the laws religious influence was called in to the support of human wisdom,—the law became a religious rite; the influence of the divinity was employed to protect Egypt from the evils of her physical formation. The salubrity of the country ceased with the practice of embalmment. The Christian missionaries proscribed the ancient usages as idolatrous and sinful, and the mode of sepulture gradually fell into disuse, and was finally prohibited and abolished 356 A. D. We have seen that the plague, though before not unknown, was a rare disease; but in 542 sprung up the terrible plague which devastated Egypt, Turkey, and Europe, to the borders of the Atlantic, and, according to Gibbon, destroyed a *hundred millions of people*.

In our former article we gave our reasons for believing that the emanations from the dead bodies buried in Lower Egypt are the real cause of the persistence of the disease in that country—that the disease is sown and preserved by the mode of sepulture—that the living are poisoned by the emanations of the dead. A porous level soil filled with dead bodies, penetrated universally by moisture during the overflowing of the Nile, is, after the subsidence of the waters, heated by a burning sun, and a vast cemetery, in the language of M. Pariset, is converted into a "true distillery of dead bodies." Hear what he says of Cairo—

"This great city is traversed by a canal of stagnant water, and the half is built upon burial-places, in general very superficial; of thirty-five cemeteries which appertain to it, twenty-five occupy, or lately occupied, the interior, so close to the inhabited houses, that the dead appear to form part of the living." (pp. 937.)

Sometimes individual instances are more striking than general statements, we therefore cite the following facts adduced by the same speaker:

"In the winter 1823-4, the Pacha built a cotton-factory at Kelioub, a small town four leagues north of Cairo. The foundations of this factory were laid among ancient and recent tombs. One day, about noon, a mason complained of headache. He was sent home, and at four o'clock he was dead. He was not visited, but eight persons who composed his family died the same evening, with both buboes and carbuncles. Kelioub was soon infected, and, of 5000 inhabitants, lost 2000.....This year there had been a great inundation and violent rains; the plague did not previously exist in the neighbourhood, and Kelioub had received nothing from the exterior.....One, two, or three workmen in this factory were afterwards daily affected.....The second fact is quite recent. All the habitations of the Copts have domestic sepultures. Lately a Copt rebuilt his house, and, on arriving at the burial-place, the workmen to the number of four took the plague." (pp. 938-9.)

If we inquire into the nature of the principle, the living man receives from the dead, the reply must be that as yet chemistry has not resolved the problem; but M. Danger, in his recent researches upon the decomposition of animal matters, and particularly of the earth of burial-grounds, has thrown much light upon the subject. We continue to quote M. Pariset.

"The immediate products of putrid decomposition are not, as has been supposed, elementary bodies, as hydrogen, carbon, and nitrogen, but, on the contrary, vapours, the organized condensible molecules of which are capable of being dried, and being indefinitely preserved in this state, and also of being completely disorganized by a certain combination of heat and moisture. Drawn along (*entrainées*) by gases, aqueous vapours, currents of air and dust, dispersed without being destroyed by pure water, as Desgenettes believed, expelled by sulphuric acid, they are only neutralized by the powerful alkaline bases, lime, potass, soda, ammonia, cinders of vegetables. They are condensed, on the contrary, and not decomposed, by porous bodies, among which fine sand may be classed. While they preserve their peculiar organization they can serve as a ferment, and consequently act upon living beings to disorganize them, finding in them the degree of heat and humidity which ought to destroy them; they are not destroyed, or do not undergo their metamorphoses but by making the being who has received them a partaker, whose life is thus compromised, and sometimes destroyed. We thus understand why, amidst so many injurious agents, the plague is not so injurious to Egypt as it might be. The air is always in motion. It is swept by torrents of dust in all seasons. The germs of the plague, thus dispersed by the winds, are less connected and less active. In the second place, the water of the Nile is very alkaline; spread over the land, it reaches the dead, and temporarily neutralizes the miasmata. But their source is not exhausted. In the mild and moist heat of winter, putrid decomposition recommences. The subtile ferments it produces are condensed in the sandy soil, of which Egypt is formeds and in which the dead repose. When the elevation of the temperature make, the ferments evaporate, man receives them by the superficial absorbent vessels, or by those of the lungs in the act of respiration." (pp. 940-1.)

We have before shown that in some parts of Egypt, to avoid the access of water, the bodies are buried above ground, and these receptacles become

still more direct sources of contagion. To destroy the plague, both systems of burial must be changed. The cemeteries must be outside the walls of the cities, and as alkaline substances have the property of neutralizing the miasmata or pernicious exhalations, advantage must be taken of the means Providence has placed in the reach of the people. Egypt is supplied with soda in abundance, lime can be easily procured, and instead of enveloping the bodies in soda, as in ancient times after embalmment, let them be buried surrounded by it, or by lime. Let these alkalies be used as manure, and with cleanliness in the towns and villages, and better food for the people, we may hope to see the country restored to its former state of salubrity. The European powers have shamefully neglected this great question. When the government of a petty province is disputed, the four great powers are not slow to interfere; but when it is proposed to exterminate a disease which has destroyed millions of one species, an Eastern pacha is left without assistance. Yet it would be a noble work not only to afford him advice but effective aid, and instead of employing their forces and exhausting their means to destroy man, to turn them to the purpose of reciprocal preservation.

We must leave, however, this tempting theme, and pass on to the next conclusion of the committee, from which we differ entirely.

"The state of Syria, of Turkey in Europe, and Asia, of the regency of Tripoli, of that of Tunis, and of the empire of Morocco, being nearly the same as at the time when epidemics of plague have arisen spontaneously, nothing authorizes us to believe that similar epidemics cannot again break out there." (p. 43.)

Now it has never been proved that the plague has ever been spontaneously developed in these countries. On the contrary, in all the visitations of which correct information has been handed down to us, proofs of importation are distinct and incontrovertible; and since the establishment of quarantine regulations, not a case of plague has appeared in these countries, except in their lazarettos, then of course imported. We always place some reliance on the general belief of the common people, drawn as it must be from experience, and having had frequent opportunities of conversing with natives of all these countries, we may say that we never met with one who, on being questioned as to where the plague came from, had not some story of importation at hand, while an Egyptian has but two answers to the same question, the "will of God," or "it comes from the earth." We still retain the opinion we long since expressed, that, provided the countries in question properly guard against the importation of plague, any restrictions upon commerce or communication with them are needless and injurious. The committee take this view of Algiers, because they say the habits of the Arabs and Kabyles, and the sanitary regulations enforced by the French, are a sufficient guarantee against the spontaneous development of pestilential diseases. Indeed, Algiers is not subjected to quarantine, although Morocco and the Barbary States, which are in unrestricted communication with Algiers, are so. This absurd inconsistency, which we before pointed out, remains unaltered.

The conclusion of the first part of the report is the following.

"The progress of civilization, and a general and constant application of the laws of hygiene, can alone furnish the means of preventing the development of the spontaneous plague." (p. 48.)

The second part of the report is commenced with an inquiry, whether plague, when it spreads with rapidity, is distinguished by the characters of other epidemic diseases, and these characters are defined as follows.

"A disease is epidemic when in a given time it attacks a great number of persons.

"Epidemic diseases present characters which distinguish them from diseases non-epidemic.

"1. They have in their progress a special course. They have generally three periods: periods of commencement, persistence, and decline or termination. These three periods often do not present either the same symptoms, the same lesions, or the same severity.

"2. During the reign of an epidemic disease, other diseases are less numerous and receive the impress of the dominant affection.

"3. When an epidemic disease rages, it is rare that persons who remain in good health do not, more or less, experience the general influence.

"4. Epidemic diseases frequently return and cease in the same season, and in general have the same duration.

"5. An epidemic disease is frequently preceded by other affections, more or less grave, more or less general, which in a manner serve as forerunners." (pp. 48-9.)

Taking these characters as the test of epidemic diseases, the committee conclude, from the evidence before them, that—

"When the plague has raged with violence in Africa, Asia, and Europe, it has always shown itself with the principal characters of epidemic diseases." (p. 73.)

In this we concur, but would remark that smallpox and the contagious exanthemata have the same distinguishing pecularities when they become prevalent. The committee bring forward some interesting facts to show that the situation of a place, and the condition of its inhabitants, have more to do with the epidemic propagation of plague than communication with other infected places. We think, however, that the statement that "infected persons have left infected places, and have lived or died in localities where the epidemic influence did not reign, without the propagation of the malady," only shows what we before urged, that certain conditions of the air prevent the extension of the disease as an epidemic, and that it requires a considerable condensation of the exhalations from plague patients to render them noxious. One of the facts brought forward by the committee is a most glaring misstatement, completely contradicting the real facts of the case. They say—

"In 1813 the English general, Maitland, governor of Malta, not being able, notwithstanding the most severe measures, to extinguish the plague which reigned in Valletta, resolved to have barracks constructed out of the town, and obliged the inhabitants to go and inhabit them. From this moment the plague completely ceased." (p. 71.)

Now all these statements are absolutely unfounded. The plague was not confined to Valletta, but was scattered over nearly the whole island—it was shut up in one village, some months after Valletta was free,—no barracks were even erected outside the town, and the population never left the city. It was on the contrary, by the strict segregation of infected persons in the town, and not by removal from any particular locality that the plague was stopped. It is singular that the only allusion made by the committee to the plague of Malta, should consist of so important an error; perhaps if our personal knowledge extended to other

facts brought forward, we might be able to explain some apparent contradictions in their relations. In this case the mistake is unpardonable, as the works of Hennen, Faulkner, and the dispatches of Sir T. Maitland have made the real facts notorious.

In considering the causes of immunity in certain districts, the *susceptibility* of the population has been too much overlooked, not only with regard to plague but to other epidemic, and contagious diseases. Thus, after a wide-spreading epidemic great numbers of the population are protected by one attack against a second, as a general rule, and a new generation must spring up to supply unprotected victims, in other words, to form a susceptible population. Again, the different varieties of the human race are susceptible in very different degrees. Thus, the Negro enjoys an almost entire immunity from the fevers which are so destructive to the white race on the western coast of Africa, and is peculiarly susceptible to the scourge which devastates the north-eastern. Dr. Aubert-Roche has recorded the proportional mortality of different races during the great plague of Alexandria, in 1835. We subjoin his table with the remark that the Negroes, Nubians, and Arabs lived almost in the same hygienic conditions, all being in free pratique, while, with regard to the remainder of the population, the conclusion must be less certain, as the conditions of isolation and hygiene are not the same for all.

The Negroes and Nubians lost	84 per cent., or	1528 in	1800
The Maltese	61	367	600
Arabs, not soldiers	55	10,936	20,000
Greeks	14	257	1800
Jews, Armenians, and Copts	12	482	4000
Turks	11	678	6000
Italians, and other inhabitants of Southern Europe	7	118	1600
French, English, Russians, and Germans	5	52	1000

(p. 67.)

The committee go on to inquire why the sporadic plague, observed each year in Egypt, is regarded as non-contagious, while the epidemic plague is highly so. They say that this is not surprising to the physician who knows that sporadic dysentery is not contagious, while epidemic dysentery is so to a high degree. We never knew that this had been proved, indeed the experience of English observers in the East, would lead to the opposite opinion. The explanation, however, is not at all difficult. The conditions of air, moisture, and temperature of the population in general, or of the impested in particular, may not be such as is required for the transmission of the disease. We know that typhus does not spread while a patient is scattered here and there over the wards of a large hospital, but collect a number in a confined or ill-ventilated space, their emanations are concentrated, and those who inhale them often fall victims. Just so a single plague patient though really exhaling noxious vapours can only exert a feeble and limited action, while if circumstances have exposed several others to the same cause, or have been such as to prevent ventilation of his dwelling, and expose others to his concentrated effluvia, the injurious action is increased by the accession of every victim in unknown proportion. Bearing this in mind we can easily understand the following conclusion:

"The sporadic differs from the epidemic plague, not only by the small number of persons affected by the disease, but also and especially because it does not present the characters belonging to epidemic affections." (p. 75.)

A very important question follows, whether the plague can be propagated in the same manner as most epidemic diseases, that is to say, by the migration of certain atmospheric influences, and independently of the action that may be exercised by impested persons.

"Nothing could be more important, when a pestilential epidemic has reigned in a town, than to know how many sick owe their affection to the epidemic constitution, and how many either to absorption of the miasmata escaped from the impested, or to direct or indirect contact with the latter.

"This study was made for the first time in Egypt in 1835. Dr. Lachèze found that at Alexandria and Cairo the epidemic influence had affected persons well isolated, so as to destroy one individual in four hundred, while the plague destroyed one in three among the population in free pratique, that is to say, exposed at the same time to the action of the epidemic constitution, to that of the miasmata which might escape from the impested, and lastly, to direct or indirect contact with infected persons." (p. 77.)

Clot Bey and his sect account for these facts by the superior social condition and domestic comfort of those who observe quarantine, but this surely cannot explain exemption almost total. It would, doubtless, greatly diminish susceptibility, but not afford such complete immunity from atmospheric influence. In the arsenal at Alexandria during the epidemic of 1835, of 6000 workmen, 300 only were attacked, or 1 in 20, while the proportion in the population in free communication with each other was 1 in 3, and there is not that difference between the social condition of the arsenal labourers and the general population, as between the latter who do and do not isolate themselves. The committee attribute the 300 attacks to the atmospheric influence, as they say no accumulation of pestilential miasmata ever existed, and actual contact could not be considered as a cause of extension. On this last point, however, others are at issue with the committee, and trace all the attacks to disregard or infraction of the regulations of segregation. The conclusion is that—

"The plague, abstracting the influence which the impested can exert, is propagated in the mode of most epidemic diseases, that is to say, by the action of general causes." (p. 79.)

We should rather this were worded as not yet fully proved, and a subject for further investigation, which it certainly is.

The *third part* of the report commences with an account of the results of experiments by inoculation. We need only give the conclusion, which perfectly accords with that formerly stated by ourselves, although at page 738 of the discussion M. Hamont relates some very striking experiments, but unfortunately they were all made during the existence of an epidemic.

"The inoculation of blood drawn from the vein of a plague patient, or of pus from a pestilential bubo, have only furnished equivocal results; the inoculation of the serosity taken from the phlyctenæ of a pestilential carbuncle has never given the plague; therefore it is not proved that the plague can be transmitted by inoculation even under the influence of a pestilential constitution.

"We know of no experiment on the same subject made beyond an epidemic focus." (p. 86.)

After a very full inquiry whether the plague is transmissible by contact of the sick *in places where it is epidemic*, the committee experience difficulty in determining whether in any given case the disease is owing

to contact, or to the epidemic or endemic conditions in operation; they therefore conclude as follows:

"An attentive and severe examination of recorded facts establishes on the one hand that the immediate contact of thousands of plague patients has proved without danger to those who have been exposed to it in the open air or in well-ventilated situations, and on the other hand that no rigorous observation demonstrates the transmissibility of the plague by sole contact with the sick." (p. 103.)

Thus they agree with the suggestion we long since offered, that the only unobjectionable observations on the transmission of plague are those collected in districts where the endemic or epidemic causes are not in operation, in countries where the disease does not arise spontaneously. It was on this ground that we attached so much importance to the facts observed in Malta, and we still think that, with those recorded by Dr. White as the result of his experience in Corfu, they afford the most exact body of evidence which has ever been applied to this part of the question.

The conclusion with regard to the contact of the clothes or effects of plague patients is that—

"Facts, in a very great number, prove that the goods and clothing which have served the impested have not communicated the plague to persons who have used them without any previous purification.

"The facts which appear to have given an opposite result can only acquire value if confirmed by new observations made beyond epidemic foci, far from the centres of miasmatic infection, far from the countries where the plague is endemic." (p. 113.)

It must be borne in mind, however, first, that the clothes and effects of persons dead of plague are never sold or used in Egypt until after St. John's Day, that is, until after the season in which plague ceases, in other words, until the degree of heat and moisture in the atmosphere, or some unknown condition, is incompatible with the spread of the disease; 2d, that they are freely exposed to the air. It would be difficult to disprove the opinion of many Egyptian physicians, that when the proper season returns these very clothes, if not well purified by exposure to air, are one great cause of the continual existence of the plague in Egypt.

We fully concur in the next conclusion—

"The transmissibility of the plague by merchandise in the countries where the disease is endemic or epidemic, is not an established fact." (p. 115.)

The succeeding question is whether in *epidemic foci* the plague is transmitted by means of air charged with pestilential miasmata exhaled from the lungs and bodies of the sick, and it is a very difficult one to answer, as the doubt always arises whether fresh cases are owing to epidemic influence or to infection. We therefore extract some important facts which tend to its elucidation.

"In 1835 most of the apothecaries of the plague hospital called Ras-el-Tin, at Alexandria, were Italians, who carried to the extreme the fear of every suspicious contact, and the precautions which they regarded as necessary to preserve them from the scourge. Nevertheless, the service they are obliged to render in the wards, where a great number of plague cases were collected, sufficed to make every one of them contract a mortal plague." (p. 117.)

"In the hospitals of Cairo during the same epidemic of 1835, physicians, apothecaries, and nurses were affected with the plague. Of twenty pupils who were sent there from the medical school of Abouzabel, nineteen died.

"Here again we must inquire if these cases were due to the epidemic influence, to contact or infection?

"We do not think we can answer this question better than by repeating what occurred at Abouzabel when this place was invaded by the pestilential contagion.

"At Abouzabel the plague patients were examined, touched, and treated in elevated and well-ventilated country barracks. Neither the physicians nor the apothecaries were affected, notwithstanding repeated contact with impested men and goods, notwithstanding residence in the epidemic focus. But a free ventilation had prevented the formation of any focus of infection." (pp. 117-8.)

Many other facts of great interest might be quoted, but all are more open than those we have just extracted to the objection of epidemic influence rather than that of infection, and show more strongly than ever that it is in places far from the action of the former, at sea and in the lazarettos of Europe, that the question must be determined. The conclusion is given as follows:

"In epidemic foci the plague is transmissible by the miasmata which the impested exhale, and by the foci of infection which may result from them." (p. 132.)

We now pass to the grand question, "Is the plague transmissible beyond epidemic foci?" An affirmative answer maintains, a negative abolishes quarantine regulations; but as we have already entered so fully into this part of the subject in our former articles, we need do little more than give the conclusion of the committee drawn from authentic notices of 25 ships, which since 1720 have arrived in France and Italy with the plague on board.

"The plague is not only transmitted on board ship among persons leaving the same shores, breathing the same air, having the same exercise, the same habits, the same food. The plague patient deposited in an European lazaretto, becomes the cause which develops in others the affection from which he is suffering." (p. 163.)

It is stated that other facts observed at Venice, Leghorn, and Genoa prove the justice of the conclusions drawn from facts observed at Marseilles, and the general conclusion is the following:

"It is incontestable that the plague is transmissible beyond epidemic foci, both in ships at sea and in the lazarettos of Europe." (p. 165.)

If anything is still required to strengthen this conclusion, nothing could do so more forcibly than the return from the records of the quarantine department of Malta of the vessels which had arrived with the plague on board since 1819, published in the parliamentary correspondence before us. It is singular the committee should not have noticed these facts, as the French consul at Malta appears to have been supplied with a copy in 1845, for the express use of the Academy.

With regard to the mode of transmission, it is concluded that transmission by simple contact cannot be proved. This, however, is a very unnecessary refinement, as it would be difficult to be in such close relation with a person as to touch him without inhaling some of his pulmonary exhalations. As to the danger of transmission beyond epidemic foci by effects and clothes of plague patients, the committee do not deny the danger, but doubt, and think further researches necessary in localities where it is certain the plague is not endemic.

We have already given our opinion of the following statements:

"Nothing proves that merchandise can transport the plague beyond epidemic foci." (p. 170.)

"The classification admitted in our lazarettos of susceptible and non-susceptible objects is not founded upon any fact or experiment worthy of confidence." (p. 175.)

"We will not further prolong these considerations on the means of destroying the pestilential principle. They would be certainly insufficient if it were demonstrated that this principle could remain inherent in goods, clothes, or merchandise, and become the cause of the transmission of the disease. They are and will be completely without object until it shall be proved that the virus or pestilential miasm can be presented in these different receptacles.

"The study of the means of disinfecting goods, clothes, and merchandise is still to be made.

"To be rational, researches on this subject ought to be preceded by proof that these different objects can really become charged with the principle of the plague." (p. 179.)

"The plague may be transmitted beyond epidemic foci by miasmatic infection, that is to say, by air charged with miasmata exhaled from the bodies of the impested.

"We shall follow this conclusion by three others, which are but corollaries, but which it appears useful to detail separately, as upon each of them quarantine regulations are based.

"1. It results from facts detailed in the chapters relative to the transmissibility of the plague in and beyond epidemic foci, that the impested, by vitiating the air of the localities in which they are confined, can create centres of pestilential infection capable of transmitting the disease.

"2. The centres of pestilential infection can persist after the removal of the impested.

"3. The centres of pestilential infection once formed on board a ship, by the presence of one or many sick, may be transported even to great distances." (p. 181.)

The two latter conclusions are drawn from a number of curious facts, and it becomes an interesting question how the virus is preserved, whether by imperceptible incrustations forming on the walls of houses or timbers of of ships, or by other unnoticed means. At a late meeting of the Chemical Society, Dr. R. A. Smith read an account of experiments to determine what was the real evil in the polluted atmosphere of towns, and stated that the moisture condensed from the breath contained organic matter in large quantities; and when collected from the windows of crowded rooms, it smelt strongly of human sweat during the evaporation and when the solid residue was heated, it gave the odour of burning flesh. (*Literary Gazette*, March 6th, 1847.)

It appears that physicians in Egypt of the most opposite opinions on the general question of contagion, are agreed with regard to the non-contagion of the disease in its sporadic form. Between June 1835 and December 1838, 649 cases of sporadic plague were observed in Alexandria, and 646 of these did not communicate the disease to those who touched or attended on them. In 3 it was doubtful whether it did so or not. The Franks also, in the Levant, who observe strict quarantine during epidemics, adopt no precaution against the disease when only present in a sporadic form. It appears further, that in cases imported by vessels to Europe, these vessels have *always* departed from infected places

while *an epidemic was raging*. It is desirable that these researches should be carried on, and—

"If the results should confirm those already obtained, we shall be able without inconvenience to confine quarantine precautions against the importation of plague to Europe to the periods when pestilential epidemics reign. This would be a great service rendered by medicine to society.

"At present we believe we should define the conclusion of this chapter thus:

"Patients affected with sporadic plague do not appear to be able to determine centres of infection sufficiently active to transmit the disease." (p. 188.)

"Is the plague more or less transmissible according to the intensity of the epidemic, its different periods, and the organic dispositions of the persons submitted to the action of the pestilential miasmata?"

The answer is in the affirmative; age, sex, state of health, and other causes, with the condition of the atmosphere, having an evident effect upon the spread of this as of all other contagious diseases.

The third part is terminated by the following observation:

"If it is not proved that the existence of a pestilential constitution in a country where the plague is imported is necessary for the transmission and propagation of the latter, still it appears certain that this imported plague cannot effect great ravages, if it does not encounter in the climate, atmosphere, and inhabitants conditions favorable to its development." (p. 196.)

The necessity for preventing importation of plague having been proved, the period under which persons arriving from suspected places must be kept under observation is to be determined by the number of days the disease may remain latent in the human body. We showed in our former article that there was every reason to believe that the period of latency never exceeded ten days. The committee go further, and state that although a fixed and absolute limit cannot be assigned to the duration of the incubation of plague—

"Still it appears demonstrated by known facts, that far from countries where the plague is endemic and beyond epidemic foci, this disease has never broken out in compromised persons after an isolation of *eight* days. The few facts that might be regarded as exceptions to this rule are all susceptible of another interpretation." (p. 199.)

In our parliamentary correspondence we find a document strongly corroborating this statement, being a return of all the persons who had been placed under observation in the lazaretto of Alexandria during four years, 1840 to 1843 inclusive. The number of families was 538, of individuals 5240, and out of this number 43 were attacked with plague, and all *before the eighth day* after the operation of *spoglio*, which consists in washing and being separated from all clothes, bedding, or anything with which they or suspected persons had been in contact. We find on summing up the table that 12 were attacked on the *third* day, 8 on the *second*, 7 on the *first*, 5 on the *fourth*, 3 on the *sixth*, 2 on the fifth, seventh, ninth, and tenth, and 1 on the eighth.

Up to 1841, the period of quarantine after *spoglio* was eleven days, but as it was proved that in those apparently attacked on the 8th, 9th, or 10th days the disease had always really shown itself before the seventh day, in 1842 the period of observation was reduced to seven days only after the operation of spoglio in the lazaretto.

These facts were known to the Academy, and those in favour of longer

quarantine brought forward the objection, that the cases were arrivals from Constantinople, and consequently the disease was a milder one than that of Egypt, and it did not follow that the period of latency was the same. But this is completely erroneous; those admitted into the lazaretto of Alexandria being almost invariably the remaining members of a family in the city, one of the members of which has died of plague, or persons who have been in communication with them.

In his communication to the committee, M. Segur-Dupeyron quotes a case which lately occurred at Malta, seventeen days after the arrival of the vessel, and forty-six after her departure from Alexandria. He argues very gravely that this could not have been a case of incubation of 46 days, but that the person must have taken the disease from having opened some of his trunks with papers and clothes after his arrival in Malta. Now we happen to have seen this man, and to know that his disease was not plague, and as the case is one which shows how every apparent fact opposing principles deduced from general observation should be examined before it is received as true, we shall give a short account of this supposed instance of 17 or 46 days' incubation.

On the 17th day after the arrival of a vessel from Alexandria, a man complained of fever, and on being visited by the quarantine physician was found in a typhoid condition, with a bubo in the groin. He had had the plague in Malta, in 1813, and stated his own conviction that he was suffering from the same disease. Two physicians, who had also had great experience of the plague in the same year, agreed with him, and the fact being calculated to materially influence proposed changes in the quarantine regulations, the chief medical officers of the army and navy were called in to the assistance of the Maltese physicians. The English gentlemen had never seen plague, but had seen a great deal of syphilis and mercurial cachexia, and gave a very strong opinion that the man was suffering from the latter, although he stoutly denied it. Every precaution was taken as if the case were plague, and the case went on from bad to worse, the English and Maltese physicians still holding opposite opinions, and the man himself stoutly adhering to his original statements. The intelligent captain of the lazaretto, suspecting some concealed motive for this conduct, went to the man, and told him that the doctors were killing him, as they were giving him medicine for plague, which would certainly kill any one suffering from syphilis. This led to a confession, but the man died before it could be formally received. Nothing satisfactory resulted from the post-mortem examination, and the case would have still remained open to dispute, the opposite assertions of the man being equally unworthy of confidence, had not his wife and two persons living in the house with them soon afterwards arrived at Malta from Alexandria. All these people were examined on oath before the Board of Health, at Malta, and swore that the man had a bubo before he left Alexandria, that he was known to have associated with a prostitute, and, on leaving Alexandria, to have had both mercurial pills and mercurial ointment in his possession. So much for M. Segur-Dupeyron and his 17 days' incubation of plague.

Let us now inquire what are the practical applications of the scientific conclusions of the committee:

"The sanitary measures which should assure the security of France may be classed under five heads:

"1. Indication of the countries from which we have to fear the importation of the plague.

"2. Precautions to be taken at the departure of ships leaving suspected countries for France.

"3. Rules to observe during the passage or stoppages.

"4. Precautions at arrival in a French port.

"5. Measures to be taken in case the plague should break out in a French town." (p. 205.)

In this chapter a want of personal knowledge among the members of the committee of lazarettos, ships, and the present means of segregation and observation is evident in every page, and, what is far less excusable, their most important conclusions are not carried out in the measures they propose. Thus they conclude, not only that the plague has never been imported except by a ship with plague cases on board which had left an infected place, but also that in every such case the plague was raging *as an epidemic* in this place at the time of the ship's departure. This is not strictly correct, as M. Segur-Dupeyron has recorded nine cases in which plague had appeared after the arrival of a ship, and had not done so during the passage. But in every such case the voyage has only been from three to six days, that is, within the usual period of latency, and every one had sailed from infected places. The latter part of their conclusion is confirmed by the experience of 200 years, and certainly proves that no quarantine is necessary, except upon ships arriving from places where plague was raging at the time of departure. This is what we long since urged, and are still convinced that it is the only means of preserving Europe from the plague, without impeding commerce and communication. It does not destroy lazarettos, nor render a properly-trained sanitary body less necessary than at present, any more than peace leads to the disbanding of a standing army. Everything should be prepared for the moment of danger, but no restrictions enforced which security renders needless. The committee still keep Syria, Turkey, the Barbary States, and Morocco subjected to quarantine, whereas their own conclusion shows that this is perfectly useless when there is no plague there. Since 1837, the period when the sanitary board was established at Constantinople, the only plague cases observed there have been in the lazaretto; these are clearly imported, and did not extend beyond the lazaretto. The same fact has been observed since 1838 in Smyrna and in Greece, and Syra, the principal lazaretto of the Grecian Archipelago; the only cases of plague have occurred in the lazarettos, and have been incontestably imported. The plagues of Barbary have always been imported, and generally by vessels with pilgrims on their return from Mecca; but this only when plague was epidemic in the countries they had left. Egypt is at present under different circumstances, as sporadic cases occasionally occur; but it is clear that the state of public health in any port at the time of a ship's departure should be the guide whether quarantine should be imposed upon her arrival in Europe.

A number of precautions are recommended before a ship leaves suspected countries, but the only important alterations in existing regulations are that the bill of health should be signed by a consular physician, and should be given on the day of departure of the ship. The latter would be a great improvement, as the present plan of granting departure within six days of the date of the certificate renders it entirely useless. The

physician is recommended to remark upon cleanliness and ventilation of the ship; but when a vessel is full of cargo, he must be very sharp-sighted indeed who could discover the clean or dirty state of her hold. It is recommended that a physician should be attached to each consulate, to give the certificate of the sanitary condition of the district, but this would lead to expenses, to which merchants might reasonably object, as the consuls always give their certificates upon reports furnished by the principal medical men of the place, who must be as well informed as a consular physician could be. On the whole, the recommendation has the objectionable air of a *job*. The abolition of the suspected bill of health is very properly advised; but we should object to the rule to give a clean bill when there are *only a few* sporadic cases, and a foul bill only when these are more numerous, and prefer giving a foul bill when any plague is present, a clean bill when there is none, as one of the *very few cases* might find its way on board ship while the disease was latent. Besides it is impossible to say when the sporadic disease may become epidemic, or, in the words of the report, when an epidemic is imminent. Who can say when this is to be feared? Who can draw the line between the termination of the sporadic and the commencement of the epidemic condition? How many sporadic cases can be present before the disease can be considered epidemic? The rule of the committee is altogether too vague; too much is left to the discretion of the physician, who, if a contagionist, would scarcely ever give a clean bill of health, and if a non-contagionist, never a foul one.

The precautions during the voyage consist in ventilation and a sick list to be kept by the surgeon or captain.

The periods of quarantine on arrival in Europe are regulated as follows:

"1. For ships having a physician on board, and coming from Egypt, Syria, or Turkey, with a *clean* bill of health, the quarantine shall be ten full days from *departure*, when neither the plague nor any suspected disease is manifested on board during the voyage.

"The quarantine shall be fifteen full days from *departure* when the same ships arrive with a *foul* bill of health, if neither plague nor any suspected disease is manifested on board during the voyage.

"2. For merchant ships arrived with a *clean* bill of health, not having a physician on board, a quarantine of observation of ten full days shall be prescribed from *arrival*.

"When the same ships arrive with a *foul* bill, but without either plague or any suspected disease having appeared at sea, they shall undergo a vigorous quarantine of fifteen days from date of *arrival*." (p. 217.)

All this is pitiful yielding to existing prejudices, an evident fear of applying their own deductions from authenticated facts and long experience to actual practice. To give ten full days' quarantine from time of arrival to a ship with a *clean* bill of health is an absurd contradiction of the most important of their own conclusions; and the distinctions between ships with and without a medical man are unjust if both have a clean bill of health, and no suspected disease has appeared in either. The committee show useless severity in time of safety, and we think not sufficient caution in time of danger; for in the present state of our knowledge it appears that a period at least equal to that of the longest period of latency of the disease should pass from the *arrival* of the ship and

ventilation of the cargo and effects, before the crew and passengers are admitted to free pratique, when a *foul* bill has been given at the port of departure. It would be well also in every case where a suspected disease shows itself on board ship, to leave the duration of the quarantine to the discretion of the local authorities.

Neither do we approve of the mode of procedure recommended in the report, in case plague should appear in a European town, when it is recommended to "induce or even compel the greatest possible number of inhabitants to leave the town." The accounts in Dr. White's work and the facts observed in Malta, in 1813, show that isolation is the grand point, separation of the sick from the healthy and from each other. The sick and those who have been in communication with them should be removed to well-aired places in the suburbs, not the healthy and uncompromised inhabitants. The rules are, however, on the whole, good and worthy of extract.

"If the plague appears in a house, the person affected should be immediately carried to a distant and well-ventilated situation, where he can receive all the care his case requires. All the other inhabitants of the house should repair to a place pointed out by the authorities, where they may be submitted to the inspection of a physician. The evacuated house should be cleaned, washed, ventilated, purified, and remain empty at least a month.

"If several houses should be attacked, the same conduct must be pursued in each case. Further, the greatest possible number of inhabitants should be induced or compelled to leave the town, places of refuge being assigned them, and being submitted to measures of isolation necessary to prevent the propagation of the disease to the neighbouring population." (p. 229.)

This would be impracticable. Could places of refuge be built for the inhabitants of a large town at a day's notice?

"If entire towns were the seat of an epidemic of plague, these dispositions should be put in execution on a grander scale, and with a vigorous severity, but the principles would remain unchanged. It would be always necessary, on the one hand, to endeavour to make all persons not affected by the disease leave the epidemic foci; and, on the other, to isolate and disseminate the impested in elevated and well-ventilated situations, so as to prevent the formation of centres of pestilential infection." (p. 230.)

We have now gone through this Report, and need not refer to the documents on which it is founded, having already given the inferences drawn from them, and a large portion consisting of translations from the parliamentary correspondence are analysed in our former articles. From the discussions which follow, it appears that almost every member of the committee signed the report with an understanding that he was to express his dissent to certain conclusions in the general meetings of the Academy, and their speeches show that they still entertain very opposite opinions on the leading principles of the controversy. These principles, however, have been so fully discussed in the different sections of the report that we need not again refer to them, but remark that the work as a whole, report, documents, and discussions, is by far the most complete body of evidence, and the most accurate account of the natural history of plague which has ever yet appeared, being a complete analysis of the numerous published and unpublished works which have hitherto been written, always excepting those of the English writers on the disease in Malta and Corfu; still we must express our opinion that, with practical reference to the quarantine

question, the field of inquiry might have been judiciously limited, more care taken in distinguishing assertions from facts, and more definite regulations framed upon the scientific conclusions. The report is highly creditable to the Academy, and particularly so to M. Prus, who drew it up, although we think he has shown more learning, industry, and research than close reasoning, or the peculiar talent of seizing and forcibly presenting the really important facts presented to him.

Before concluding we must refer to the effects produced by this report. In the Chamber of Deputies the Minister of Commerce announced that he should regulate his proposed reforms by the advice of the Academy, but he met with considerable opposition from the superior council of health, which, being composed of peers, deputies, and bankers, wished to retain the patronage of £80,000 a year the sanitary administration of quarantine costs the country. Lively debates took place in the Chamber of Deputies, and we are happy to be able to allude to the creditable position held there by M. Bouillaud, as a proof that a scientific and practical physician can also hold an honorable and useful place in the parliament of his country. M. Thiers took part in one of the debates, and in the course of his speech made one of those sarcastic allusions to physicians, which vain pretenders have drawn upon their order, "I do not come here to talk medicine," said M. Thiers, "but common sense." The reply of M. Bouillaud was a model of tact, wit, and good taste. After summing up the argumentative part of the question, he continued—

"I am sorry to be obliged to trouble you a little longer. I never could have believed that M. Thiers, who justly prides himself upon his common sense, could have said that this was not a question of medicine, but of common sense. Gentlemen, on this point I will relate you a little anecdote which recurs to my memory. (Hear, hear.)

"Hannibal—(loud laughter)—Hannibal, during his exile, was invited to attend the course of an illustrious professor who lectured on the art of war—he had never made war, and yet he lectured on war. When Hannibal had heard the illustrious professor, he gave, on retiring, to the persons who asked his opinion on the superiority of this great professor, an answer which M. Thiers knows, and which I need not recall to his memory. (Loud laughter and cheers.)" (Journal des Débats, 4 Juin, 1846.)

To understand this joke our readers should be aware that Hannibal is a name given to M. Thiers by some of the Parisian journals, in allusion to his very warlike writings, and his very *un*warlike conduct in flying from Paris during the three days' revolution of 1830, which he had mainly contributed to excite, and his return to reap the rewards after the fight was over.

To return, however, to the results of the debate and the recommendation of the Academy. The quarantine from Constantinople has been reduced to twelve days, including the voyage in case of clean bill of health, or if the voyage exceeds twelve days, three days only of observation after arrival. Five days after arrival is imposed upon ships from Tunis, and nine upon those from Tripoli. So far Marseilles has yielded to the movement, and Malta and other Mediterranean ports have followed her example. The important effect of even this change can be understood when we state that in one day, at Marseilles, 150 vessels were admitted to pratique, on the issue of the new regulations, and an almost equal number at Malta. Egypt, however, is still exposed to a long quarantine,

even when clean bills of health are issued. All these half measures, the useless quarantine upon the Barbary States, its partial removal from Turkey, its continuance upon Egypt when healthy, are the results of the timidity of the Academy in putting their scientific conclusions into practical application,—and we have still to urge our old propositions.

1. To furnish all ships with *clean* bills of health where no plague is present at the port of departure, and with *foul* bills in the opposite state.

2. To receive all ships with clean bills of health in free pratique.

3. To impose eight clear days after arrival and ventilation of cargo to all ships with a foul bill.

4. In case of any suspected disease on board, the quarantine to be regulated in duration by the local authorities.

These are plain and definite applications of science to practice, which afford security to the public and but little inconvenience to individuals, or impediment to commerce, and concentrate the attention of the sanitary guardians upon the real seat of danger. A far nobler work still remains to be accomplished—the annihilation of the scourge which has been so fatal to our race. The work is begun, and we earnestly hope that the Arab may have cause to bless our country for its completion.

P.S. Since the preceding article, which was intended for our April number, was written, we are happy to say that both the French and English governments have adopted the rational system of quarantine advocated in it and in our former articles on the subject. The boon thus conferred on commerce is immense, while it is gratifying to know that it is exclusively to the exertions of medical men that the important changes have been brought about. We give the French ordonnance, dated 18th April, in an abridged form; the shorter English document we print in full.

I. FRENCH REGULATIONS.

"1. Vessels arriving from impested places will in future only be distinguished by *clean* and *foul* bills of health, the latter only to be given when, in a place of departure or in places therewith in free communication, some pestilential epidemic exists under circumstances dangerous to the public health, and is to be given the day of or the day before the departure of the vessel. 2. Vessels from Turkey or Egypt, with an authorized sanitary officer on board, will be admitted on arrival with a clean bill of health to free pratique if the voyage have occupied ten days. 3. Those from Turkey without a sanitary officer to have a quarantine of three days in the Mediterranean ports and one day in other French ports. 4. Those from Syria and Egypt, with a clean bill, and without a sanitary officer, to five days. 5. Those with a *foul* bill to have ten full days from arrival. 6. Those with a clean bill from Tunis to be admitted in free pratique. 7. Merchandise with a clean bill to be admitted in free pratique after a voyage of ten days, and with foul bill, three days after arrival. 8. In cases of disease on board ship the local authorities to decide the period of quarantine. 9. French sanitary physicians will be established in the Levant to sign certificates at the port of departure."

II. ENGLISH REGULATIONS.

"Council Office, Whitehall, 18th May, 1847.

"Sir,—I am directed by the Lords of the Council to state to you for the information of the Commissioners of Customs, that their Lordships, taking into consideration the healthy state of the Levant (with reference to plague) for several years past, have come to the determination of abolishing for the present all quarantine upon vessels arriving from the Levant (Turkey, Egypt, and Syria

included), whatever may be the nature of their cargo, provided such vessels are furnished with clean bills of health, having been first visited by the quarantine or other proper officer of Customs, and that the crews and all persons on board have been free from any suspicion of infectious disease during the voyage. You will therefore move the Commissioners of Customs to give the necessary instructions for carrying the above regulation into effect. I am, sir, your obedient servant,

"Charles Scovell, Esq. (Signed) W. L. BATHURST.

"Custom House, London, 21st May, 1847.

"The aforegoing copy of a letter from Mr. Bathurst (one of the Clerks of the Council in waiting) is transmitted to the collector and controller at for their information and government. By order of the Commissioners."

ART. XVIII.

Researches on the Chemistry of Food. By JUSTUS LIEBIG, M.D., Professor of Chemistry in the University of Giessen. Edited from the Manuscript of the Author, by WILLIAM GREGORY, M.D., Professor of Chemistry in the University of Edinburgh.—*London*, 1847. pp. 176.

WHEN we inform our readers that the preface to this work bears date "Giessen, 1st June, 1847," we feel assured that we have said enough to convince the most unreasonable of them that anything like a review of it (in the sense which we trust we have taught them to understand the word) is utterly out of the question on the present occasion. We regret this circumstance the less, because many parts, we may say, the greater part, admit as yet of no discussion; the volume being almost entirely made up of accounts of processes and results which are stated to have been respectively carried on and obtained in the Giessen Laboratory during the past year. "The present little work," says its author, "contains the analytical details of my investigations on these subjects, [the nature of the organic acid diffused through the muscular system, and that of the other substances contained in that system] which, in accordance with the plan of the Animal Chemistry, could not be introduced into that work." Hence any criticisms that these investigations may evoke at the hands of other chemists will, if necessary, be laid before our readers when the new edition of the Animal Chemistry is completed.

THE FIRST SECTION is devoted, for the most part, to the protein-controversy that has been going on for the last eighteen months between Mulder and Liebig, and as the question must still in a great measure be considered *sub judice*, we will, with a passing expression of our regret that a scientific difference could not have been decided in a less hostile spirit, proceed to the remaining parts of the volume.

SECTION SECOND.—*On the constituents of the juices of flesh.* Our author commences by showing that although Berzelius announced forty years ago that the acid reaction of the juice of flesh depended on the presence of lactic acid, no sufficient proof had ever been given of the real characters by which the acid could be identified, until the present investigations were undertaken; he then alludes to Chevreul's discovery (in the year 1835) of kreatine, to Berzelius's want of success in obtaining it, and to the fact that Wöhler and Schlossberger were enabled to isolate it. These researches constituted the whole of our knowledge in

regard to lactic acid and kreatine as ingredients of flesh. As we wish to give an abstract of Liebig's results rather than of his processes, we shall not trouble ourselves or our readers with his improved means of obtaining kreatine. It is at best a complicated and not very easy process, and he himself confesses to "many fruitless attempts." With regard to the physiological relations of this substance, we may observe that it seems to vary considerably in different kinds of flesh. "Of all kinds, the flesh of fowl and that of the martin contain the most, then that of the horse, the fox, the roe-deer, the red-deer and hare, the ox, pig, calf, and finally, that of fishes." (p. 45.)

The amount is greater in wild than in tame animals. The flesh of a fox fed on flesh for two hundred days did not yield so much as the tenth part of the quantity of kreatine obtained from foxes killed in the chase.

"The amount of kreatine in the muscles of an animal stands in an obvious relation to that of fat, or to the causes which determine the deposition of fat. From fat flesh there are often obtained mere traces of kreatine, and always much less than from lean flesh, for the same amount of muscular fibre." (p. 45.)

The following table gives the relative amount of kreatine in the animals specified:

1000 parts of the flesh of	fowl	yielded 3·05	kreatine.
„	the horse	0·72	„
„	the ox	0·697	„

Liebig has not been able to detect it in the substance of the brain, of the liver, or of the kidneys; but it is present in abundance in the heart of the ox, so that this organ is especially adapted for its preparation. He describes crystals of pure kreatine as "colourless, perfectly transparent, and of the highest lustre; they belong to the klinorhombic system, and form groups the character of which is exactly similar to that of sugar of lead. At 212° the crystals become dull and opaque, with loss of water." From four experiments it appeared that the water of crystallization expelled at this temperature averaged 12·17 per cent.

From several analyses it appears that the formula for crystallized kreatine is $C_8N_3H_{11}O_6$, and its atomic weight 149; while anhydrous kreatine has for its formula $C_8N_3H_9O_4$, and for its atomic weight 131.

"If we compare the formula of kreatine with that of glycocoll (sugar of gelatine), it appears that crystallized kreatine contains the elements of 2 equiv. glycocoll ($C_8N_2H_8O_6$) + 1 equiv. ammonia (NH_3). Kreatine dissolves easily in boiling water, and a solution saturated at 212° forms, on cooling, a mass of small brilliant needles. From a diluted solution it crystallizes very slowly, in somewhat larger crystals, often from two to three lines in length, and one line in thickness, which increase in size for twenty-four hours after cooling, if left in the liquid. 1000 parts of water at 64·4° dissolves 13·44 parts of kreatine, or 1 part of kreatine dissolves in 74·4 parts of water.

"In cold alcohol kreatine is nearly insoluble, 1 part requiring 9410 parts of alcohol for solution. In weaker spirits of wine it is rather more soluble.

"The cold aqueous solution of kreatine possesses, from the small quantity of dissolved matter, a weak, bitter taste, followed by a somewhat acrid sensation in the throat. When the aqueous solution of kreatine contains a trace of foreign organic matter, it decomposes very readily.....

"No quantity, however large, of kreatine can destroy the acid reaction even of the weakest acids, and it possesses no basic characters. It dissolves easily with the aid of heat in barytic water, and crystallizes from it unchanged..... But when boiled with barytic water, kreatine is decomposed, ammonia is disen-

gaged, the liquid becomes turbid, even when the air is entirely excluded, and there is deposited carbonate of baryta in crystalline grains, the quantity of which progressively increases as the boiling is continued.. .....

"The action of strong mineral acids is very remarkable. A solution of kreatine, to which, while cold, hydrochloric acid is added, gives by spontaneous evaporation crystals of unchanged kreatine. But when heated with strong hydrochloric acid, a solution of kreatine no longer yields crystals of that substance. The same result is obtained with sulphuric, phosphoric, and nitric acids. When kreatine is dissolved in one of these acids, and the solution gently evaporated, crystals are obtained, which are very soluble in alcohol, a property not belonging to kreatine. These crystals contain a portion of the acid employed, in a state of combination.

"There is formed in this reaction from kreatine, by a transformation of its elements, caused by contact with strong mineral acids, a new body of totally different properties, a true organic alkali, which I shall call *kreatinine*." (pp. 51-3.)

Kreatinine, its preparation, properties, &c. Kreatinine may be obtained in a state of purity from its hydrochlorate or sulphate. The crystals belong to the monoklinometric system, and their measurements have been determined by Kopp. Kreatinine is much more soluble in cold water than kreatine, one part dissolving in 11·5 parts of water at 60°; and in hot water it is much more soluble. It communicates an alkaline reaction to its solution. It dissolves in boiling alcohol, and crystallizes on cooling. 1000 parts of alcohol at 60° dissolve 9·8 parts of kreatinine.

By various reactions it is shown to be a powerful base; in fact, Liebig observes that, "in its chemical character, kreatinine is quite analogous to ammonia."

The conversion of kreatine into kreatinine is shown to depend on the separation of 4 eqs. of water.

$$\text{Kreatine } (C_8N_3H_{11}O_6) = \text{Kreatinine } (C_8N_3H_7O_2) + 4\ HO.$$

"If we compare with the formula for kreatinine that of caffeine (théine), it appears that kreatinine contains the elements of 1 atom of caffeine + 1 atom amide.

"Caffeine is $C_8N_2H_5O_2$: add to this

1 at. amide $N\,H_2$

The sum is $C_8N_3H_7O_2 = 1$ at. kreatinine." (p. 59.)

We now pass from the consideration of the fluid of muscular flesh to that of the urinary secretion. Those of our readers who are conversant with the chemistry of the secretions, must recollect that about three years since a new nitrogenous substance was discovered almost simultaneously in it by Pettenkofer and Heintz. This substance contains the same proportions of carbon and nitrogen as kreatine and kreatinine; in fact, Liebig shows that it is nothing more than a mixture of kreatinine with a little kreatine, which may be easily separated from one another by means of alcohol; one of them (as we have already stated) being easily soluble, the other very sparingly soluble in hot alcohol.

The composition of these two substances, as obtained from urine, was found to be as follows:

	Kreatine from urine. (anhydrous.)	Kreatinine from urine.
Carbon	36·90	42·64
Nitrogen	32·61	37·41
Hydrogen	7·07	6·23
Oxygen	23·42	13·72
	100·00	100·00

"If we compare these numbers with those obtained by the analysis of kreatine from flesh and the analysis of the kreatinine prepared from it, it is obvious that they are respectively identical, and indeed no difference can be detected in the physical and chemical characters of the two substances from urine and those from flesh." (p. 64.)

During the putrefaction of urine, the kreatine is destroyed by the action of the carbonate of ammonia formed from the urea, while the kreatinine suffers no change.

Liebig regards the urine as not only the most economical but also the most convenient source of these compounds.

In a subsequent portion of the work it is shown that kreatinine is an actual constituent of muscle, or, in other words, that it exists ready formed in flesh, and a simple process for extracting it is given.

We now arrive at the consideration of a new substance, *Sarcosine*, obtained by the action of boiling barytic water on kreatine.

Sarcosine crystallizes in right rhombic prisms, acuminated on the ends by surfaces set perpendicular on the obtuser angles of the prism. The crystals are colourless, perfectly transparent, and of considerable size. They are extremely soluble in water, very sparingly soluble in alcohol, and insoluble in ether.

The aqueous solution of sarcosine has no action on vegetable colours, but from the mode in which it combines with acids it evidently acts as a powerful base. Liebig deduces for it the formula $C_6N_1H_7O_4$, which serves to explain its production from kreatine.

"If from the elements of crystallized kreatine we subtract those of sarcosine, there remains a formula exactly identical with that of urea:

From 1 eq. kreatine	$= C_8N_3H_{11}O_6$
Deduct 1 eq. sarcosine	$= C_6N\ H_7\ O_4$
There remains 1 eq. urea	$= C_2N_2H_4\ O_2$

"It is consequently obvious that, in the decomposition of kreatine by baryta, carbonic acid and ammonia are secondary products derived from the decomposition of urea." (p. 75.)

In further confirmation of this view, Liebig has not only ascertained that a solution of urea in barytic water is resolved by long boiling into carbonate of baryta and ammonia, with the same appearances as those occurring in the production of sarcosine, but further, that urea is present in the liquid when kreatine is boiled with baryta, if examined before the whole of the kreatine is decomposed.

Sarcosine and urea are not the only products of the decomposition of kreatine by baryta; another substance of a feeble acid reaction, but not combined with baryta, was observed; it crystallized in long colourless prisms or scales, very soluble in water and alcohol. The quantity was too small to admit of an ultimate analysis.

Inosinic acid. When the liquid from flesh has entirely deposited the crystals of kreatine, and is somewhat further concentrated by evaporation, if alcohol be added to it in small quantities till the whole becomes milky, it deposits in the course of a few days yellowish or white crystals of salts of inosinic acid. The solution of inosinic acid has "a strong acid reaction, and possesses an agreeable taste of the juice of meat." When evaporated, it yields a syrup which after weeks exhibits no signs of crystallization. If

this syrup be mixed with alcohol, the thick viscid fluid is changed into a hard, firm, pulverulent mass, of which alcohol dissolves only traces. From a concentrated aqueous solution the acid is precipitated in white amorphous flocculi.

From analyses of its baryta-salt the anhydrous acid appears to be represented by the formula $C_{10}N_2H_6O_{10}$, and if we suppose the baryta replaced by its equivalent of water the formula of inosinic acid will be

$$C_{10}N_2H_7O_{11} = C_{10}N_2H_6O_{10} + HO.$$

"Inosinic acid appears from its composition to belong to the coupled acids. Considered as a hydrate, it contains the elements of acetic acid, oxalic acid, and urea:

1 eq. anhydrous acetic acid	$C_4 \quad H_3O_3$
2 eq. anhydrous oxalic acid	$C_4 \quad O_6$
1 eq. urea	$C_2 \; N_2H_4O_2$
1 eq. hydrated inosinic acid	$C_{10}N_2H_7O_{11}$ (p. 84.)

Lactic acid is next shown to be a constituent of flesh, and the best methods of preparing and purifying it are given. The lactates of lime and zinc were prepared from this source, and submitted to ultimate analysis.

In a subsequent part of the work the purposes of lactic acid in the organism are made an object of inquiry. In the first place, it is shown that this acid does not occur in healthy urine, neither is it formed from it during the process of putrefaction; and, secondly, it cannot be detected in that secretion when taken internally as lactate of potash.

"From this it plainly appears that the lactic acid in the organism is employed to support the respiratory process, and the function performed by sugar, starch, and in general all those substances which, in contact with animal matter, are convertible into lactic acid ceases to be an hypothesis. These substances are converted in the blood into lactates, which are destroyed as fast as they are produced, and which only accumulate where the supply of oxygen is less, or where some other attraction is opposed to the agency of that element." (p. 103.)

The inorganic constituents of the juices of flesh are then discussed at considerable length. Liebig asserts that the ash of the juice of meat contains only alkaline phosphates and chlorides, there being no carbonates or sulphates present. The soluble salts contain the different modifications of phosphoric acid, which are easily distinguished by their action on nitrate of silver.

"The ashes of the juice of flesh, in the case of the ox, horse, fox, and roe-deer, give with water a strongly alkaline solution, which is precipitated, first white, then yellow, by neutral nitrate of silver; and the mixture, after complete precipitation, is perfectly neutral. This proves that the ashes contain salts of phosphoric acid, with 2 atoms (pyrophosphates), and with 3 atoms (tribasic phosphates) of fixed alkaline base..... .The ashes of the juice of the flesh of fowl give a different result. The aqueous solution precipitates nitrate of silver pure white; the ashes, therefore, contain alkaline pyrophosphates; and when they are acted on by nitric acid and again ignited, the soluble portion still precipitates nitrate of silver only white, although an additional quantity of alkali is then added to the phosphate originally present. From this it follows that the juice of the flesh of fowl must contain a certain, though small, quantity of alkaline phosphate, with 1 atom of fixed base (metaphosphate), since, otherwise, after the action of nitric acid on the ashes, a certain quantity of phosphate with 3 atoms of fixed base (tribasic phosphate) must have been produced, and thereby a yellow precipitate must have been formed, to a corresponding extent, in the nitrate of silver.

"The whole amount of alkalies, therefore, present in the juice of the flesh of the ox, horse, fox, and roe-deer is not sufficient to convert the phosphoric acid of the juice entirely into the so-called neutral salt, that is, the salt with 3 atoms of fixed base. In the fowl the whole of the alkali is not even sufficient to convert the phosphoric acid entirely into the salt with 2 atoms of fixed base." (pp. 97-8.)

But the juice of flesh even before all the phosphoric acid has been precipitated by baryta, at a period, therefore, when it can contain no baryta dissolved, acquires an alkaline reaction.

"Hence it follows, that the organic acids present in the juice, the lactic and inosinic acids, &c., taken together, are not in sufficient quantity to form neutral salts with the alkalies contained in it, potash and kreatinine; and this necessarily implies that the acid reaction of the juice of flesh is caused by the presence of acid salts of the alkalies with the three acids—phosphoric, lactic, and inosinic acids. Inosinic acid constitutes too small a part of the juice to allow us to ascribe to it a perceptible share in producing the acid quality of that fluid; and this acidity depends therefore on the presence of acid alkaline lactate and acid alkaline phosphate (phosphate with one atom of alkali), or, in other words, of neutral alkaline lactate and phosphate along with free lactic and phosphoric acids." (pp. 98-9.)

It is thus obvious that we have the condition of an electrical current present. There is an alkaline fluid circulating in the blood-vessels and lymphatics, while the surrounding fluid, that of the flesh, is acid, and the tissue of which the vessels are composed is permeable for the one or the other of these fluids. Liebig considers it "far from improbable that such a current takes a certain share in the vital processes, although its action be not always indicated by proper electrical effects." (p. 104.)

We now come to some very important points regarding the differences of the salts occurring in the juice of flesh, and in blood and lymph: while the former contains phosphate of potash and chloride of potassium, the latter fluids contain phosphate of soda and chloride of sodium. A method is given by which the relative proportions of potash and soda in the juice of flesh and in blood may be determined, and by which the materials for the following table were obtained:

	Potash in the blood.	Potash in the flesh.
For 100 parts of soda in the fowl . . .	40·8	384
,, ox . . .	5·9	279
,, horse . .	9·5	285
,, fox . . .	..	214
,, pike . . .	..	497

"It is hardly necessary to state that these numbers only express approximatively the proportions of potash to soda in the flesh, because it is impossible to obtain the juice of the flesh of the ox, horse, and fowl free from blood and lymph, fluids which contain much soda. Had it been possible to obtain the juice of flesh unmixed with blood and lymph, the proportion of potash to soda would have come out much higher, so much so, indeed, that the conclusion that salts of soda form no part of that fluid is not destitute of probability; and if, as is supposed, the lymphatic vessels possess the power of taking up the salts of soda which pass from the capillaries into the substance of the muscles, and returning these salts to the larger blood-vessels, the fact just mentioned admits of a very easy explanation. From the great difference of chemical nature and qualities in the fluids circulating in the different parts of the organism, it follows that there must be a very remarkable difference in the permeability of the parietes of the vessels for these fluids. Were this permeability in all cases the same, there must have been found as much of the salts and potash in the juice of flesh as in the blood; but the blood of the ox and the fowl contains nearly a third of its whole saline contents of chloride

of sodium, while hardly a trace of this compound occurs in the juice of flesh. The vessels which secrete milk must stand in a similar relation to the blood-vessels; for in the milk of the cow the salts of potash preponderate very greatly over those of soda, and are present also in much larger quantity than in the saline constituents of blood.

"In some pathological conditions there has been observed,* at points where bones and muscles meet, an accumulation of free lactic and phosphoric acids, which has never been perceived at those points in the normal state. The solution and removal of the phosphate of lime, and therefore the disappearance of the bones, is a consequence of this state. It is not improbable that the cause, or one of the causes, of this separation of acid from the substance of the muscle is this,—that the vessels which contain the fluid of the muscles have undergone a change, whereby they lose the power of retaining within them the acid fluid they contain." (pp. 107-8.)

The next point noticed by Liebig is *the importance of chloride of sodium to the formation of blood.* The constant preponderance of the soda-salts in the blood and the potash-salts in the juice of flesh, justifies the assumption that their presence in these fluids is indispensable. The question then arises what is the source of the soda-salts, especially the phosphate of soda found in the blood? The ox and the horse must obtain their inorganic constituents from the vegetables on which they feed, and analyses have indisputably proved that plants growing at a certain distance from the sea contain no soda, or only traces of that base, while they abound in potash-salts. The answer to the question regarding the source of the phosphate of soda in the blood must be sought for in the investigation of the action of the phosphate of potash (yielded by the food) on chloride of sodium (taken additionally).

"Phosphate of potash with two atoms of potash (tribasic phosphate of potash with two atoms of fixed base, and one atom of water) $= PO_5 \left\{ \begin{matrix} 2\ KO \\ HO \end{matrix} \right\}$, is deliquescent, hardly crystallizable, and has a very feeble *alkaline* reaction.

When we supersaturate phosphoric acid (tribasic) with potash, and evaporate to crystallization, a salt is deposited which has an *acid* relation $= PO_5 \left\{ \begin{matrix} KO \\ 2\ HO \end{matrix} \right\}$. There is no salt which loses half the amount of base it contains so easily as the phosphate of potash. If phosphoric acid be neutralized with potash, and chloride of sodium added to the solution, and the whole left to spontaneous evaporation, a phosphate crystallizes, which contains both potash and soda (the tribasic salt $PO_5 \left\{ \begin{matrix} NaO \\ KO \\ HO \end{matrix} \right\}$) while chloride of potassium is found in the mother liquid.

"It is obvious that phosphate of potash is decomposed when in contact with chloride of sodium; part of the potassium combines with the chlorine, while the sodium replaces it in the phosphate; phosphate of soda being produced.

"In this way we can understand the formation of phosphate of soda in the body of an animal which obtains in its food, along with phosphate of potash or earthy phosphates and salts of potash, no compound of soda except chloride of sodium; and when in inland countries the food does not contain common salt enough to produce the phosphate of soda necessary for the formation of the blood, then more salt must be added to the food." (p. 110-11.)

We regret that our limits will not permit of our entering on the explanation of Liebig's views regarding the uses of the phosphate of soda contained in the blood. We must content ouselves with the simple announce-

* Schmidt, Annalen der Chemie u. Pharmacie, vol. 61, p 329.

ment of his belief, that the blood owes its power of absorbing and giving off carbonic acid to the presence of this constituent.

The section concludes with some remarks on the relative proportions of lime, magnesia, and alkalies to the juice of flesh, and on the best modes of determining them.

SECTION THIRD.—*Practical application of the results of the foregoing investigation.* This must be regarded as the first attempt to treat cookery on chemical and philosophical principles. Our author commences with the subject of *boiling*. It is obvious that if flesh is boiled in a sufficient quantity of water and for a sufficient time, the fluid must extract soluble alkaline phosphates, lactates, inosinates, phosphate of magnesia, and slight traces of phosphate of lime; while the boiled flesh contains the insoluble inorganic constituents and the fibrine, fat, &c. In consequence of the importance of the soluble constituents to the organism, it follows "that boiled flesh when eaten without the soup formed in boiling it (the *bouilli* without the *bouillon*) is so much the less adapted for nutrition, the greater the quantity of the water in which it has been boiled, and the longer the duration of the boiling." (p. 123.) The smell and taste of roasted flesh arise from the soluble constituents of the juice, which have undergone a slight change under the influence of the higher temperature; it is also on the soluble matter that the flavour of soup and of the different kinds of meat depend.

The flesh of old animals contains little albumen, but much fibrine; in fact, the quantities of albumen and fibrine seem always to stand in an inverse ratio to one another.

"The tenderness of boiled or roasted meat depends on the quantity of the albumen deposited between the layers and there coagulating; for the contraction or hardening of the fibrinous fibres is thereby to a certain degree prevented. This quality, tenderness, however, also depends on the duration of the boiling, for the albumen also becomes harder by continued boiling without, however, assuming a tough consistence." (p. 125.)

The following is given by Liebig as the best method of boiling meat:

"If the flesh intended to be eaten be introduced into the boiler when the water is in a state of brisk ebullition, and if the boiling be kept up for some minutes, then so much cold water added as to reduce the temperature of the water to 165° or 158°, and the whole kept at this temperature for some hours, all the conditions are united, which give to the flesh the quality best adapted to its use as food." (p. 126.)

If, however, meat is to be boiled for the purpose of making soup, a very different course must be pursued. It should be placed in cold water, which must be *very gradually* brought to the boiling point. In this way there occurs from the commencement of the process an interchange between the juices of the flesh and the external water, which would be altogether impossible according to the directions we have given above for boiling meat, in consequence of the immediate coagulation of the albumen that in that case takes place in the vicinity of the surface, and prevents the penetration of water into the interior of the mass. Liebig proves, experimentally, that boiling water allowed to act for five hours on finely-chopped flesh does not dissolve more than the fifth part of the matters soluble in cold water, even after the albumen has been separated by heat-

ing the cold infusion. We cannot refrain from extracting Liebig's prescription for soup.

"When 1lb of lean beef, free of fat, and separated from the bones, in the finely chopped state in which it is used for beef-sausages or mince-meat, is uniformly mixed with its own weight of cold water, slowly heated to boiling, and the liquid, after boiling briskly for a minute or two, is strained through a towel from the coagulated albumen and the fibrine, now become hard and horny, we obtain an equal weight of the most aromatic soup, of such strength as cannot be obtained, even by boiling for hours, from a piece of flesh. When mixed with salt, and the other usual additions by which soup is usually seasoned, and tinged somewhat darker by means of roasted onions or burnt sugar, it forms the very best soup which can in any way be prepared from 1lb of flesh." (pp. 131-2.)

By the concentration of such a soup on the water-bath, till it ceases to lose water, we obtain a dark brown, soft mass, of which half an ounce suffices to convert 1lb. of water, with the addition of a little salt, into a strong well-flavoured soup. This is true portable soup, while that of commerce almost invariably consists of mere gelatine.

"Of the true extract nearly 80 per cent. is soluble in alcohol of 85 per cent., while the ordinary tablets of portable soup rarely yield to that menstruum more than 4 or 5 per cent. The presence of kreatine and kreatinine, the latter of which is instantly detected by the addition of chloride of zinc to the alcoholic solution, as well as the nature of the salts left on incineration, which chiefly consist of soluble phosphates, furnish sufficient data for judging of the quality of the true extract of flesh." (pp. 133-4.)

Liebig considers this extract of meat as most valuable for ships and fortresses, and in fact in all cases where fresh meat and vegetables are wanting, and the people are supported on salt meat.

Salted meat is not sufficient to preserve the organism in a condition of health, for the brine formed by rubbing the salt into the flesh contains the chief constituents of a concentrated soup or extract, namely, soluble phosphates (especially phosphate of potash), lactic acid, kreatine, and kreatinine. If these substances be not supplied from other quarters, the juices of the flesh cannot be retained in their normal state.

"A change in the quality of the gastric juice, and consequently in that of the products of the digestive process, must be regarded as an inevitable result of the long continued use of salted meat; and if, during digestion, the substances necessary to the transformation of that species of food be taken from other parts of the organism, these parts must lose their normal condition." (pp. 135-6.)

Liebig believes that meat acted on by salt containing chlorides of calcium and magnesium would probably be less injurious than when salted in the ordinary manner.

The only subject remaining for consideration in this section is the *gastric juice*. Lehmann's experiments proving that lactic acid is present in that secretion in dogs are admitted as perfectly decisive; in fact, our author is now as ready to find lactic acid, as he was a very short time ago to disprove its existence in the body.* He regards the gastric juice as similar to the juice of flesh, and he is consequently of opinion that his soup may, perhaps, admit of being employed as a valuable remedy in many cases of dyspepsia, with a view to increasing the activity of the stomach and promoting digestion.

* "In a normal state of health, *no lactic acid* is formed in the stomach." (Liebig's Animal Chemistry, &c., p. 112, 2d edition, 1843.)

The presence of free hydrochloric acid in this fluid is no longer requisite for the carrying out of his theories, and singular to relate, that acid no longer exists there. In the second edition of the 'Animal Chemistry,' published four years since, we are informed that "the presence of free muriatic acid in the gastric juice, first observed by Prout, has been confirmed by all those chemists who have examined that fluid since;" but now (June, 1847),

"when we recollect that lactic and phosphoric acids, at temperatures in which hydrochloric, acetic, and barytic acids are volatilized, are almost fixed, we can explain how it happens that, in many cases, hydrochloric acid, in others, acetic or butyric acid, has been obtained by distilling the gastric juice. Acetates, butyrates, and even chloride of sodium are decomposed by lactic acid, as well as by acid phosphates in these circumstances, and the occurrence of the one or the other of the more volatile acids must vary with the amount of the lactic or phosphoric acid present in the gastric juice, and the amount also of their salts in the same fluid." (p. 139.)

In the preceding pages we have given our readers an abstract of all the more important parts of this volume, which we deem the most valuable that has yet appeared from the pen of its illustrious author, in so far as concerns the application of animal chemistry to the actual practice of medicine.

We have alluded in one or two instances to the differences of opinion, on points of high importance, occurring in the present volume and in the last edition of the 'Animal Chemistry,' and we might have given more illustrations of a similar nature. We have done so with the view of showing that if, from the evidence afforded by his own writings, the first chemist of his age may fall into grievous errors, he should look with some degree of forbearance on those who have the misfortune to differ from him in points of comparatively minor importance. However, the points of controversy between himself and Mulder may be ultimately decided, his warmest admirers must ever regret the unaccommodating and hostile spirit he has exhibited. The following sentence from Mulder's defence* is strong: we do not say that it is altogether true; but the very fact that such opinions are entertained by others ought to make the great professor more cautious and less dogmatical in the expression of his own. "Freedom of scientific opinion has never been understood by Liebig. For years past a tribunal has been established in Giessen, before which Liebig is at the same time accuser, witness, public prosecutor, advocate and judge."

Art. XIX.

Practical Observations on some of the Diseases of the Stomach and Alimentary Canal. By James Alderson, M.D., F.R.S., late Senior Physician to the Hull Infirmary.—*London*, 1847. 8vo, pp. 208. With Ten coloured Plates.

The author of this volume, after having long enjoyed an extensive practice in the country, has recently withdrawn from its harassing duties and taken up his abode in the metropolis. In the comparative leisure thus temporarily created he has set himself to review his past experience,

* Mulder's Reply to Liebig, p. 6.

and the present volume is the first-fruits of the scrutiny. In a modest preface he tells us that he claims "neither the character of a discoverer nor of a theorist, nor of a proposer of any particularly novel or scientific plan of treatment." He has written, he says, "in the belief, or at least the hope, that the result of extensive observation, classified and reasoned upon, would be found useful." He may rest assured that his professional brethren will give him all credit both for his motives and his exertions, and will receive his book in the honorable spirit in which it is presented to them.

The work is divided into two Parts—the first, containing observations on structural, the second on functional diseases of the alimentary canal. The structural diseases noticed are—carcinoma of the pharynx, œsophagus, stomach, pylorus; malignant disease of the colon; encephaloid disease of the liver; hypertrophy of the stomach; perforation of the stomach and ileum. The functional diseases are, different forms of dyspepsia, and disorder of the colon.

The work consists mainly of cases with remarks, many of which are well illustrated by engravings. To such of our readers as are familiar with the special works on cancer in general, and with the various illustrated treatises on morbid anatomy, the volume of Dr. Alderson will convey little or no new information: as an authentic record, however, of cases of severe diseases, traced to their termination and beyond, and subjected to rational and enlightened treatment, it will deserve the attention of practitioners as well as of systematic writers and pathologists.

In Chapter VII. entitled "Carcinoma of the stomach, external to it, and involving other organs," we have a very good account of a form of malignant disease which is of frequent occurrence and yet often misunderstood. It is sometimes confounded with enlargement of the liver or spleen, often with the "tubercular accretion" of Dr. Baron, and we have known it confounded with aneurism of the aorta. We extract the brief account given of the pathology of this disease, and also a part of the author's observations on the diagnosis, symptoms and treatment; and with these extracts we shall close this brief notice.

"Deposit of carcinomatous matter may take place beneath, or on the free surface, of the serous membrane covering the stomach, and the neighbouring viscera (as the duodenum, the head of the pancreas, the omenta, the mesocolon, &c.): the deposit varies in consistence from the hardness of scirrhus to the softness of pulp. It is difficult, if not impossible, to assign a position to the earlier deposit: as it increases, however, lymph is effused, and adhesions are formed, by which contiguous parts are glued together; the viscera become bound to the spine, and to each other, by these adhesions, and, as a consequence, the healthy performance of their various functions is interfered with. The adhesions often present a dark smoky hue, and jelly-like appearance, and are easily torn through; the proximity of organs, rather than the similarity of function, seems to be the cause of their being involved. The order in which the parts are severally included in the diseased mass may be inferred from the succession of the symptoms which will be hereafter detailed; this order, as well as the number of organs finally included, varies in different cases. In some cases the disease proceeds until portions of the stomach, the duodenum, the pancreas, the colon, and even the liver, are all involved in one diseased mass, forming an undefined tumour. In the later stages of the disease ulceration finds its way into the duodenum, the stomach, or the colon. The ulceration has a peculiar character, differing from that which results when the primary seat of the deposit is in the submucous cellular tissue; the disorganising process is, perhaps, more properly expressed by erosion, which takes

place through the coats, where either hard or soft material of carcinoma is deposited.

"When the extension of the disease is to the duodenum, and perforation of the coats takes place, the orifice of the gall-ducts becomes involved, and jaundice supervenes; the caliber of the intestine becomes narrowed, and the stomach in consequence obtains a habit of unusual distension.

"The pancreas is ascertained generally by post-mortem examination to be included in the disease; and from its proximity it is to be inferred that it is simultaneously affected with the duodenum: there is no train of symptoms, however, with which I am acquainted to give certain notice of this organ being involved.

"When the extension of the disease is towards the stomach, similar erosion of the coats takes place as in the duodenum, and is followed by the same set of symptoms as have been enumerated as denoting ulceration of the mucous coat.

"When the extension is to the colon, the centre of the arch, from proximity, is the part attacked; the bowel at that point becomes narrowed, and there is a mechanical obstruction to the passage of the antecedent contents; the contractile power of the muscular coat is at the same time interfered with by the adhesions: the capacity of the whole tube to this point enlarges, and the bowel remains habitually distended, just as the stomach does by pyloric or duodenal interference. This state of the colon gives rise to the accumulation which will be noticed as a prominent symptom; diarrhœa or dysentery follow from the effects of ulceration through the mucous lining of the colon.

"The liver is secondarily affected by the deposit of encephaloid tubera, and in some cases subsequently becomes adherent to the diseased mass; the gall-bladder is usually distended with bile, and contains gall-stones. Secondary deposit also sometimes takes place in the lungs, though very rarely, but it does not proceed to a state to cause any very prominent pulmonic symptoms." (pp. 96-9.)

"Amongst the earlier symptoms of this disease, unsatisfactory relief from the bowels is the most prominent. It is experienced for a year or more before the more urgent symptoms set in, and is accompanied at intervals by nausea, retchings, and headache. Pain is also suffered, but not to any great extent, and it is referred by the patient to an unremoved accumulation in the bowels; it is described as situated at the pit of the stomach, or rather lower. The patient subsequently begins to lose flesh, the features shrink, the complexion becomes opaque, sallow, and exsanguineous; the eye looks sunken, and the strength begins to fail: great anxiety about his health now possesses the patient, who is often prone to seek various opinions, until he finds a voice ready to flatter him with the delusive promise of a cure." (p. 100.)

"Within three or four months of a fatal termination a careful examination of the abdomen usually reveals an unusual fulness a little to the right of the pit of the stomach; it is hardly felt as a tumour, but as an undefined resisting mass, and it appears to rest upon the spine: pressure by the hand causes pain; and, the same as in cases of ulcerated carcinoma of the stomach, I have several times noticed that the patient exclaims with satisfaction that the precise seat of the pain has been touched, and he generally adds his conviction that the cause will be removed. At this time loss of flesh and strength increases, the appetite fails entirely, there are vomitings and retchings: the nights are restless, attended with pain. Hiccup is a very distressing symptom, and increases both in violence and in duration of the fit as the disease advances." (p. 101.)

"It is obvious that, according to the organs included in the disease, their different functions will be interfered with. Amongst the most constant symptoms of the first stage of the complaint is the sensation of load in the bowels. Relief is only obtained by several daily evacuations, and the uneasy sensations again daily return. This partial relief may arise from the actual removal of a lodgment of feculent matter in the arch of the colon, or by the aperient acting as an antiphlogistic measure, by causing increased secretion from the mucous coat: but I am inclined to think, from the fact that the uneasy sensations recur again so soon

after copious action of the bowels, that the uneasiness is rather to be attributed to the presence of the carcinomatous deposit, and its progressing adhesions, than to accumulation within the bowel, to which the patient always attributes it.

"In every stage of the complaint the evacuations will give evidence of there having been accumulation in the colon, which will seem to call for the repetition of purgatives; but as the difficulty to be overcome is, after all, merely a symptom, it is necessary to be cautious in subjecting the patient to a severe discipline which could only be warranted on the supposition that the endurance would be rewarded by permanent benefit." (pp. 111-3.)

"At an early period of the disease the deposit is evidently confined to a small space; the future ramifications have yet to be determined; and the symptoms obviously vary according to the progress of the encroachment: thus hepatic symptoms become more prominent as the disease proceeds towards the duodenum and pylorus. Mechanical opposition to the food from the stomach begins, and vomiting comes on. The vomiting is not of the same kind as that in cases of ulceration of the mucous lining; it is full and free, as in scirrhous pylorus: the stomach becomes habitually distended, and admits of a large accumulation of contents, which are sometimes retained for a period of twenty-four hours or more, and then rejected all at once, without any accompanying nausea. The stomach, being comparatively in a sound state, craves for food immediately after it has got rid of the contents, and hence the voracious appetite which is sometimes experienced. The eagerness for food is also to be attributed to a sense of relief obtained by a full state of the organ which replaces the neighbouring viscera in their easiest relative position: in such a case, it is impossible to deny the patient the solid food which he desires." (pp. 115-6.)

ART. XX.

Ueber die Gesetze nach welchen die Mischung von Flüssigkeiten und ihr Eindringen in permeable Substanzen erfolgt, mit besonderer Rücksicht auf die Vorgünge im menschlichen und thierischen Organismus. Von JULIUS VOGEL.—Göttingen, 1846.

On the Laws regarding the Mixing of Fluids and their penetration into permeable substances, with special reference to the processes in the Human and Animal Organism. By JULIUS VOGEL.—8vo, pp. 42.

THIS is an excellent attempt to explain many of the obscure points in the process of secretion, by a reference to purely physical laws. It must be obvious to the most casual observer that many phenomena in the human body exclusively depend on the permeation of fluids through organic partitions, whilst in many others it plays a more or less important part. Hence the obvious necessity for a careful investigation of the law regulating the commixture of fluids through membranous partitions, such as occur in the human body.

"The food of which we partake passes first through the mouth, the fauces, and the œsophagus: all these parts are covered by a thick epithelium, which does not readily yield a passage to fluids. In the stomach, however, whose walls permit fluids to pass more readily through them, an energetic reaction takes place between its fluid contents and the blood in the gastric vessels, and a similar process is continued throughout the whole of the intestinal canal. The contents of the stomach are generally much more aqueous than the blood; the various kinds of drink and most articles of fluid food are naturally so, while the saliva and gastric juice reduce solid food to that condition. As is usually the case, where a concentrated fluid comes in contact with one that is more aqueous, there can be no doubt that here also a larger quantity of the thinner fluid passes through the membranous partition

to the concentrated fluid, than conversely. Thus by degrees a larger portion of the contents of the stomach and intestinal canal passes into the blood than is conversely yielded by the blood to the fluid of the digestive canal, as has been directly proved by the experiments of Poiseuille. We are still deficient in exact researches respecting the substances that pass from the blood-vessels to the contents of the intestinal canal; but they are probably salts with a small quantity of extractive matters and some protein-compounds, which latter are there converted into mucus. What is usually denominated gastric and intestinal mucus is doubtless the equivalent yielded in the act of digestion by the blood to the contents of the stomach and intestinal canal; but it must be understood that gastric and intestinal mucus may also be secreted by the blood, in the manner we shall subsequently consider under the head of secretions, without that fluid receiving anything in return. It is, however, arranged in a most wonderful manner, that nearly the whole contents of the stomach and intestinal canal may gradually penetrate into the interior of the organism without any appreciable quantity of matter passing as an equivalent from the blood to the intestinal canal. In the first place the acid character of the gastric juice seems to be of great importance. The experiments of Dutrochet show us that acid fluids, especially in their mixture with another fluid through an animal membrane, yield more than they receive; and thus the acid character of the contents of the stomach seems to be precisely the means by which resorption is promoted in a simple physical manner. It would, however, be highly desirable to institute a series of carefully conducted experiments on the influence which the acidity of the gastric juice thus apparently exercises on the resorption of the chyme." (pp. 25-6.)

Vogel then proceeds to illustrate the law, that "the more two fluids separated by an animal membrane differ from one another in the degree of their concentration, the more proportionally will the concentrated abstract from the thinner fluid." This law explains how it is that the passage of nutriment into the circulation is essentially facilitated by the liquidity of the contents of the intestinal canal, induced by the admixture of fluids much more aqueous than the blood, namely, the bile and pancreatic fluid, and further by the drink for which a desire is usually experienced after meal-times.

"The bile contains in itself much more water than the blood, containing on an average about 10 per cent. of solid constituents, while the blood contains more than 20 per cent. The bile, however, becomes yet more aqueous in the intestinal canal owing to a portion of its solid constituents (the bilate of soda) being decomposed by the acid of the chyme, and separated in part in a modified (insoluble) condition, as dyslysin, &c. It contributes therefore essentially to the dilution of the chyme, facilitating its resorption. The pancreatic fluid acts in a like manner, for although we know but little of its composition, it is at all events much more aqueous than the blood, its solid constituents being about 8 per cent. Another means acting with the same object is the circulation of the blood, the consequence of which is, that the portion of blood become more dilute by the absorption of aqueous constituents from the chyme is being constantly removed and replaced by more concentrated blood." (p. 26.)

The object of the lacteals is, according to our author, to abstract the chyle that is too concentrated to be taken up in the above manner, and to take up fatty matter.

"Fat does not mix with water, and as it can either not at all or only with difficulty pass through membranes moistened by watery solutions, it cannot penetrate in more than a very small quantity from the chyme into the blood-vessels. Fat may, however, penetrate from the intestinal canal into the lacteals as has been shown by experiments made on the chyle of animals, killed soon after partaking of fatty food. The penetration of fat through the intestinal walls into the chyle-vessels, is no doubt effected in the same way that oil penetrates through a filter moistened with water; thus in some parts of the filter the particles of oil

accumulate so much as to displace the water, the filter becoming saturated with oil at these points, and then forming as it were bridges for the passage of the succeeding oleaginous particles, which are forced into the lacteals by mechanical pressure in the same manner as the aqueous particles of the chyle. Fat, therefore, is not dissolved by the action of the digestive fluid, but the solution is effected by the heat of the body, and fatty mixtures whose fusion-point is higher than 104° F., are either not at all digested (that is to say, resorbed) or only very gradually dissolved by the agency of more fluid fat, subsequently taken into the system by the food. In the resorption of fatty food the surface of the intestinal canal divides itself as it were into two parts, of which one takes up the aqueous chyle and the other the fat. The digestion of fatty food requires a longer period than that of aqueous food, owing to the time required by the fat to force the water from the walls of the separate villi. The resorption of aqueous fluids is rendered more difficult when fat has been previously taken on an empty stomach, for the surface of the intestinal canal becomes invested with a coating of oil which hinders the passage of water. This explains the reason of the inconvenience experienced on partaking copiously of water after eating fatty food, and in like manner why the effect of intoxicating drinks (as beer, for instance) may be retarded for some time by taking a few spoonfuls of oil on an empty stomach." (pp. 28-9.)

Our author then proceeds to explain on similar grounds why it is that the intoxicating effect of a given quantity of alcohol depends in a great measure on the degree of dilution, and to notice the cause of the purgative effect of saline solutions; but as the latter of these views has been prominently brought forward by Liebig and Dr. Golding Bird, we proceed to notice the concluding pages of his pamphlet devoted to the subject of secretion. After stating his views regarding the secretion of sweat, tears, seminal fluid, mucus, and saliva, he gives the following account of the action of the glands of the stomach:

"The gastric glands present certain peculiarities: the product of their secretion so far coincides with the saliva, that it consists of an aqueous fluid with salts and extractive matters, whilst the remains of dissolved gland-cells testify to the presence of protein consumed and modified by organization. But the gastric juice exhibits the peculiarity of containing a free acid. The manner in which this acid is conveyed to it is so much the more difficult of explanation that we do not even clearly know its chemical character, for while it was formerly regarded as hydrochloric acid, it is supposed by more recent investigators to be lactic, combined with a little phosphoric acid; which latter, however, simply owes its origin to the action of the lactic acid upon the alkaline phosphates simultaneously present. By what means is the presence of the free acid brought about? The answer to this question is difficult, and many conjectures may be hazarded on the subject. The acid undoubtedly owes its origin to the blood, and the process of its formation may be owing to the decomposition of some of the salts (alkaline lactates or chlorides) of that fluid by the cells of the gastric glands which retain the acid, while the alkali returns by diffusion into the blood. As the cells gradually become broken up and dissolved, the acid is liberated and mixes with the gastric juice." (pp. 37-8.)

We regret that our limits will not permit us to follow our author through his remarks on the secretion of bile and urine; but we trust we have already stated enough to impress on our readers the importance of the phenomena of the diffusion of fluids within the body; for it is only by a clear understanding of the laws regulating these phenomena and those of molecular chemical actions, that we can ever hope satisfactorily to elucidate the various metamorphoses of matter ever occurring in organised beings.

PART SECOND.

Bibliographical Notices.

ART. I.—*Experimental Researches on the Post-Mortem Contractility of the Muscles, with Observations on the Reflex Theory.* By BENNET DOWLER, M.D.—*New York*, 1846. 8vo, pp. 39.

THE experimental researches contained in this paper (which is reprinted from the New York Journal of Medicine) are of much interest; as they relate to a phenomenon which we have little opportunity of witnessing on this side of the Atlantic. Many of our readers will remember that, after death from the Asiatic cholera, very marked contractions of the muscles, producing slow but energetic movements of the limbs, not unfrequently presented themselves. These appear to be of frequent occurrence after death by yellow fever; and may occur either spontaneously or in obedience to a stimulus applied to the muscle itself. The following is a description of a well-marked case of the former kind, the subject of which was an Irishman, aged twenty-eight. Upon the cessation of the respiration, the body became quiescent and motionless in every part; but soon afterwards the following series of movements commenced. First he carried his left hand, by a regular motion, to his throat, then to the crown of his head; the right arm followed the same route on the right side; the left hand was then carried back to the throat; thence to the breast, reversing all its original motions; the right hand and arm performed exactly the same motions; in all, eight consecutive, identical, equable motions, in apparently equal times, and with a regularity indicative of volition, though before death he was totally insensible.

Where, however, such movements do not occur spontaneously, they may be excited by simple percussion of the muscle with the hand, with a stick, the flat side of a hatchet, or any other blunt instrument, as in the following case:

"R. C., a Kentuckian, aged 25. In two hours after death, when the arm was extended to an angle of 45° from the trunk, and was struck with my hand, or still better with the side of the hatchet, carried his hand to his epigastrium; but when the arm was extended upon the floor, so as to form a right angle with the body, he slapped himself upon the mouth and nose. The contractility began to decline in the third hour; and by the fourth hour all motions of the limbs ceased, though the pectoral muscles assumed the ridgy or lumpy form when percussed. An hour after death the thigh was moderately contractile. The left heel hung down near the floor; its flexors, after being struck, drew up the heel against the buttock. Five hours after death, the contractility had ceased and rigidity prevailed."

A number of similar experiments, nearly all of them made upon yellow-fever subjects, are related by Dr. Dowler; and in some of them it was proved, by completely separating the limbs from the body, that the influence of the nervous centres is not required for the production of these

motions. It is remarkable that in all the cases mentioned, the heat of the body remained at a remarkably high standard for some time after death; and according to Dr. Dowler, it even rose in some instances after the cessation of the respiration. In the case just quoted, the heat continued above the usual standard of health for seven hours after death; only sinking in that time from the extraordinary elevation of 111° to 102°.

We do not see in these phenomena anything which need be a source of perplexity to the physiologist. They obviously result simply from the contractility of the muscle itself, called into action by the mechanical stimulus directly applied to it. Several circumstances appear to favour the manifestation of this property in an unusual degree; especially the high temperature of the body previously to death, and the subsequent retardation of its cooling. This will tend to preserve the vital properties of the muscle; which will manifest themselves the more independently on account of the complete cessation of all nervous influence. The experiments are well worthy of being repeated in this country; for although we do not apprehend that such marked results can be obtained after death from other causes, as Dr. Dowler has encountered in his yellow-fever subjects, still we have little doubt that this post-mortem contractility will be found to be much greater than is usually supposed.

The "Observations on the Reflex Theory," which form the remainder of the pamphlet, are to us utterly incomprehensible. The author declares that he is altogether unable to understand the doctrine of reflex action; and instead of arguing upon it, he puts it to that very worst of all the tests of truth,—ridicule:

"Whatever agency," he says, "the anterior roots may have when galvanized, they are incomparably inferior to the motor power of the muscle when excited by the hand, the hatchet, or a cane. A hand-, or a hatchet-, or a cane-theory ought to have the preference over the reflex-theory, as the former is not only the most simple but the most scientific, affording a nearer approximation to the known physiological functions of man."

Let Dr. D. explain by his "hand-, hatchet-, or cane-theory," the contraction of the muscles of the *thigh* in a decapitated frog when the *web* is pinched, or the movements of the *legs* of a paraplegic man when the sole of the *foot* is tickled; and the champions of the reflex-theory may then enter the lists with him. But at present he is confusing two sets of phenomena wholly distinct; and his views would make the nervous system of no use whatever.

ART. II.—*Practical Observations on the Pathology and Treatment of certain Diseases of the Skin, generally pronounced Incurable. Illustrated by upwards of Forty Cases.* By THOMAS HUNT, M.R.C.S., &c. —*London*, 1847. 8vo, pp. 156.

MR. HUNT writes too much like one who indulges in the luxury of a hobby-horse, to gain assent to the whole of his views as to the power of arsenic in cutaneous diseases. He writes, however, in an earnest, truthful manner; and, making all allowance for the strong bias he exhibits towards his favorite remedy, the facts and views he brings forward eminently merit the attention of the practitioner.

The preparation of arsenic used by Mr. Hunt, is that commonly termed

liquor arsenicalis, or Fowler's solution, the liquor potassæ arsenitis of the Pharmacopœia. With this remedy he has cured several forms of inveterate cutaneous disease, prurigo of the anus, scrotum, and pudendum, lepra, psoriasis, urticaria, impetigo, eczema, acne, sycosis, lupus exedens and nœvus araneus, are those in which he has found it successful, and some most obstinate cases are detailed as yielding to its powers.

The value of Mr. Hunt's publication consists rather, however, in the experience he has accumulated and given in it, as to the real effect of arsenic on the system, and as to the modes in which it should be administered. He advises, for example, that full doses should be administered at the first onset, as five minims of the solution thrice a day, and then that the doses should be diminished. The time for lowering the dose is indicated by the supervention of conjunctivitis: when a pricking sensation is experienced in the tarsi, the eyes suffused with tears, and the conjunctiva inflamed, the system is under the influence of the remedy, just as ptyalism indicates the full action of mercury. The skin on the trunk of the patient is also affected with slight inflammatory action, and subsequently the extremities, on a continued use of the medicine. This appears as a faint pityriasis, and all those parts of the surface protected from the light have a brownish, dingy, unwashed appearance. If this state be kept up, the disease will vanish just as rapidly as if the conjunctiva were kept sore.

Another observation (and one we think applicable to many other remedies besides arsenic) is, that the remedy is much less likely to affect the stomach or bowels if taken during a meal. With this we fully concur. The tolerance of the remedy varies much in different individuals. One patient was cured of a psoriasis guttata, *by a fourth part of a minim* of the solution taken thrice a day, or only $\frac{1}{480}$ of a grain of white oxide of arsenic.

We had noted some of the incidental results of the arsenical course to which Mr. Hunt submitted his patients, some of which were continued for two years. In several it appears to have acted as a vigorous tonic. We must, however, recommend the practitioner to read the book for himself; it will repay the trifling cost of time.

ART. III.—*The Ancient World; or Picturesque Sketches of Creation.* By D. T. ANSTED, M.A., F.R.S., F.G.S.; Professor of Geology in King's College, London, &c. &c.—*London*, 1847. Small 8vo, pp. 408, with numerous Wood Engravings.

WE know few instances in which a book has been more completely adapted to supply an existing want, than the one now before us. Every educated man in the present day is expected to know something of the past history of the globe on which we live; but the difficulty of obtaining a general and philosophical view of that history from any treatises previously extant, without having the attention distracted by a mass of details which are valuable only to those who are systematically pursuing the subject, must be well known to all who have made the attempt. Now the object of this treatise of Professor Ansted's is to communicate in a simple form to the general reader the chief *results* of geological investigation; more especially such as have reference to the living inhabitants

of the earth at different epochs. A succinct outline is given of those great physical changes, which have produced that diversity of conditions to which we owe the successive extinction of certain races and the opportunity for the introduction of others; but the chief attention is bestowed upon the probable characters and habits of the denizens of the globe at different periods, as deduced from the study of those fossil remains which have been preserved to our own time. The most interesting of these are described in such a manner as to bring them before the mind's eye, with much of the vividness of conception which we derive from the accounts of some new region explored by an adventurous traveller, who has personally witnessed what he depicts. And although it may seem as if it were necessary to draw somewhat upon the imagination in such delineations, yet we believe that Professor Ansted has in no instance gone beyond the most legitimate deductions from the wonderfully complete evidence, which a careful examination of the records of the ancient world brings under the review of the Geologist. As he justly remarks, there are not wanting other instances in which conclusions perfectly satisfactory have been arrived at, and histories prepared, without the existence of any written documents whatever. The domestic manners of the ancient Egyptians are now known to us fully as well as those of their existing representatives, through the medium of those pictorial delineations of themselves which they have left us in such abundance; and from similar delineation, we are learning more and more of another ancient people, the Etrurians, whose very existence could only be inferred with hesitation from direct historical testimony, but of whose high civilization and proficiency in the arts of life, at a period long anterior to that usually assigned to the foundation of Rome, new evidence is continually being acquired, especially from sepulchral monuments. And if we look with interest to the discoveries which are being made from time to time, with reference to the past history of fragmentary portions of our own race, we surely need not entertain a less degree of curiosity with regard to those gigantic changes of which our globe has been so often the subject, previously to man's introduction upon its surface; whilst the novelty of the forms of animated beings which have been its successive tenants, their evident adaptation to conditions of existence very remote from those which now prevail, and the abundant materials for speculation as to the structure of those parts which are lost to us, present one of the most attractive fields that can be conceived for the wholesome and pleasurable exercise of the mind.

We cannot too strongly recommend this volume to our readers; its exterior is elegant, and its interior attractive to a superficial glance; and we are confident that its contents, when carefully studied, will not disappoint the first impression.

Art. IV.—*A Lecture, introductory to a Course of Clinical Medicine, delivered in the Theatre of Queen's College, Birmingham, &c. &c.* By Samuel Wright, M.D., &c.

An interesting brochure, and containing sound truths well expressed. We doubt not, that as Professor of Clinical Medicine, Dr. Wright will bring his talents and energy to bear upon his duties, and the profession as well as his class will benefit by his labours.

Art. V.—*Hygrometrical Tables, to be used with, and Description of the Dry- and Wet-Bulb Thermometers.* By James Glaisher, Esq., of the Royal Observatory, Greenwich.—*London*, 1847. 8vo, pp. 45.

Those who are interested in medical topography are well aware how scanty our knowledge is of the humidity of the air. The quantity of rain which falls, and the number of rainy days, are nearly all the data we have at present of the humidity of most climates; and yet the influence of this condition on the human body is confessedly great, and any exact information promises much benefit. On this account we call attention to this pamphlet, which describes a more simple method of ascertaining the principal facts connected with the moisture of the atmosphere than has yet been adopted. The hygrometers made of catgut, wood, hair, whalebone, &c. are easily deranged, and cannot be relied on. Daniel's hygrometer requires the purest kind of ether, and much experience to use it effectively. The instrument now proposed is merely a common thermometer, the bulb of which is covered with muslin and kept wet with cold water by a simple contrivance. A second thermometer, with a dry bulb is necessary, as the humidity of the air is ascertained by comparing the temperature of the one with the other. If the air is saturated with moisture, no evaporation takes place, and there is no difference between the two thermometers; but if the air is dry evaporation takes place, cold is produced, the wet-bulb thermometer sinks, and its fall is in proportion to the dryness of the air. Having ascertained the two temperatures, that of the dry, and that of the wet-bulb, the observer has only to consult the tables calculated by Mr. Glaisher, and he at once ascertains various particulars regarding the humidity of the air.

For instance:—If the temperature be alike in both, then the air is saturated with moisture; but if the dry-bulb is 70° and the wet-bulb 55°, then the table shows that the degree of humidity is 0·470, that of complete saturation being represented by unity. Another column shows that under these circumstances the weight of the vapour in a cubic foot of air is 3·76 grains, and as it takes 8 grains of water to saturate a cubic foot of air, there will be required 4·24 grains of water more, to complete saturation. Another column gives the dew-point, and a fourth the height of a column of mercury, which the vapour will support.

The application of this instrument to the sick room is obvious. If the air is very dry, the difference between the two instruments will be great; if moister, there will be less difference, and when the air is saturated with moisture there will be no difference at all. If the difference between the two is very great, it may be necessary to make a moister atmosphere by exposing water in shallow vessels; or if a more rapid effect is required, by using heated water until the air is sufficiently moist, and then by replacing it with cold. If the air is too moist, or a dry air is needed, all water should be removed or covered, and some substance, like sulphuric acid which rapidly absorbs water, should be placed in the room until the requisite dryness is obtained.

Mr. Glaisher alludes to the value of information which may be obtained by this instrument, as to the humidity of the air in various places and times, in connexion with the mortality, and in addition to the Reports on Mortality of the Registrar-General. On this account, as well as for its use

in the sick-room, we give this slight notice, referring those who are likely to take the necessary pains in registering the particulars of the climate in which they reside, to Mr. Glaisher's pamphlet, which, besides the tables, contains very full directions for the employment of the thermometer, both in a wet and dry state. The papers on Medical Topography, in the Transactions of the Provincial Medical Association, indicate that there are many careful observers scattered over the kingdom, who would rejoice in any practical hints as to the best mode of conducting their inquiries. We would suggest, that such careful daily records might be best kept by many of those who are retired from the more active duties of their profession (either nolentes or volentes), many of whom might find in such constantly and regularly recurring observations requiring accuracy and time, a stated employment, whilst the reductions of these, so as to obtain some useful results, would satisfy and keep in practice the higher powers of their minds.

ART. VI.—*Education and Educational Institutions considered, with reference to the Industrial Professions and the present aspect of Society.* By the Rev. J. BOOTH, LL.D., F.R.S., M.R.I.A., &c., formerly Principal of Bristol College, and Vice-Principal of the Liverpool Collegiate Institution.—*London,* 1846. 8vo, pp. 108.

WE commend this essay to the special consideration of all who are interested in the subject of which it treats,—and who among our readers is not?—the education of the middle classes. It might be almost regarded as an expansion of our own article on the Preliminary Education of the Medical Student (Vol. IX); and did we not happen to know that Dr. Booth was unacquainted with that article, until after the publication of his treatise, we might have got up a very plausible charge of plagiarism against him. We need scarcely add, therefore, that Dr. Booth's views, expressed in a clear logical style, with much felicity of illustration, have our hearty concurrence upon almost every point; and they are, of course, the more valuable, as coming from a gentleman of greater practical acquaintance with educational matters than falls to the lot of most men. Did our space permit, we should have been glad to present our readers with a few extracts, which point out, in a very striking manner, the insufficiency of the system of education generally pursued in our public schools to meet the wants of the present age, and the importance of substituting the study of the exact sciences for a portion of the very imperfect though prolonged course of instruction now given in classics. Upon these points, however, we cannot now dwell; nor shall we grapple with the question of the degree in which the Government of this country ought to support and regulate such institutions as those with which Dr. Booth has been connected. We must remark, however, that we cannot but regret to find Dr. Booth still so far trammelled by class prejudices, as to give what we must regard an unfair representation of the leading doctrine of the 'Vestiges of Creation' (p. 69, *note*). His acquaintance with philosophical history, however, has enabled him to prove the curious fact—that the general idea was very clearly stated by Seneca; an "anticipation" which was not a little remarkable in the then state of science.

Art. VII.—*Observations on the Connexion between Famine and Fever in Ireland and elsewhere.* By Henry Kennedy, a.b., m.b., &c.—1847. Pamphlet, 8vo, pp. 50.

These observations are a reply to Dr. Corrigan's pamphlet, on Famine and Fever as Cause and Effect in Ireland. Dr. Corrigan's facts are analysed and shown to have a different meaning than that which he has drawn from them, while his doctrine, that famine is the prevalent cause of fever in Ireland, is shown to be untenable. It is evident that Dr. Kennedy has the best of the argument, partly because he takes a wider view of the causes of epidemical diseases, partly because he has apparently a more philosophical turn of mind than his antagonist, and partly because Dr. Corrigan's position was one of the most untenable he could take up.

Dr. Kennedy maintains that the use of bad or deficient food injures the health by weakening the vital powers, and if the quantity be too small to maintain these diminished vital powers, then death results. This unhealthy condition predisposes to the attacks of diseases, and of fever amongst the rest, and if an epidemic be prevalent will increase its fatality, and prevalence, like any other debilitating cause. The destruction of vegetable life causing the famine, is dependent on the same cause as the epidemic itself; that, in fact, there is an "epidemic constitution" which involves all organisms within its influence, so that not only are these epidemical diseases amongst men, but domestic and other animals suffer with them. All this is, we think, very reasonable and in accordance with the views of the best writers on the subject.

It is manifest that a proper understanding of the meaning of terms used, is of first importance in a discussion of this kind. We are sure Dr. Corrigan would grant the utmost importance in the etiology of the Irish epidemic to the habits of the Irish people. In what other country in the civilized world do we read of corpses being left unburied in the filthy hovels in which the inhabitants have died? of the dead being devoured by dogs, simply because their graves are indolently made so shallow as to be within a few inches only of the surface? of the awful crowding in the hovels and workhouses? of the piling up of coffins outside a union workhouse, containing two, three, or four corpses each; remaining there for a fortnight unburied, and sweltering in putrefaction and decay? There is not a single practitioner in England and Scotland, who, with such facts before him, would say that a want of food was the cause of fever in a country like Ireland, and among a people like the Irish. Dr. Corrigan will be justified in stating that famine causes the over-crowding of the hovels, hospitals, and workhouses, but surely there are more direct causes of fever *continually* existing in Ireland than an empty stomach.

The inundation of the starving population of Ireland has convinced the inhabitants of Great Britain that the masses have no regard to personal cleanliness, or the decencies of civilized life. So long as the Irish continue in that condition, every successive epidemic must necessarily be destructive, and the well-meaning philanthropic efforts of men like Dr. Corrigan remain unregarded.

ART. VIII.—*An Account of the late Epidemic of Scarlatina in Newcastle and its Neighbourhood.* By EDWARD CHARLTON, M.D. Edin., &c.—1847. Pamphlet, 8vo, pp. 62.

DR. CHARLTON has laudably assumed the office of historian for Newcastle, in so far as relates to the epidemic of scarlatina, which prevailed more extensively in that town during the summer and autumn of 1846, than at any period since 1778, when Dr. John Clark recorded its progress and characteristics. The well-written tract before us professes to contain little of novelty, and adds rather to our experience than our science. Its principal defect is the absence of numerical data, and of a more minute comparison of the relations which the origin and progress of the epidemic bore to the meteorological phenomena. General statements are becoming less and less admissible in medical writings. There are, however, some interesting facts detailed, and it is probable that Dr. Charlton has made the best use he could of the materials at his disposal. In turning to the head of "Cause," we do not find a word as to the influence of famine; other more efficient febrific agents are, however, noticed.

"The fever was not confined to the ancient and low-lying streets along the water-side on either bank of the Tyne, it spread through the streets and courts higher above the level of that river, especially to those recently built houses inhabited by numerous families, about Arthur's Hill, to the densely-crowded courts and narrow lanes at the foot of Westgate street, while in Gateshead its ravages were greatest in the wretched purlieus of Leonard's court, and the cottages about Busy Burn." (pp. 24-5.)

Arthur's Hill, and Leonard's Court, so distinguished for scarlatina, are in an Irish condition. Impassable quagmires, unpaved streets, "four or five beds in one small apartment, lighted by a single window which is rarely opened;" such is a picture corresponding very graphically to the localities of fever in Ireland. It is clear that while we think of the poor man's stomach, we must not forget his lungs.

ART. IX.—*Hülfstabellen zur Erkennung zoochemischer Substanzen.* Von Dr. FREIHERRN E. VON BIBRA.—*Erlangen*, 1846.

Tabular Aids to the Recognition of Zoochemical Substances. By Dr. F. E. VON BIBRA.—*Erlangen*, 1846.

DR. V. BIBRA has presented us, under this title, with four enormous sheets containing the reactions of all the important proximate constituents occurring in the animal body.

These tables give the chemical tests, physical characters, ultimate analyses, &c., of the following groups:

1. *Protein-compounds;* albumen, fibrin, casein, globulin, tritoxide of protein, binoxide of protein, and protein itself.
2. *Blood-pigments;* hæmatin, hæmaphæin, and hæmacyanin.
3. *Extractive matters;* those of flesh, blood, milk, and urine; osmazome, creatin.
4. *Gelatin;* glutin and chondrin.
5. *Horny matter;* keratin and chitin.
6. *Urine;* urea, uric acid, hippuric acid, uric oxide, cystin, kyestein, and allantoin.

7. *Urine-pigments;* uroerythrin, cyanurin, melanurin, uroxanthin, uroglaucin, and urohodin.

8. *Bile;* bilate of soda.

9. *Bile-pigment;* biliverdin, biliphæin, and bilifulvin.

10. *Ptyalin, pyin, and pepsin.*

11. *Fats;* stearin, margarin, olein, serolin, cholesterin, butyrin, castorin, cetin, aethal, hircin, and phocenin.

12. *Brain-fats;* cerebrot, cephalot, cerebrol, and stearoconot.

13. *Sugar;* diabetic sugar and milk-sugar.

14. *Organic acids;* acetic acid, lactic acid, oxalic acid, lithofellinic acid, and bezoaric acid.

15. *Gases;* oxygen, hydrogen, nitrogen, carbonic acid, carburetted hydrogen, sulphuretted hydrogen, and phosphoretted hydrogen.

16. *Sulphur and Phosphorus.*

17. *Inorganic acids;* hydrochloric acid, sulphuric acid, nitric acid, hydrofluoric acid, carbonic acid, phosphoric acid, silicic acid, and hydrosulphocyanic acid.

18. *Bases;* soda, potash, ammonia, lime, magnesia, alumina, iron, manganese, arsenic, and copper.

We extract his remarks on albumen as affording a fair illustration of his mode of treating his subject:

"*Albumen* (fluid). In all substances conveying fresh material for the nutrition of the organism, in blood, lymph, chyle, eggs, &c., also in morbid products. It coagulates in a tolerably concentrated solution in flocculi at 75° [169° F.], while in a dilute solution it gives rise to opalescence. Nitric acid, corrosive sublimate, and nitrate of the protoxide of mercury cause precipitates in extremely dilute solutions. Acetic acid does not precipitate it, but a precipitation occurs on the addition of ferrocyanide of potassium to a solution faintly acidulated by acetic acid. Alkalies prevent its coagulation by heat. Fluid albumen may be dried without assuming the coagulated state either *in vacuo*, or at a temperature not much exceeding 30° [86° F.] It is again soluble in water. Coagulated albumen: soluble in acetic acid, and again precipitable by exact neutralization with an alkali. It is thrown down from its acetic-acid solution on the addition of ferrocyanide of potassium. Coagulated albumen becomes blue if kept for some time in contact with concentrated hydrochloric acid."

Three ultimate analyses of albumen from different sources are appended.

Suspended against a laboratory wall, these tables will undoubtedly be found useful by students of animal chemistry in the early part of their studies. Tables three feet long and two feet broad cannot be conveniently consulted in any other manner.

Art. X.—*Body and Soul; or Life, Mind, and Matter, considered as to their peculiar nature, and combined condition in living things; with a view to render the Physiology of Life and Mind more easily understood by the general reader.* By George Redford, M.R.C.S. &c.—*London*, 1847, 8vo, pp. 232.

The title of this work did not excite in us any very favorable prepossession as to its character; since it appeared to indicate one of those attempts to obtain the attention of the public by a sort of clap-trap advertisement, which are so frequently made by half-educated pretenders; or one of those ambitious essays at philosophical composition, which serve to indi-

cate nothing else than the shallowness and self-sufficiency of their authors. After an examination of its contents, however, we feel it right to state that the book cannot be justly referred to either of these categories. It contains a good deal of matter which will be novel and interesting to the general reader, gleaned from the writings of Müller and other physiologists and psychologists; and this is arranged in a manner that will no doubt be attractive to such as have not previously thought deeply on the subjects which the work embraces. Although we cannot give it very high praise, yet, on the other hand, we do not feel called upon to pass a severe judgment upon it. The author enters into his subject in a good spirit; that of endeavouring to convey some definite information upon it to those "who are sometimes mystified or shallowly enlightened by the confident explanations of materialists, over-chemical chemists, phrenologists, and mesmerists." He laments, not unreasonably, that so little can be said in explanation of its mysteries; "still," as he justly remarks,

"It is generally useful to know what there is that we do *not* know, and how far explanation may be carried, especially that we may avoid that too common disposition to throw a veil of mystery, and a sort of almighty incomprehensibility, over subjects which are really perhaps capable of reduction to the effect of general laws, without the possibility of detracting from that reverence which belongs to the mysterious omnipotence of the Deity."

The work consists of three parts: the first treating of *Life*, the second of *Mind*, and the third of the *Combined Phenomena of Life and Mind*. Notwithstanding our disagreement with the author on many points, we regard his treatise as on the whole well adapted to his object, and as likely to do good by the information it imparts and the incitements which it offers to a deeper study of the phenomena of which it treats.

ART. XI.—*Metastasi Riprovate dalla Struttura dei Tessuti e dalle funzioni dei Medesimi.* Tratto di G. B. BELLINI, Maestro Operatore in Santa Maria Nuova.—*Firenze*, 1845.

Metastasis Disproved by the Structure and Functions of Tissues. By G. B. BELLINI, Operating Surgeon in the large Hospital of Florence.—*Florence*, 1845.

OF this work one fasciculus only has reached us. Its object is to prove that what is generally considered to be metastasis is not so, but a new disease generated in the part supposed to be the seat of metastasis; that the functions of the absorbent and circulatory systems would explain the fact of morbid matter being taken from one part and affecting the general system, but not setting up a disease confined to one organ or tissue; that many supposed instances of metastasis are contemporaneous attacks in different parts, or mere extension of disease; that others are simple instances of reflex nervous irritation, or excitement of some organ which has to do the duty of another, whose functions are interfered with by disease.

It would be unfair to criticise the reasoning of Dr. Bellini before the completion of his work. Our readers can form their own opinions of its nature and value from the statement we have just given of its objects.

THE

BRITISH AND FOREIGN

MEDICAL REVIEW,

FOR OCTOBER, 1847.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

Handbuch der rationellen Pathologie. Von Dr. J. Henle, Professor der Anatomie und Physiologie in Heidelberg. Erster Band. Einleitung und allgemeiner Theil. Zweite unveränderte Auflage.—*Braunschweig*, 1846.

Manual of Rational Pathology. By Dr. J. Henle, Professor of Anatomy and Physiology in Heidelberg. First Volume. Introduction and General Part. Second and unchanged edition,—*Brunswick*, 1846. 8vo, pp. 357.

Professor Henle is well known as one of the most distinguished of the distinguished anatomists and physiologists of Germany. As a pathologist, also, he was introduced to our readers some years ago. Still continuing his attention to pathology he has, in conjunction with his colleague Professor Pfeufer, for some time past conducted a journal (Zeitschrift für rationelle Medicin) for the reception of such researches and observations as turn to account the recent progress of pathological anatomy, organic chemistry, physiology, and general anatomy in elucidating the nature of disease and the action of remedies; and now we have from his pen the work before us.

This work, the author tells us in his preface, is an attempt to arrange, in that form which the systematic genius of the Germans requires, the physiological facts which observation of the diseased body has brought to light, together with the theories and hypotheses to which they have given rise, in order to assign to them their place in the history of the development of science.

The fact that, soon after the appearance of this first volume of the work, a second edition was required, affords evidence of the high estimation in which the author's labours are held by his professional countrymen. Participating in this estimation and thinking the work calculated to clear the medical mind of a great deal of misconception and prejudice,

we bring it under the notice of our readers by means of a pretty full abstract.

In the introduction the first subjects discussed are the *Methods in Medicine*. Two methods strive for the preference, viz. the *empirical* and the *rational*.

The first method describes the symptoms merely, without inquiring into their cause and intimate connexion ; its names are not definitions, but only proper names, and when it uses a word, as, for example, inflammation, it means by it nothing more than the combination of the symptoms of redness, heat, swelling, and pain. The adherents of this method trouble themselves as little with the mode of action of the causes and the remedies as with the conditions on which the symptoms depend. As to remedies they use those which have most frequently been followed by good effects. As in this case the certainty of the methods of cure is not determined by internal arguments, but only by the number of observations the practitioner has made, everything depends on collecting and making use of as extensive a series of observations as possible. Clinical experience is here the rule of treatment. Such is the *empirical method*.

The second method—the *theoretical*, *physiological*, or *rational*—considers the symptoms in their dependence on each other, and in their connexion with internal changes ; and these changes it views as the effects of external influences on the organic structure endowed with peculiar powers. In defining a disease, instead of the symptoms, it describes the state which it believes it has ascertained to be that on which the symptoms depend. It employs names which express the nature of the morbid changes, preferring, for example, such names as "increased plasticity," "hyperæmia," "stasis," &c., for inflammation, according to the prevailing opinions regarding the essential nature of that morbid process. As to remedies, theoretical medicine investigates their absolute powers and properties and their so-called physiological action, i. e. the mode in which they affect the substance and forces of the organism.

The diagnosis in empirical medicine, like that in the descriptive natural sciences, teaches nothing more than that the particular case, from its external marks, comes under this or that class of known cases ; the diagnosis in theoretical medicine is the synoptical and independent history of the particular case. To the empirical physician a definite combination of symptoms, appreciable by the senses, immediately indicates a *specific* mode of treatment ; to the theoretical physician an internal lesion, which is supposed to exist, first indicates the internal change to be effected, and thus, indirectly, the treatment by which it is supposed the required change may be effected.

If it were a question of choice between the two methods, it must be confessed that the empirical in principle promises most certainty ; for in a simple comparison of phenomena—a mere enumeration of *pros* and *cons*—there is less liability to error than in a process of reasoning, in which conclusion is founded on conclusion, and in which a defective link may vitiate the whole chain. Such a fact as that bark cures ague, on the contrary, is in itself so certain that it is quite indifferent how the nature of ague and the mode of action of bark are explained. But such indisputably certain remedies are rare. After an experience of 2000 years, doubts are

still expressed as to the efficacy of medicine at all, and it often happens that in similar cases directly opposite modes of treatment are adopted.

The causes of the small progress in medicine here referred to, Henle considers to be partly subjective and therefore general, and partly objective, i. e. founded on the peculiarities of the object of medicine. That which subjectively or from within ourselves renders experience difficult is, *first*, a tendency natural to the mind to explain and to arrange facts which are seen to recur in inseparable connexion in the relation of cause and effect, and, for physical effects, to invent a world of metaphysical causes. Hence have arisen the mythological fancies of a *materia peccans*, a *vis medicatrix naturæ*, which lie at the bottom not only of the theories of Hippocrates, Galen, Helmont, Stahl, and the so-called natural historical school, but which also constitute the foundation of a physiology still most extensively prevalent. For to define the reaction which supervenes on irritation as a manifestation of an effort of the organism to resist external influences operating prejudicially to it, is merely to declare that every action is a *molimen criticum*, brought about by the autocracy of the organism for the purpose of removing the enemy. Instead of the condition on which the excitation depends, it is merely giving the object or final cause of it. *Secondly*, another tendency of our mind which obstructs the advance of knowledge, is to distort and even falsify facts in order to form systems.

To make medicine, so far as it is not yet, a science of experience and observation is, as is known, the object of the new French school, as the head of which Louis is distinguished. Rejecting uncertain tradition, and aware of the errors which result from coming to a conclusion regarding symptoms and the effects of treatment from the observation and superficial remembrance of a small number of cases, Louis requires that the value of the symptoms, as well as the methods of cure, be expressed in figures obtained by the comparison of as large a series as possible of accurately reported observations. This method, called the *numerical* or *statistical*, is in fact the only one from the employment of which empirical medicine, as well as other empirical sciences, has any advantage to hope for; inasmuch as logical certainty, in decisions founded on experience, is never obtained, but only a greater or less degree of probability, according to the number of observations and the proportion of affirmatives to negatives. Even the so-called *natural laws* possess only the highest degree of probability. In other cases the exceptions are too well established to be denied. The affirmed fact thus loses in probability; it expresses itself no more as a *law*, but only as a *rule* or *norm*. The value of such a rule, however, is variable, and numerically expressible by the proportion of the affirmative to the negative cases. Such expressions are sought for by statistics.

In practical medicine most propositions undoubtedly belong to the disputable. There are few *laws;* as to *rules* they are seldom well established, and when they are, they are of uncertain value. If it must be admitted that in similar cases of disease, comprised under one species, a certain symptom is sometimes present, sometimes absent, that certain phenomena precede often a fatal, but sometimes also a favorable issue, &c., then the necessity cannot be got over for correcting such superficial deductions by means of figures.

Calculations like those above indicated, however, do not warrant more than the expression that the facts stand in connexion according to *law* or *rule;* they teach nothing of their intimate connexion. That such an intimate connexion exists, and in what manner it does exist, it is the theorizing intellect only which shows and elucidates. Sciences of pure experience and observation, therefore, intentionally abstain from giving judgment on this point, and so also does pure empirical pathology. It knows that ulcers of the intestines and typhus fever, swelling of the spleen and intermittent fever, increased quantity of fibrin in the blood, and the local symptoms of inflammation occur together; but it avoids, or ought to avoid, speaking of the causal connexion of these anatomical changes with the symptoms of the disease. It leaves it, for example, to every one's fancy to consider the ulcers of the intestines as the cause or as the effect of the disease. If, however, knowledge, though so empirically acquired, is to attain to practical application, the question as to the causes can no longer be altogether excluded. However superficially the consideration of the phenomena of disease and recovery, considered in themselves, might be allowed to be passed over, the practical application is not at all conceivable, without the presupposition that in corresponding cases a method of cure and the course of a disease had stood in a certain causal relation.

For these reasons it is delusive to believe that in medicine we can rest on a foundation of pure experience and observation. Every inevitable conclusion is a source of logical error, a source of the deceptive "*post hoc ergo propter hoc*," against which, as is known, the advocates of opposed methods have never failed to warn each other. Medical experience tells merely that treatment and recovery succeed each other; it is inference and hypothesis to say that the recovery is the effect of the treatment. Though this is an old remark it is necessary to repeat it, because it is always forgotten, and the materia medica in consequence overloaded with remedies which are altogether without power of any kind.

The true touchstone for such hypotheses is experiment, and it is the number of experiments which determines the degree of probability with which a certain action is inferred to be the effect of a certain cause, and with which the same effect may for the future be expected from the same cause. But the problem becomes more complicated when many conditions, especially if opposed to each other, have part in one result. If it is desired to learn the value of one of these conditions, for example, a particular method of cure, it is not sufficient merely to count up the cases in which it has been tried with or without success, but it is necessary to give the number of the cases in which cures were effected by it *in proportion* to that of those in which it was not employed, or in which some other plan was had recourse to. This is the object of medical statistics, and for an earnest empiricism there is no other way of coming to a decision—the statistical or numerical method and the empirical are one. Another question, indeed, is what the numerical method has done, and is likely to do for practice. This question will be gone into more fully immediately, but at present it may be observed that when the *numerical method* is not in a condition to guide the choice of remedies, still less so is common so-called experience.

The statistical method is not applicable where there is not at command

a considerable number of cases admitting of being compared together. It is not adapted to rare diseases, nor to such as present great irregularities in their course; still less to the not inconsiderable number of chronic diseases, which do not properly belong to any of the recognised families, and are therefore excluded from our system. Its proper field are acute and especially endemic or epidemic diseases, in which the diagnosis is easy and certain, the influence of individual circumstances slight, and governed by the general causes of the disease. But, even in this circumscribed field, statistics are not to be applied without limitation. Acute diseases which, with our present knowledge, we cannot but group together under one head, present themselves according to place and time with very different characters, and it is doubtful, if not positively mischievous, to place observations collected in particular regions and at particular seasons alongside others collected under different circumstances. Acute diseases, which at present are specifically separated, were formerly collected together under one name, and in like manner perhaps our species will themselves be, at some future time, subdivided. Every division of this kind, however, renders the materials hitherto collected worthless. But suppose the identity of the cases of disease under comparison to stand fast, still, how many new difficulties oppose themselves to a uniform action of remedies!

But when, in the class of diseases to which it must limit itself, numerical medicine at last arrives at authentic and stable results by means of a favorable concurrence of all objective and subjective circumstances, objections may still be raised against the practical usefulness of these results. What, says Henle, have we to do when the comparison of two methods of cure gives for the one a slight or even a considerable preference? Are we to adopt it for *all* cases, and are we for the future to make up our minds to the sacrifice of a certain number of patients? Might not those, which we now give up, be rescued by a mode of treatment not adapted for the majority? Are we to comfort ourselves, like gamblers, with the prospect that in the end the gain will exceed the loss? But even if diseases were commensurable, individualities are not so. It is only on the field of battle that fifty living men are an equivalent for fifty dead; to the physician every individual person is incommensurable. The duties, which evidently spring from this circumstance, must be fulfilled by individualizing. But whoever individualizes theorizes. The characteristic of what is individual, in opposition to what is specific, is the concurrence of phenomena occasioned by a multiplicity of disturbing and promoting causes, which does not admit of being prognosticated, and the repetition of which can scarcely even be expected; there can therefore be for the individual neither law nor rule, but there is instead the field of analogy, of combination, of abstraction, i. e. the separation of the accidental from the essential, and the discernment of the import of everything accidental.

To conclude—as pure empiricism does not suffice for every case, and can serve as a guide for our treatment only in a very few,—as we must proceed in particular cases according to particular considerations, which can arise only from opinions on the *signification* of the symptoms and the *nature* of the remedies,—it is a duty to enter as deeply as possible into the nature and intimate connexion of the phenomena which we have to investigate. Whoever is once necessitated to theorize acts foolishly and,

if he at the same time undertakes responsibility, unconscientiously, when he leaves it to chance what theory shall occur to him at the moment of decision.

From what has thus been said it will be seen that a strict separation between empirical and rational medicine is not possible.

The *Departments of Medical Science* and the *Position of Rational Pathology* constitute the next subject of the introduction.

The departments of medical science may be divided into three groups: the *first*, comprising what we know of disease, and its different forms, viz. *Pathology;* the *second*, what we know of remedies, viz. *Materia Medica;* the *third*, laying down the rules for the employment of remedies, viz. *Therapeutics*.

Pathology, materia medica, and therapeutics are differently treated of, according as they are studied in reference to the empirical or to the theoretical method.

1st. *According to the empirical method.* In accordance with the way in which the *empirical* physician embraces pathology, he requires true descriptions of the symptoms and course of diseases by which to distinguish them from other diseases. To supply such descriptions is the task of *special pathology*, which may therefore also be named *empirical*. Special pathology views, tacitly or expressly, each case of disease as an individual, combines similar cases to make up the description of the species, collects species into genera, and these into families, &c.; and thus obtains, by an analytical method, a system which, like the systems of the descriptive sciences, discusses the sum of the details.

In the natural sciences, artificial systems have now been completely superseded by natural systems. Medicine seeks to take the same direction, without, however, rightly understanding the requirements for the one or the other kind of classification. Properly speaking, there cannot be an artificial classification of diseases, as there are no signs common to all, and which, with certain modifications of quality or quantity, could be pointed to in every single species. The only exception is as regards duration, according to which, indeed, diseases have been and still are divided into acute and chronic, but that this principle is not applicable for farther subdivision is evident. All other empirical or analytical systems are natural.

The doctrine of symptoms or *semiotics* may be viewed as a complement to special pathology—an index, as it were, to it. Special pathology classifies diseases according to these forms, and lays down the combination of symptoms proper to each species. Semiotics classifies, according to the symptoms, and enumerates the species, i. e. the combinations of the disease in which they appear.

The materia medica is to the empirical physician the table of remedies at his command, with directions for their employment in certain forms of disease or combinations of symptoms comprehended in the system. The action of the remedies on the healthy body is indifferent to him.

The therapeutics of the empirical physician is what is called *special therapeutics*. It points out the approved plan of treatment for every form of disease comprehended in the special pathology. Special therapeutics is to pharmacology what special pathology is to semiotics. Thera-

peutics indicates the remedies for the diseases, pharmacology the diseases for the remedies.

2d. *According to the theoretical method.* Rational pathology gives an account of the causes and nature of the morbid processes. This used to be done either according to prevailing mythical or "*natur-philosophical*" views, or the characters common to the diseases were grouped together, in order to rise analytically to an idea of the disease. Thus arose the *theory of disease*, or the so-called *general pathology*, in which, besides the specific phenomena of the diseases, special etiology was comprised, and, in consequence of a misunderstanding, semiotics also. General pathology is, however, only a part, and that merely the general part of theoretical pathology.

Rational pathology, such as Henle views it, occupies itself with the vital manifestations of the diseased body. It has, therefore, also been named the physiology of disease, or the physiology of the diseased body. Physiology and pathology are one. If, however, pathology continue separate fron physiology, this is owing merely to the difference in the way pursued by them, and their imperfection hitherto. Physiology sets out from the organs in order to learn the different modes in which their life is manifested under different conditions; pathology, on the other hand, sets out from the manifestations of life, in order, by reasoning backwards to come to a conclusion as to the active organ and the conditions of its abnormal activity. There are symptoms and groups of symptoms to which physiology, in its way, has not yet descended, and to the source of which pathology has not yet risen. Till this be done, there will remain a gap between physiology and pathology, the removal of which is more for the interest of the latter than of the former.

The materia medica of the rational physician is also a physiological doctrine, for it occupies itself in the endeavour to obtain an insight into the mode of action of the remedies on the organic matter. This, however, is very difficult, on account of the therapeutical experiments, from which the conclusions require to be drawn, being much too complex.

If it were but once possible, not only to elicit indications from the nature of diseases and remedies, but also to determine, *à priori*, the mode of procedure adapted to them, we should obtain a rational therapeutics, which comprehends within itself the hitherto so-called general therapeutics in exactly the same way that rational does general pathology. But here also, before planting the field farther is thought of, the weeds already sown by the inconsiderate itch for explanations on the part of those calling themselves empirical must be rooted out.

Rational therapeutics has its special department; and this must be the foundation of the general. Special therapeutics depends on facts partly of rational pathology, partly of empirical therapeutics. Where the former has once penetrated, in the manner already mentioned, to the simple or compound cause of a disease, the prescription for the avoidance and cure of it is evident, viz. to counteract the cause, or neutralize it, or bring back the altered organic material to the normal form and composition. In many cases, however, our knowledge is not sufficient to enable us to lay down rational indications; in other cases, in which such could be laid down, remedies fitted to fulfil them are unknown. Under these circumstances, we take as a fact the empirically established efficacy of certain remedies in certain diseases, and try it with our explanations. This Henle

admits to be without doubt the most difficult and as yet the weakest side of rational pathology. A cure is the product of two factors—the disease and the medicine; if one factor is in some degree known, the other may be approximately found, thus from the nature of the organic changes the mode of action of the remedy is inferred, and *vice versâ, ex juvantibus et nocentibus*, the nature of the disease. Unfortunately, however, both factors have in general to be discovered, and in this case different solutions of the question are offered according to the prevailing opinions at the time.

Considering all these difficulties, Henle says that the elaboration of a system of rational therapeutics must perhaps be left to the future.

The third section of the Introduction is on the *Methods of Rational Pathology*.

Rational pathology and physiology are identical; the method of both is therefore the same; it is the method of all sciences of experience and observation, and especially of the natural sciences. By the coincidence of certain morbid phenomena with particular material changes, we are led to the admission of a causal relation between the two; experimenting, we conclude, as far as possible, as to the cause, and assure ourselves, when we observe the results, of the correctness of our conclusion. At the same time, it has in view, first, the so-called localization of the symptoms, i. e. the determination of the organ whence they proceed; but afterwards the quality of the morbid changes by the comparison of the altered form and composition with the normal. The more deeply we penetrate into the finer structure and composition of the tissues, the more manifold, considerable, and important are the differences disclosed. For the most important facts, therefore, pathology is indebted to the microscope and to organic chemistry. The last object is to refer the material processes which constitute the condition of the phenomena of the disease, as far as possible, to physical and chemical processes, and thereby to bring them under a common point of view with the phenomena of inorganic nature.

In the fourth section *the divisions and subdivisions of Rational Pathology* are laid down.

Rational pathology is divided into a general and a special part.

The general part treats of—

1. The idea and nature of disease.
2. The causes of disease in general—general etiology.
3. The general history of disease as regards its locality in the organism, and the mode of its transition from one organ to another.
4. The general history of disease as regards course, duration, and events.

The special part. The multiplicity of pathological manifestations is occasioned only by the multiplicity of external causes, and of the tissues entering into the composition of the organs, and the specific forms of disease depend always on the combination of the concrete cause and of the concrete organ. If there was only one cause, there would be as many specific forms of disease as organs; if only one organ, as many forms of disease as causes.

If rational pathology were perfected as a science, the special part would embrace two divisions, viz. special etiology, or the doctrine of the action of the causes of disease on the organs and tissues; and special sympto-

matology, or the doctrine of the mode of reaction of the tissues and organs. But so long as the specific causes, and the seat of a great many, perhaps the greater number, of diseases are hidden, or at least uncertain, a chapter on the pathological processes, i. e. a chapter in which diseases are arranged according to their symptoms, irrespective of their external and internal causes, is not to be dispensed with.

The fifth and last section of the introduction is occupied with an *Historical Review of Systems of Medicine.*

Into this we shall not enter, but proceed with the remainder of the volume, which comprises the general part of rational pathology, the subjects treated of in which have been just enumerated.

IDEA AND NATURE OF DISEASE.

Definition. To the ideas *normal* and *abnormal* the ideas *healthy* and *diseased* correspond so far, that the *healthy* is always at the same time the *normal*, the *diseased* the *abnormal*. It is not, however, usual to call everything that is normal healthy, and everything that is abnormal diseased. A crystal which deviates from the normal is not said to be diseased; a congenital defect (or even a defect originating in the course of life), which undergoes no further change, is in general not considered a diseased state. In short, disease is an *abnormal process*, in which there is manifestation of change, progress, or movement. What is abnormal and stationary is a defect,—vitium. The predicate of health, as the opposite of disease, is thus applicable only when there is a capability for an abnormal movement or development. A crystal cannot be said to be healthy but only normal. Health being a movement and a process, it implies the possibility of passing into disease. What cannot become diseased cannot be said to be healthy.

Such is the *idea* of health and disease, so far as it admits of being briefly stated, given by Henle. The *definition* of disease which he founds on it is, "the deviation from the normal typical, i. e. healthy, vital process." This definition, he affirms, is not mere circumlocution. It does not contain, as might at first sight appear, a mere negation, for *the deviating* is;— it is not a *nonentity*, but a *state of being different*. Disease is neither negative, mere absence of health; nor positive, something new, a superstructure on health; but is rather something *instead* of health—a modification of it.

Nature of disease. Fundamentally, disease is—not *abnormal reaction under similar conditions*, but *normal reaction* under different conditions.

This principle, Henle affirms, is either right, or scientific medicine is a chimera. In appearance, indeed, disease is abnormal reaction; pain, as it occurs in inflammation, is an unusual state; but in its nature it is normal reaction, for it is natural for healthy nerves to be the seat of pain on pressure, however caused. It can be surprising only to medical logic, when I affirm, says Henle, that every so-called manifestation of disease exactly corresponds to its external cause. But some one will say, "still irritability is evidently increased in inflammation; the same exposure to cold may cause toothache in one, diarrhœa in another." In answer to this, Henle by the following example illustrates and justifies the meaning of his assertion. In the eye, the sensation of light may be excited, not only

by the agency of elementary light, but by other agencies, such as pressure. Suppose, then, common daylight be $=a$, and the direct rays of the sun $= 2a$, and that most eyes bear the irritation of a well, but are blinded by $2a$. If an eye be blinded even by a, it is said to be abnormal, but if this eye have previously received an impression from some other agency, the excitement occasioned by which corresponds to the mass of light a, the sum of irritation which the eye has thus received will be equal to $2a$, and therefore it is not surprising that it should be dazzled as much as the normal eye is by the sun's rays.

In like manner is explained how, by apparently the same cause, different morbid processes are occasioned. In one case, cold operates in secret combination with an injurious influence, which has already altered the vitality of the nerve of the tooth; in another case, in combination with one which has altered the condition of the mucous membrane of the intestinal canal. The product thus appears to be abnormal only when we compare it with *part* of the causes in operation, especially the last; but it shows itself to be normal and necessary, as soon as we take into consideration *all* the causes. The state which in relation to its bearing towards new irritations we call *morbid*, is in relation to previous irritations, the *normal*.

PROCESS OF DISEASE. As all bodies of the same species react in the same way under similar circumstances, the peculiarities of reaction which bodies of different species exhibit under similar circumstances, must at last be referred to peculiarities of type. We have thus to search, in the peculiarities of type which distinguish organic from inorganic bodies, for the cause why it is in the former only that anomalies (or manifestations of the typical force under unusual circumstances) present themselves as *processes*. From the peculiarities of type of organic beings, it happens why it is in them only that anomalies present themselves as diseases or morbid processes. Elimination and renewal of substance is a fundamental character of living beings. It is not wonderful that this fundamental feature preserves itself under unusual circumstances. Only so long as it does preserve itself is the individual diseased; any change which completely stops it, occasions, not disease, but death. Disease is thus a process because life is a process; when it appertains to the type of a body to change its form or substance, abnormal influences change it not only for the instant, but they change also its mode of becoming altered; they turn it away for a longer or shorter time, or for ever, according to the duration of their action, from its aim. The process of development deviating from the aim is the pathological process.

GENERAL ETIOLOGY.

Proceeding from the proposition that the typical force operates according to unchangeable laws, we must recognise as the cause of every anomaly the action of unusual *external* conditions. The cause of all disease lies originally in an insufficient condition of the compensating material, or in the unappropriateness of the changing influences. Morbid formations of whatever kind are thus originally products of normal typical forces, and inadequate compensating material, or inadequate external conditions of formation. They are merely what the typical force was able to create under the given most unfavorable conditions.

In so far as the body, dependent on external supply, is changeable by

external agents, it has the tendency to become diseased. It has health and disease, both *only* in the disposition, because it is a *becoming body*, and because something external must come to it, or be added to it, by means of which it may *become*. The disposition to become diseased is determined by the same typical force as the disposition to healthy development, and is the same for all indivduals of the same species, and existing under similar circumstances of age and sex.

General action of irritants. To the living body is ascribed the property of being excited by every impression to a manifestation of the activity peculiar to it. On account of this property, it is called *irritable* or *excitable*. The influences capable of rousing the slumbering activity are called *irritants;* the operation itself, by which the irritation is responded to, is called *reaction*. Whatever be the irritant, no other reaction takes place but that which is peculiar to the organ.

We view the function of an organ as the consequence of its composition and form, the composition and form again as the result of interchange of substance, the duration and rapidity of which is regulated by the type. Whatever accidentally changes the composition and form, changes the function. The change may have the consequence that the activity becomes quantitatively less or more than it usually is, according to the *tone*, or that slight degree of activity in which an organ, under ordinary circumstances, constantly is without any particular excitement from without. When the activity becomes quantitatively more, the changing influence or irritant *appears to rouse* the activity, supposing it previously overlooked, but, in fact, *changes* it only. What we call irritant, is thus an influence which, at the same time that it alters the substance, changes its powers and activity. But the same thing is effected by every chemico-physical influence on inorganic bodies. The mode in which living bodies comport themselves to irritants is, however, really different from that in which inorganic bodies do to chemico-physical influences; and this difference depends on the fundamental characteristic of living bodies, viz. their property of constantly renewing their substance.

The sort of reaction which is peculiar to the living organism, is that by which the disturbing force is equalized. It is that by which it resists external influences. It is not the excitement which follows the irritation, but the transition to rest *after* the irritation. As two different processes ought not to be distinguished by the same name, Henle retains the name *reaction* for the vital manifestations, which are the immediate consequences of the irritation, and suggests the name *restitution* for the after-action, by which the organ returns to the normal state. From the word reaction the idea of independent reaction, or counter-effect, is thus to be separated; in physiology, *reaction* should mean nothing more than in chemistry, viz. the phenomenon which is called forth in a body by foreign action.

Exhaustion. An organ, the action of which is constant, may be quantitatively altered by irritants in two directions; its function may, in comparison with what is normal, appear accelerated and strengthened, or retarded and weakened. The irritants which increase the activity of the function, are named exciting irritants,* irritants in the narrowest sense. The name *depressing* irritants,† although it contains a contradiction, has

* *Hypersthenisants* of the Italian therapeutists.

† *Hyposthenisants* of the Italian therapeutists.

been usually applied to the influences which depress the activity. Both kinds of irritants may operate in such a degree as wholly to put a stop to the nutritive interchange, and for ever deprive the organic structure of its vital endowments, but in different ways: in the case of depressing irritants the complete paralysis occurs *directly;* in the case of exciting irritants, *indirectly*. Both kinds resemble each other in this also, that their effects are, by restitution, gradually equalized; they differ from each other, however, in their remote consequences. After *depressing* irritants, restitution restores the organ to, or nearly to, its normal activity; after *exciting*, it brings it down not only *to*, but *below* its normal activity, to a state of fatigue, from which it only subsequently recovers. The necessity of this latter fact does not appear from what has been above said of the nature of irritation; for if irritation be merely the expression of an alteration of the organic matter, which again becomes equalized in rest, it does not appear why a movement in the opposite direction must first take place. It, however, becomes conceivable when it is remembered that restitution of individual parts, as well as of the whole organism, is dependent on conditions which are furnished to it from without, and the supply of which has no necessary connexion with the cause of the excitement. The individual organs stand in the same relation to the blood as the organism, considered as a whole, does to the vital irritants. With every disposition to react, reaction will not take place if the renewal of the blood be interrupted, or the nutrient constituents of it consumed. On the application of exciting irritants, both results appear to take place; the one by stagnation of the blood, the other by increased affinity of the organ for the vital irritants, or increased nutritive interchange. In consequence of this increased nutritive interchange there is increased energy of function, but, by and by, the source of nourishment becomes exhausted, its supply being limited both as to time and space, the vital conditions thus fail, and diminished energy of function is the result. The contrary is the case with depressing irritants. When an irritant diminishes the affinity of the organic substance for the vital irritants, the nutritive interchange becomes less, hence diminution of the force, hence fatigue and paralysis. The final result of the action of exciting and depressing irritants is the same, viz. diminished vital process, diminished energy, but in the former, from want of new vital irritants, in the latter, from incapacity to receive what exists.

It has been above shown how the excited organ, so long as exhaustion does not ensue, appears to be in a state of increased excitability, because the new irritant concurs with the consequences of a previous irritant. Where qualitative differences are possible, the form of the excitability is at the same time determined by the existing excitement; the organ becomes specifically more irritable for one *kind* of sensations, for the contrasting especially. Here, also, the concurrence of a new change with one already effected in the nerve always occasions the appearance of a reciprocating sum of excitability. The retina, which after green perceives red more vividly, would, after some time, have of itself seen red; it sees, so to speak, doubly red when to the subjective the objective is superadded. On the same principle, a moderate irritant proves a violent one, when it encounters the organ prepared by the contrasting action.

Exercise and habit. By exercise an organ becomes more susceptible of irritation, so that smaller quantities are required for exciting it to the same

degree of activity. By habit, on the contrary, an organ becomes less susceptible of irritation, so that larger quantities of irritation are required for exciting it to the same degree of activity. This shows that the affirmation, that by restitution the effect of altering irritants is again equalized, is not to be taken literally. It does, indeed, appear that, after irritation, equalization takes place if we view the consequence of a single irritation; but when irritations of the like kind often recur in the same organ, appreciable changes at last appear, which cannot have otherwise arisen than by the addition of similar but *inappreciable* changes after every irritation. The effects of these changes may be either increased or diminished susceptibility.

Erethism. In every degree of irritability and tone, the relation, already several times mentioned, of the active organ to the compensating means gives occasion to deviations, which manifest a disproportion between the energy and the duration of the excitement. The rapidity with which the vital excitants are supplied and renewed, is dependent on other conditions than the force with which the individual parts of the body are capable of attracting it. In general both increase simultaneously, but there is a state possible in which the capacity for renewal lags behind excitability, to this state the name of *erethism*, or *irritable weakness*, is applied. It is especially remarkable when the excitability is normal, or even more vivid than usual.

Erethism is local or general. An example of local erethism is presented by parts, the vascular system of which has a particular disposition to congestion. Examples of general erethism are met with after great losses of blood and other fluids, and in convalescence from febrile diseases.

After-action. It has been admitted that every irritation changes the relation of the irritated part to the blood; in accordance with this, the condition of the blood must, according to the deviations of the states of excitement of individual organs, be modified, and hence, as the blood is the common source of the nourishment of all organs, every local change must at last induce a change of the whole organism. This change may be, to a certain degree, either beneficial or prejudicial; beneficial as when, by appropriate exercise, the general condition is improved along with an increase in the power of the exercised organ and in the appetite; prejudicial, as when the loss of the vital irritants is not at all, or not rapidly enough supplied, and general exhaustion produced, the primarily-excited organ itself being at the same time weakened, or, on the contrary, strengthened.

Disposition to disease. In its relations with the external world, in its dependence on the vital irritants, in its changeability by accidental irritants, the organism possesses the *capability* of becoming diseased. In order that disease may *actually occur*, the operation of external influences on the organized individual is necessary. The particular disease which shall occur depends on the particular organic relations and mode of reaction of the individual, and on the concrete nature of the external influences.

An external and an internal condition must thus concur in the production of a particular form of disease—the internal condition constitutes what is called *predisposition to disease* (*seminia morborum* of Gaubius).

The admission of particular predispositions rests on the fact that the

same injurious influence produces, in different individuals, different diseases; that it sometimes acts violently, sometimes mildly, or not at all.

A different effect of a similar cause, however, does not and cannot occur in individuals of the same species, as above shown. When there is an appearance of such a thing, it is found, on examination, that either the individuals compared existed under different conditions of normal formation, or under the influence of different external agencies. It is necessary to distinguish these two cases; for the individual which reacts peculiarly only by virtue of the sex or age to which it belongs, is healthy; the individual, on the contrary, which, on account of co-operating external influences, manifests a particular disposition, is already changed by such influences, and is therefore diseased. The disposition which depends on the normal relations, Henle calls *normal predisposition*, and that which depends on the previous operation of external influences, *abnormal predisposition*. The former only is really innate—the inheritance of the race as it came from the hands of the Creator; the latter is always acquired, though to the individual in which it appears it may have been transmitted by generation.

Normal predisposition to disease. To the normal conditions on which peculiarities of predisposition depend, belong age, sex, perhaps race, temperament, and similar individual characters within the norm. From the comparison of the peculiarities of reaction occurring in one species, genus or higher class with those of other species, genera, &c., the normal disposition of the group in question is ascertained.

The comparison of different predispositions may lead to two results:

1. That the disposition to become diseased in general may be greater or less according to age, sex, race, &c.

2. That certain forms of disease exclusively or especially belong to a particular group.

In the first case we distinguish the *quantity*, in the second the *quality* of the predisposition.

In regard to the quality of the predisposition: the forms of disease are, under similar external circumstances, directly determined by the nature of the organs and tissues acted on by the cause, and the organs and tissues being the same, by their peculiar typical modes of reaction. The tendency to become diseased in a particular manner, or the *qualitative* predisposition to disease, is thus dependent on the constitution of the organs and systems, and on the modes of reaction.

As to the constitution of the organs and systems. It is self-evident that every group is distinguished by anomalies of the organs which are exclusively peculiar to it; that the non-development, arrestment of the development, and certain malformations present themselves as diseases only at the age at which development is not yet completed, and still to be expected, or are first perceived at the age when the organ should enter upon its function. From this it is seen how that systems anatomically or functionally predominant must enter into manifold relations with the external world and the other organs; and be exposed in an especial manner to noxious influences. In this way is explained, the correctness of the fact being assumed, the very great tendency to diseases of digestion in childhood, of the lungs in youth, and of the mind in manhood.

As to the modes of reaction. By nothing but by a reference to the eternal typical laws is it to be explained why in the salamander whole members are reproduced, whilst in the allied frog, regeneration is, as in the higher animals, limited to a few tissues ; why the same kind of wound in one species of the mammifera heals with the formation of a crust, in another by suppuration ; why, lastly, the same substance is in one species harmless, in another poisonous.

Abnormal predisposition to disease. Abnormal predisposition is itself, in fact, disease. To it is applicable the expression "*affectio,*" with which Gaubius designated predisposition in general. The pathological state called predisposition may be undeveloped, and as yet without manifest symptoms, *affectio occulta* of the older authors ; or is already evident disease, but preparatory (*affectio predisponens*) to another state of disease, which is either a higher degree of that which already exists, or a disease different in quality from it. Morbid predisposition has, moreover, this in common with disease, that it may be persistent or temporary, inherited or acquired after birth.

Pars minoris resistentiæ is an organ which is habitually in a state of over- or under-excitement, in comparison with the other organs, and assailable from all points of the body by every kind of influence. In a person with a carious tooth, for example, toothache may be brought on by cold, by heat, by mental affection, errors of diet.

Hereditability. From fissiparous generation literally, from gemmiparous generation very evidently, and oviparous generation not less really, though less evidently, it appears why the action of morbidly altering influences may extend to the posterity, nay, that they *must* extend. But they do not explain why specific diseases, such as dyscrasiæ of the parent are repeated in the offspring. Disease thus transmitted must be distinguished from heritable or inherited disease, which is not merely a continuation but also a repetition of the disease of the parents.

Hereditability of disease is no farther explicable than by reference to the empirical law that the young resemble their parents in their peculiarities.

Hereditary disease may already exist at birth (*congenital*), or make its appearance only after birth. When not congenital, it is to be viewed as *affectio occulta* until the period favorable to its outbreak. Congenital hereditary diseases are not to be confounded with diseases which, though congenital, have had their origin in some accidental cause during intrauterine life.

Constitution. Habit. The name morbid predisposition has been above employed to designate every permanent or temporary, slight or inveterate abnormal state, in so far only as it is viewed as preparatory to some other morbid state. The name *constitution* is employed to designate a peculiar kind of predisposition which is in any case permanent, mostly congenital or inherited, and in no case important enough to be viewed as a disease in itself. The state which is comprehended under the name of *predisposition,* as something relative in reference to a certain morbid process, appears, under the name of *constitution,* as something absolute, ready, and of itself existing. Predisposition and constitution are apt to be confounded ; for example, scrofulous predisposition instead of scrofulous constitution. Scrofulous constitution is that kind of constitution in which

the predisposition to scrofula exists. The constitution is, as mentioned, generally congenital, and at any rate, developed by subsequent influences; still it may be also acquired, or, which is the same thing, qualitatively changed by the mode of life and supervention of disease.

The external sign of the constitution, or of a permanent anomaly which cannot yet be called disease, is *habit*. The elements which compose the habit are the form and colour of the exterior of the body, and the condition of it, from which a conclusion may be mediately drawn as to the nutrition, the quantity of blood, the richness of the blood in particular constituents, the energy of the nervous and muscular systems, &c.

EXTERNAL CAUSES. So long as the combination of symptoms, on which the diagnosis is founded, was considered as an entity and called disease, the internal derangement, which is the condition of the symptoms, was spoken of as the proximate cause of the disease. This *causa proxima* was distinguished from the remote causes, *causæ remotæ*, the causes of the derangement itself. Nowadays, when symptoms are no longer viewed as manifestations of an ideal entity, but as the effects of the material derangement, this derangement is recognised as the disease itself.

A great, perhaps the greatest, part of diseases which come under observation is the result of a plurality of causes. When several causes occur at the same time, a threefold consequence is possible: the effects of each cause may—1st, proceed in company, little or not at all disturbing each other; or, 2d, increase each other; or, 3d, limit each other; or, 4th, neither increase nor limit; but there may result from their combination an effect qualitatively different from any of them.

LOCALITY OF DISEASE, AND MODE OF TRANSITION FROM ONE ORGAN TO ANOTHER.

SYMPTOMS OF DISEASE. The healthy state of the body and organs is recognisable by the external appearance, the form and composition of the parts, and by their functions. The morbid state is manifested by deviations of these from the normal condition. Changes of the aggregate state and function, by which the disease is made known to the patient himself, or objectively to others, are called the *symptoms* or *phenomena* of the disease.

Strictly speaking, symptom is thus the external morbid phenomenon, whether changed function or change of aggregate state. Disease, on the contrary, is the material derangement which is inferred to be the cause of the morbid phenomenon. According to this, symptom is the *occasioned*, disease the *occasioning*; and to symptoms as well as to disease there belong two attributes, viz. *symptom* is the external and the effect, *disease* the internal and the cause. Symptom considered as the external sign of the disease, though always in causal relation to it, is not always its direct effect, but may be merely the effect of a direct effect of the disease, thus headache is not the direct effect of derangement of the stomach, but is the effect of some change in the frontal nerve, which is the direct effect of the derangement of the stomach.

On the other hand, symptom considered as the effect, is not necessarily external sign, but may be the cause of external signs; thus effusion of serum into the cellular tissue, which is a symptom of certain diseases of the

blood, of pressure on veins, is not itself an external sign, but is the cause the external signs of dropsy.

Symptomatic and idiopathic disease. Jaundice may arise from disease of the liver occasioned by disease of the stomach, brain, or other organ, or it may arise from primary disease of the liver. In the former case, it is said to be symptomatic, in the latter case idiopathic. In both cases, however, the jaundice is merely a symptom of the liver complaint, which, in the former case, is the symptomatic disease, in the latter, the idiopathic.

The symptoms of the cause of the disease are to be distinguished from the symptoms of the disease itself; for example, the smell of spirits in the breath of a drunk person is the symptom of the cause of the drunk state.

Local and general diseases. The determination of the general or local nature of a disease was not possible until it was determined what is meant by disease. Fever was, at one time, said to be general, because it was considered to be a disease of the blood; but when the change in the blood was found to arise from a local cause, fever came to be viewed as a local disease. When, however, the local cause was discovered to be the effect of a deterioration of the blood, fever was again viewed as a general disease. In all this the true proximate cause of fever was not yet taken into consideration. Had it been remembered that the febrile symptoms are owing to an affection of all or many nerves, the disease would have been viewed as general; and had it then been found that the cause of the affection of the nerves lay in a part of the central organ, fever would have again been put down as local.

We now know that disease is a process comprising different changes, partly local, partly general—changes, the causal relations of which with each other may be very manifold. Before a disease is put down as local or general, it must be determined which link in the chain of its conditions is the most essential, and, above all, in what mode of connexion the links stand to each other in individual cases.

As every drop of blood is mixed with the whole, if a drop be altered in any part of the vascular system, the alteration is communicated to the whole. Disease of the blood is, therefore, necessarily and absolutely general, and may be contradistinguished from diseases of the solids, which are properly local, and only more or less disseminated. This distinction, however, is of little value, as few diseases remain confined either to the fluids or to the solids.

General disease, in the empirical sense, requires to be distingished from general disease in the theoretical sense. General disease, in the empirical sense, is the proximate organic condition of the general symptoms. General disease, in the theoretical sense, is a disease of the blood alone.

General disease, or disease from general causes, is frequently understood in the same sense as *constitutional* disease; a *constitutional* or *general* predisposition is presupposed in the case of a symptom which appears to have arisen without sufficient occasional cause. This confusion may, in most cases admit of justification; most constitutional diseases are not only in the empirical, but probably also in the theoretical sense, general, seeing that they may break out, not only in many parts of the body, and in the whole extent of a tissue, but also appear to owe their first origin to such causes as operate on and through the juices. It is, however, a

too hasty proceeding, and one not without disadvantage to the investigation of the individual pathological processes, to assume constitutional disease and disease of the juices as exactly synonymous. Every disease of the juices is not constitutional, nor is every constitutional disease a disease of the juices. Madness and rheumatism may be constitutional, but are they owing to a morbid condition of the blood?

That disease is constitutional which arises from a long series of inconsiderable influences without marked, or, at least, without adequate occasional cause. General diseases are diseases of the blood without reference to origin. Syphilitic dyscrasy Henle would call general, not constitutional syphilis. Of hereditary mental diseases, he would say that they arise from internal or constitutional, not from general causes. Other diseases, as gout, scrofula, &c., are, at the same time, both constitutional and general.

SYMPATHY AND ANTAGONISM. The various parts of the organism co-operate in the maintenance of the whole; each part thus stands in reciprocal relation with all the others, and continues healthy only so long as all the others perform their functions correctly. If this relation of all the parts to each other were general and uniform, the result of the change of one must be equally communicated to, and spread over all the others. There is, however, both in physiological and in pathological processes, a particular co-operation between particular organs, so that the changed condition of one is expressed by a changed condition of another, and excitement of one occasions excitement or, its contrary, diminished activity of another. Strong irritation of the retina, for example, occasions contraction of the pupil, closure of the eyelids, and sneezing; disease of the liver, pain in the right shoulder.

Two organs which are thus excitable and changeable by each other, stand, as is said, in sympathy or consensus.

There are two modes of sympathy, viz. synergy and antagonism. The name synergy, Henle proposes for that mode of sympathy in which the same change ensues in one organ which was excited in the other, and the name antagonism for that mode in which the change in the first organ occasions an opposite change in the second. In general, the sympathetical connexion between two organs may be either exclusively synergical, or exclusively antagonistic, or alternately synergical and antagonistic, and perhaps only more frequently in the one way or in the other.

What the conditions are, on which synergy in one case and antagonism in another depend, have not been made out except in a very few cases.

The phenomena of sympathy, as well synergy as antagonism, depend on physiological grounds—on the original normal connexion of individual organs and tissues. Those communications only are sympathetic which are conveyed by a permanent and pre-existing relation of the parts to each other. There are, however, normal and abnormal sympathies. Normal sympathies occur in every healthy body, as they are partly conditions without which the normal function would not be exercised. Sympathies are abnormal, either in consequence of the readiness with which, or the extent to which they take place, or in consequence of their occurring between parts which do not usually sympathise. The organs between which abnormal sympathies occur are pathologically changed.

According to the place where the cause of the disease has operated, the

sympathies define the extent and form of the disease. The special knowledge of them is indispensable to the practitioner, partly for the sake of diagnosis—for the symptoms of many diseases of internal organs are the consequences only of sympathetical irritation in structures more accessible to exploration; partly for the sake of therapeutics, for many organs can be affected only through others connected with them sympathetically. On the laws of antagonism is founded the treatment by derivation.

The formula which may be established as the pretty generally applicable expression for the process of sympathy is: A *acts through the medium* X *on* B. The communicating medium is the *source* or *conveyer* of the sympathy.

It has been above observed that the idea sympathy or *consensus* comprises the idea of a permanent connexion originally existing in the organization. As the carrier of sympathy, therefore, the permanent structure comprehended in the original plan of organization is to be recognised.

In short, sympathy may be defined the connate or permanent connexion of the parts of an organism kept up through the medium of the normal tissues or organs in such a way, generally reciprocal, that the changed state of the one has for its consequence a changed state of the other.

This definition Henle considers to be in general sufficient to distinguish sympathetic reactions from those which are direct—the simply consecutive—and from those which arise from the extension or transplantation of the cause, or from accidental morbid products.

As communicators of sympathies two parts are known, viz. the blood and the nervous system. Accordingly, two classes of sympathies are recognised: sympathies through the blood and sympathies through the nervous system. To these must be added a third class, in regard to the source of which, however, it is not certain whether it be through the blood or the nerves.

A. *Normal Sympathies.*

I. *Sympathies through the blood.* Sympathy through the blood is presented by those parts which have similar relations to one of the constituents of that fluid,—in nutrition, in secretion, and in excretion. If the particular constituent be increased in quantity in the blood, the action of all the parts in question is increased. The constituent remaining the same, inactivity of one of the parts will call forth increased activity of the others, and *vice versâ*. Sympathy through the blood thus always presents itself in the form of antagonism. Examples of antagonism through the blood are presented by the skin and kidneys as regards the excretion of water, and between the lungs and skin perhaps as regards carbonic acid.

II. *Sympathies through the nerves.* The communication of excitement from one nervous fibre to another in the central organs of the nervous system, viz. the brain, spinal marrow, and ganglions, is the basis of all unequivocal sympathies through the nerves. The laws according to which the communication is effected are at the same time the laws of sympathies.

It is impossible to conceive the communication to be effected in any other way than that of proximity in space. Nerves which diverge peripherally may be in close proximity in the central organ, hence distinct parts of the body may be in such sympathetical connexion, as if the irritation immediately spread from one to the other.

If our knowledge of the arrangement of the origins of the nerves in the central organs were perfect, nothing farther would be necessary than to show that nerves which are here in contact stand in special sympathy with each other. We are, however, still far from possessing such a knowledge of the structure of the nervous system.

The sympathies between the peripheral parts of the body are better known, and if it were only proved that sympathetically excitable nerves lie together in their central course, more certain conclusions might be drawn from the sympathies as to the structure of the central organ. The proposition therefore, that nervous sympathy is the consequence of an extension of the excitement in the central organ according to the contiguity of the nervous fibres, is not to be taken for more than an hypothesis; an hypothesis which will be found the more admissible, the more frequent, on the one hand, sympathies between reputed neighbouring nervous fibres are, and on the other, the more it admits of being rendered probable that nerves sympathetically connected approximate each other by their central ends.

The conjectures as to the arrangement of the nervous fibres in the central organs are founded partly on anatomical, partly on physiological researches. As a basis, therefore, for his discussions on the nervous sympathies, Henle devotes a considerable space to anatomical and physiological observations on the nervous system, which, however, we must pass over, and then observes that, in order to separate as much as possible what is certain from what is hypothetical, he will treat of the sympathies of each group of nerves separately; enumerating in each successive group, the sympathies of the nerves belonging to it with each other and with the nerves of the preceding group. Sympathies are divisible into five classes: 1. Sympathies of recognised cerebro-spinal nerves, i. e. of the sensitive nerves of the external parts of the body, and of the voluntary motor nerves, with the exception of the sensorial organ. 2. Sympathies of the visceral nerves, sensitive and motor. 3. Of the nerves of the cellular tissue. 4. Of the nerves of the vessels; and, 5, of the psychical nerves.

1. Sympathies of the cerebro-spinal nerves. There are three directions in which a communication may take place through the central gray substance of the spinal marrow, between the fibres of the white cords, viz. 1. Broadways, or from one side to the other. 2. Lengthways, from below upwards, or from above downwards. 3. Thickways, or from behind forwards, or from before backwards. Thus, if a sensitive nerve be excited from without, the excitement is directly communicated either to the corresponding symmetrical sensitive nerve of the other side, or to a sensitive nerve of the same side immediately above or immediately below, or, lastly, to the motor nerve of the same side situated on the same level. To one or other of these three kinds of communication all sympathies may be referred.

Synergical communication:—

From one side to the other. *a. Sensitive Nerves.* The following are examples: The supervention of toothache in the sound tooth of the opposite side. The case observed by Ollivier, in which, though the skin of the left side of the body was itself insensible to pinching, &c., a slight sensation was thereby excited in the skin of the opposite side. The fact that the width of the pupil is regulated in each eye by the sum of the

light falling on both, as shown by this, that if one eye be closed, the pupil of the other will become somewhat dilated, though still exposed to the same degree of light it was individually exposed to before; and that when one eye is amaurotic, the pupil is dilated while the sound eye is covered, but generally contracts when the sound eye is opened to the light. This, Henle thinks, may be owing to sympathy between the two retinæ, or to an association between the motor nerves of the two irides. *b. Motor Nerves.* As to synergical communication from one side to the other between symmetrical motor nerves,—voluntary nerves at least,—Henle thinks it difficult to say, whether the following, out of several others, be a real example: a case of hemiplegia recorded by Dr. Marshall Hall, in which movement of the sound arm was accompanied by unconscious movements of the paralysed one.

As to the more compound diseases which affect symmetrical organs or regions, Henle observes, that in many cases symmetrical disease is referable to the similarity of the conditions of the two organs as regards structure, exposure to cause, and especially as regards the condition of the blood. But why particular symmetrical regions of the skin, particular joints, &c., become diseased in preference to other regions of the skin, other joints, &c., of the same side, Henle thinks must rather be looked for in the nervous system.

Ascending and descending communications. *a. From sensitive to sensitive nerves of the same side.* The facts that almost every violent pain apparently extends in the neighbourhood of the affected part, that strong excitement of the retina occasions tickling in the nose, that a disagreeable sound sets the teeth on edge, and produces creeping of the skin, &c. may be viewed as examples of this kind of communication.

b. From motor to motor nerves of the same side. As examples of this kind may be mentioned: difficulty of moving certain fingers without others. Such cases as these, a patient with paralysis of the left side, when he yawned raised the paralytic arm; another patient when he sneezed raised the paralytic arm, and when he coughed, the paralytic leg, &c. Many spasmodic diseases, as squinting, stuttering, &c., depend on a morbid tendency to such sympathetic movements.

Communications from before backwards, or from behind forwards. *a. From before backwards—from motor to sensitive nerves.*—Examples: *Reflex sensations.* The common case of cramp and neuralgy. *b. From behind forwards—from sensitive to motor nerves.* Examples: *Reflex motions.* The best known and most extensive sympathies.

Antagonistic communication:—

The most striking examples of antagonism are presented by the anterior and posterior columns of the spinal marrow in their reciprocal relations. Irritation of a part of the skin occasions, by synergy, reflected cramps in corresponding muscles. Cramps originally owing to internal causes, on the contrary, admit of cure by irritation of the corresponding sensitive nerves; it is well known that painful cramp of the calves of the legs may be relieved by rubbing the skin. To reflex sensation, in which cramp occasions pain, stand opposed, as corresponding antagonistic phenomena, those cases in which muscular action depresses the sensibility.

2. Sympathies of the visceral nerves. *a. Synergy.* In regard to the visceral nerves, the preliminary anatomico-physiological investigations led

to the conclusion that they end partly in the ganglions, and partly in the spinal marrow, after having passed through the ganglions, and that, before joining the spinal marrow, they usually run in the limitrophic cord for some extent upwards, in order to join a higher situated spinal nerve; more rarely downwards, to join a lower situated spinal nerve. As to the motor nerves of the intestines, it remained undetermined whether they extend into the brain; as to the sensitive nerves. Henle believes that it may be affirmed that they do extend into the brain. If this view be correct, and if the gray substance of the ganglions, as well as that of the brain and spinal marrow, is a conducting medium, smaller tracts of the intestines must be brought into sympathy through the ganglions in and on the organs, whilst larger tracts of the intestines must be brought into sympathy through the remoter and larger ganglions into which many nerves enter; and, lastly, whole tracts of the intestines, and the intestines themselves must be brought into sympathy with the external parts of the body through the spinal marrow.

The results of experiments on reflex motions correspond with this construction. Henle observed in mammifera and frogs, that the circular contraction which ensues on superficial irritation of a circumscribed region of the gut was limited to a very small extent, when the gut was cut off close to the mesentery; that, on the contrary, the contraction extended to the neighbouring parts in a progressive, peristaltic manner, when, after destruction of the brain and spinal marrow, the connexion of the nerves of the gut with the ganglions was left undisturbed; and, lastly, that the most extensive peristaltic motions took place when the gut was locally irritated in recently slaughtered and irritable animals in which the central organ had not been injured. When organs wholly removed from the body respond to a partial irritation by movements in their totality, especially by regular movements, the conducting ganglions must be contained in the substance of the organ, as has been proved in regard to the heart, and rendered especially evident by Volkmann's experiment, in which, after the heart has been divided longways above a certain point, its synchronism is destroyed, and under certain conditions also the propagation of the excitement from one half to the other.

In order to produce reflected motions in the external parts by exciting the intestines, the spinal marrow must be entire, and in the frog the medulla oblongata also.

From the sympathies of the intestinal nerves *with each other*, reflex movements and reflex sensations are, in pathological and physiological processes, especially remarkable. Under the head of reflex sensations may be reckoned the pain in colic and during uterine contraction. As in external parts, reflex motions take place in general immediately under the part of the skin irritated, so reflex motions ensue on irritation of serous membranes as well as mucous, and that, as Henle believes he has remarked, more readily in the first than in the second case. Still certain regions of the mucous membranes also stand in relation with remoter muscles: thus, when there are worms in the intestinal canal, the pupil becomes dilated in consequence of the irritation of the sympathetic root of the ophthalmic ganglion.

In the relation of the intestinal to the external nerves, there appears, in the first place, to exist a sympathy between the walls of the cavities and

the contained organs; for example, not only between the scalp and the brain, between the skin of the neck and the larynx in which there might be suspected a vascular communication, but also between the walls of the thorax and lungs, between the walls of the abdomen and the viscera.

Sympathetic sensations. The following are common examples of sympathy. If the stomach is disordered we have headache; in disease of the liver, pain in the right shoulder; in disease of the spleen, pain in the left shoulder; itching of the nose and anus in worms; itching of the glans in nephritic calculi. In most cases in which external nerves participate in the state of excitement of internal, the pain in the former is the most prominent symptom, because, in regard to the situation and quality of pain in the latter, the consciousness is less definite. For the same reason, on the other hand, communication from spinal to sympathetic nerves is rarer.

Sympathetic motions. Involuntary muscles very frequently come into play during the action of voluntary muscles, thus the *iris* contracts when the eyeball is turned in, and what deserves to be especially remarked is, that the inverted position of even only one eyeball, or voluntary squinting inwards with one eye, has the effect of determining contraction of the iris of both eyes,—a sympathy, the ground of which cannot be well explained, except by a reference to the cerebral origin of the motor nerves of the circular fibres of the iris. As other examples of sympathetic motions, it may be mentioned that in laughing, sympathetic contractions of the abdominal viscera may occur, occasioning involuntary discharge of urine or flatulence. The sympathetic vomiting in coughing is well known. The muscular walls of the bladder, uterus, seminal vesicles, intestines, may be excited to action by voluntary contraction of the abdominal and pelvic muscles. The palpitation of the heart, which occurs in consequence of muscular exertion, is referred by Müller to the head of sympathetic motions, and Henle explains in the same manner the contractions of the bronchi and the necessity for accelerated respiration in ascending hills, &c.

Reflex sensations. Henle admits that there is scarcely an unequivocal example of reflex sensation between internal motor and external sensitive nerves, or between external motor and internal sensitive nerves.

Reflex motions from nerves of the trunk to internal nerves, and *vice versâ*, are the most frequent. Besides the cases, daily met with, of cough, hiccough, and vomiting from affection of mucous and serous membranes, there may be mentioned, as proofs of the peculiar relation between higher external and deeper internal organs, squinting in disordered states of the stomach and in worms, trismus from worms, as also, in the contrary case, vomiting from irritation of the optic or trigeminus nerve produced by pressure on the eye and in headache, &c.

2. Sympathies of the visceral nerves. *b. Antagonism.* Abercrombie first showed that in ileus there is not spasm but paralysis of the gut, which paralysis comes on sometimes alone, sometimes in connexion with inflammation of the mucous membrane; in the latter case the inflammation may have been present from the first, or have only come on subsequently. There is thus, says Henle, if we regard the appearances only, antagonism between the mucous and muscular coat—irritation of the former with paralysis of the latter. But another question arises, viz. whether the intimate connexion is here that of antagonism, whether the paralysis is

occasioned by the irritation of the mucous membrane operating through the nerves. Henle thinks that the same disturbance of the circulation which is the cause of the symptoms of inflammation in the mucous membrane, at the same time directly depresses the tone of the muscles.

In neuralgia of the fifth pair there is often mydriasis. If this be owing to paralysis of the circular fibres of the iris, the nerves of which come from the third pair, the case would be one of antagonism; but if the dilatation of the pupil be active, and owing to irritation of the sympathetic root of the ciliary ganglion, the case would be one of synergy betwixt this root and the sensitive fibres of the fifth pair. Facial neuralgia and cardialgia often alternate with each other.

3. Sympathies of the cellular tissue. The sympathies of the cellular tissue are limited, like those of the vessels, to sympathetic motions, reflex motions, and perhaps antagonistic paralysis.

As the iris moves in concert with the muscles of the eyeball, so the dartos contracts in concert with the contractions of the muscles of the perineum, and even of the lower extremities. Violent contraction of the sphincters of the anus and bladder, in order to resist the tendency to evacuation, has also for its accompaniment corrugation of the scrotum.

Cramp of the skin (*cutis anserina*) from disagreeable sounds and cold, is an example of reflex motion.

In local irritation of sensitive nerves, and local reaction of the cellular tissue, the cramp manifests itself at the irritated part of the skin: e. g. erection of the nipple, and contraction of the dartos from tickling of the superincumbent skin, bristling of the hair in violent neuralgia of the head.

4. Sympathies of the nerves of the vessels. The tone of the vessels admits of being inferred only mediately from the resistance which they oppose to the blood sent from the heart. The force of the heart and the quantity of blood being given, each vessel will yield to the wave of blood the more readily, and will be by it the more dilated the less resistance its muscular tunic offers. The distension may be immediately perceived either by the sight or touch in the larger vessels towards the surface. Strong pulsation, so far from being a sign of increased activity of an artery, is a proof that it is in a state of relaxation. The distension, and mediately the power of resistance of smaller vessels, may be inferred from the colour and turgescence of the tissues; for the broader the streamlets of blood in proportion to the parenchyma, the more does the red colour of the blood prevail; and the wider the vessels, the thinner their walls, and the more copious the exudation through them, which is indicated by swelling or effusion of plasma. In glands, increased secretion takes place in this way; in other parts, the symptoms of dropsy are developed, inflammation and its events. On the other hand, contraction of the vessels occasions paleness of the tissues, collapse, and, when the state continues, atrophy. Under certain circumstances when the arteries are contracted, the blood accumulates in the radicles of the veins, and occasions a blue, livid colour of skin, which may easily be distinguished from the redness owing to capillary congestion.

We are thus in a condition, due reference being made to the power of the heart, and the quantity and condition of the blood, to diagnosticate, in individual cases, the state of excitement of the nerves of the vessels. And as both conditions are always the same for all parts of the body, so

the comparison of the colour and turgescence of any one part with those of the others, gives us the measure for the actual excitement of the vessels of any one part.

This excitement becomes changed along with the activity of the organs, whether sensible or motor: it rises or falls with it in a direct or inverse ratio.

a. Synergy. 1. *With motor nerves.* In most if not all cases of muscular contracture, there is at the same time continued spasm of the blood-vessels of the part, not of the muscular branches merely, but also of the trunks and vessels of the skin covering the muscles. 2. *With sensitive nerves.* Sometimes in *tic douloureux*, and also in commencing inflammation; paleness precedes the redness, in other words, contraction of the walls of the vessels precedes relaxation.

b. Antagonism. 1. *To motor nerves.* This is manifested by the increased nutrition of a part much moved. In the active muscular exertion the vessels become relaxed, and therefore dilated, the consequence of which is increased exudation.

2. *To sensitive nerves.* Examples of this are:—increased excitement of sensitive nerves with diminished excitement of the nerves of vessels, and diminished excitement of sensitive nerves with increased excitement of the nerves of vessels. Increased excitement of sensitive nerves with diminished excitement of the nerves of the vessels, and consequently the dilatation of the vessels, is manifested by the symptoms of inflammation.*

Increased excitement of sensitive nerves from internal causes has also diminished excitement of the nerves of vessels for its consequence, as is shown by the flow of saliva, tears, and nasal mucus, by the injection of the conjunctiva, &c. in facial neuralgia, &c. Increased excitement of special nerves of sense is perhaps also a cause of diminished action of the nerves of the vessels, thus undue exertion of vision is followed by congestion of the eye. As an example of diminished excitement of sensitive nerves with increased excitement of the nerves of vessels, may be mentioned the contraction of vessels which, in cold, occurs in company with diminished sensibility.

The sympathy of animal and organic nerves, whether manifested as synergy or antagonism, presents itself as regards locality quite in accordance with the phenomena of reflex action.

5. Sympathies of the sensorial organ. The connexion betwixt the nerves of the body and the sensorial organ manifests itself sometimes as synergy, sometimes as antagonism. A thought, with passion, has for its consequence sympathetic movements and phantasies of the senses: the activity of the sensorial organ may, however, be so concentrated in the thought, that the motor nerves sink even below the average of their excitement,—below the normal tone. During intense contemplation of an object, the energy of the muscular nerves is diminished, as manifested in the features of the face by a paralytic-like expression; and in passion the members may refuse their office and the sphincters relax. On the other hand, bodily activity depresses the liveliness of the psychical affection; anger and vexation, sorrow and joy become worn out in the half involuntary muscular action, in the spasmodic respiratory movements which they first sympathetically gave rise to.

* See Report on Inflammation, in this Review, for April, 1844.

A direct communication between the sensorial organ and the nerves of vessels is not demonstrable. When in mental affections the tone of vessels is changed, there are always at the same time altered sensations, and also frequently unusual muscular action, and on these the condition of the vessels may immediately depend.

III. *Sympathies, the source of which is unknown.* Under this head Henle places,—1, the sympathy betwixt the breasts and the uterus, including the ovaries, by means of which irritation of one or other of these organs operates sometimes synergically, sometimes antagonistically, on the sympathetically connected organ. The breasts swell and are painful after conception (synergically), but also after delivery, and sometimes even after the death of the child (antagonistically); although in the latter case they more frequently become flaccid; lactation generally prevents menstruation, and limits the capacity to conceive, and if during lactation it should so happen that menstruation or conception take place, the awakened activity of the uterus puts a stop to the function of the breasts. Blisters on the breasts excite menstruation (synergically), and stop discharges of blood from the uterus (antagonistically). 2. The sympathy betwixt the parotid and the breasts and the ovaries in the female, betwixt the parotid and testicles in the male. Rheumatic swelling of the parotid transfers itself to the organs mentioned, and may, by means of irritants applied in the region of the parotid, be again brought back to this organ.

B. *Abnormal Sympathies.*

I. *Abnormal sympathies through the blood.* Henle gives the following illustrations of the conditions under which the blood may become the medium of an abnormally acquired sympathy:—1. Suppose the blood to contain an abnormal excrementitious matter, for the excretion of which the skin was delegated, and suppose, moreover, that the kidneys or any membrane possessed the power to attract and deposit that matter when it remained in the blood, then suppression of the cutaneous secretion would be injurious in a manner quite different from that in which it is usually injurious, and the organs which would suffer from the derangement of the function of the skin would be different from those which usually suffer. 2. A new antagonism is established in chronic dropsy, between the affected serous cavity and the kidneys, on which the cure of dropsy by means of diuretics depends; in gout, between the parts of the body which are the seat of the deposition of uric acid and the kidneys, so that the formation of gouty deposits alternates with gravel and urinary calculi.

These cases of morbid sympathy, however, are rare and doubtful.

II. *Abnormal sympathies through the nerves.* When a smell excites nausea or venereal desire, it is not because there is any particular sympathy between the nose and the stomach, or between the nose and the genital organs; the effect is produced through the mind. Again in persons disposed to toothache, overloading of the stomach will bring on an attack, not, however, because a sympathy exists between the stomach and teeth, but because the overloading of the stomach excites all the nerves, and the state of excitement of the nerves of the teeth already existing is readily increased to pain.

The external conditions on which the occurrence and spread of the communication of sympathy depends are the strength and kind of the

irritation. The greater the strength of the irritation, the more dispersed and extensive the movements induced,—the more prolonged the irritation (irrespective of strength), the more readily are the reflex movements produced. In paraplegics, tickling more readily excites reflex movement than pinching. Superficial inflammation of membranes is accompanied by greater reflex action than deep-seated and severe inflammation, thus, in superficial inflammation, of the respiratory mucous membrane, there is great coughing; of the nasal mucous membrane, sneezing; worms in the intestines rather produce nervous disturbance than enteritis; many purgatives operate well in small doses, but cause colic and stoppage in large.

Abnormally increased sympathy may be owing to a pre-existing excited state of the primarily affected nerve, the sympathetically affected nerve or of both at the same time. The occurrence and spread of sympathies are favored by the pre-existence of excitement and increased excitability of the nerves from which the sympathy sets out; thus, in severe inflammation of the eye, spasmodic closure of the lids, great contraction of the pupil, and lachrymation, which in the natural state are excited only by strong light, are excited by moderate light. Causes which would otherwise produce slight inflammation, will, in an excited state of the nerves, produce severe inflammation. The occurrence and spread of sympathy are favoured by increased excitement, and excitability of the nerves which secondarily take part in the excitement of the primarily affected nerves, for example, a paroxysm of neuralgia, will come on from motion of the neighbouring muscles.

Henle draws the conclusion that there is no particular organic disposition to sympathies, and that the spread of sympathy depends alone on the strength and kind of the irritation, and on the degree of excitability, or, which is the same thing, the degree of excitement.

COURSE AND EVENTS OF DISEASE.

1. COURSE.—*a. Duration. Type.* The oldest classification of diseases is that, according to their course, into *acute* and *chronic;* but though the difference be great, as respects course, between inflammation of the brain and phthisis, there are diseases which cannot well be referred to the head either of acute or chronic.

Three conditions are implied in the ideas of *acute* and *chronic*. First, the ideas refer to the absolute *duration* of the disease, *acute* is = rapid in course, *chronic* = slow in course; but in this point of view, the division has the value merely of an artificial system, as under it what is closely related may be widely separated. Secondly, the ideas acute and chronic were resolved into *febrile* and *non-febrile,* but although this holds very generally, still it is to be remembered that fever often occurs in chronic disease in its latter stages, and is sometimes absent in acute diseases. Thirdly, acute diseases were defined as diseases running through a certain length of course, and through an evident succession of stages; chronic, on the contrary, as irregular in their course, without decided progress to recovery or death. According to this view, acute is synonymous with *typical,* chronic with *atypical.* But in order to understand rightly the meaning of this distinction, and to judge of its value, it is necessary to take into consideration the foundation of type and the signification of the word.

We arrive at the type of a species, of a genus, of a family, when, on comparison of a series of individual species and genera, we recognise the accidental and peculiar characters, from the general and constant. The type of the species is, therefore, the law for the form of the individuals, and the form of the individuals is typical, or regular. But it is not the form merely which is typical, but also the development and the reactions on external irritants. When reactions are typical, diseases are so also, for disease is, according to the definition above given, nothing more than reaction against unusual irritants.

Properly there are none but typical diseases, but diseases are distinguished into *typical* and *non-typical;* diseases with regular and irregular course. On what is this distinction founded? With particular reference to their development in respect of *time,* those diseases are called typical which, besides the other specific characters by which several cases are recognised as belonging to the same species, resemble each other in their course; diseases, on the contrary, are called atypical when the course is subject to considerable deviations, without any evident specific difference to explain it. In general typical diseases are such as arise from causes operating once, and that quickly; the non-typical, on the contrary, such as are produced by causes operating gradually or constantly. Moreover, typical diseases are at the same time *pure,* whilst non-typical are in general identical with *specific, complicated, constitutional,* modified by the influence of predisposing causes. Lastly, typical diseases are those to which there belongs no other than the most general predisposition, whilst, on the contrary, most non-typical diseases presuppose a particular morbid, even hereditary predisposition.

In regard to the question how far the ideas typical and non-typical correspond to the ideas *acute* and *chronic.* It has been shown that neither the absolute duration, nor the accompanying fever, are characters according to which diseases may be naturally separated and divided into groups. It is otherwise with the type. Whether the course be regular or not, this depends, as just shown, on the most essential difference, viz. the etiological condition; it is of immediate influence on the determination of the nature of the whole morbid process, nay, even on the treatment, for it directly results that this must, in non-typical diseases, be also directed to the removal of the predisposing disease or the still operating cause. If then the words *acute* and *chronic* are to be used, as hitherto, to designate the two principal classes of diseases, which have been separated without reflection, it appears proper to impart to them the ideas *typical* and *atypical.*

It is accidental that most pure typical diseases are at the same time rapid in their course and febrile, and that most complicated or atypical diseases are at the same time slow in their course and without fever. The first are febrile, the second non-febrile, because fever symptoms, like so many other reactions which have their source in the nervous system, arise only from influences which operate suddenly and quickly.

Typical and acute cannot thus be taken as identical; for although the idea acute must comprehend more than duration, it nevertheless excludes slow diseases. In this case, acute diseases would thus comprise only a part of the typical diseases, viz. the miasmatico-contagious, besides congestions and inflammations, the course of which is always rapid and

progressive, and which, when the attack is at all severe, are usually accompanied by fever. Acute is thus almost identical with inflammatory.

In the course of acute diseases to death or recovery, there may, for the most part, be recognised a succession of distinct stages, of which *five* are pretty generally admitted.

1. The stage of commencement or premonitory stage (*St. opportunitatis, St. prodromorum,* and, as regards contagious diseases, *St. latentis contagii*). This sometimes appears suddenly with marked rigors; sometimes gradually with paleness, shivering, pains all through the body, nausea and anorexia, even slight fever, symptoms which are common to many diseases, and which do not yet indicate the specific form of the coming disease.

2. The stage of progression (*St. incrementi*), from the appearance of the essential symptoms to their full development.

3. The height of the disease (*St. acmes, staseos*), called also the stage of the crisis.

4. The stage of decline (*St. decrementi*).

5. The stage of recovery (*St. reconvalescentiæ*).

b. Periodicity—Rhythm. A disease is rhythmical or periodical, when one or all of its symptoms alternately rise and decline or disappear, in certain more or less regular periods of time, without any external cause having occasioned the increase. When there is no rhythmical rise and fall the disease is said to be continued.

Rhythmical disease may be at the same time typical or atypical, acute or chronic.

In rhythmical disease the organic condition of it continues even during the intermission; it is thereby distinguished from *recurrent disease,* which is a new disease called forth in a case of predisposition, by a newly applied external cause.

There are only two causes to which the periodicity of disease is referable; the injurious influence either operates periodically, or the morbid symptom depends on the changed action of an organ, the healthy life of which is periodical. We have first to determine the *cause* of periodicity in the normal processes of life. Then, in order to investigate the cause of periodicity in particular diseases, it is necessary to seek out, from among the physiological processes, those which are rhythmical. For all the processes of life are not rhythmical though all are typical.

Under the head of periodicity in health, Henle considers the normal periodicity of sleeping and waking, the periodical differences in the frequency of the pulse and respiration, and in the quantity of carbonic acid and animal heat generated, the periodicity of menstruation, &c., and then observes, in regard to *periodicity in disease*:—It was seen, in considering the healthy rhythmical functions, that in the life of the nerves a daily periodicity prevails. A continued organic change must therefore exhibit daily remissions, because the reaction of the nerves increases and remits rhythmically and in daily periods. It is probable that the rhythms of several days, at least the rhythm of three to four days, is owing to the same cause, seeing that the quotidian rhythm passes into the tertian and quartan, and *vice versâ,* and seeing that it is in accordance with the laws also of normal periodicity, that the duration of the periods immediately becomes doubled, when from any cause an exacerbation or remission fails to take place. Thus hunger, desire to go to stool, sleep, menstruation, if

once suppressed, do not recur immediately after the suppressing cause has ceased, but not until the next normal period. There are, however, on the other hand, reasons to suspect that there is a rhythm in the organs of the nutritive functions also, which comprises a series of days. For it is remarkable, that in quartan fever the disturbance of nutrition is so much more marked, and the affection of the viscera so much more considerable, though supposing the paroxysm be viewed as excitement, the longer duration of the intermission may indicate a slighter affection of the nervous system. If six to seven days' periods in normal nutrition be admitted, the whole duration of many diseases would embrace exactly one or several such periods. The monthly rhythm of diseases is readily explained by the periodicity of the sexual functions in the female, and of nutrition in both sexes. Even the annual course of certain morbid symptoms may be referred to a yearly rhythm in the growth of the body.

Henle would thus, speaking generally, refer morbid symptoms with daily rhythm to the nervous system; with monthly rhythm to the female sexual organs, or to the organs of nutrition; with annual rhythm to the organs of nutrition.

2. OF THE EVENTS OF DISEASE. Disease ends either in recovery or in death, or it passes into a new disease.

Recovery. When a disease ends in recovery, without the interference of art, the recovery is said to take place *spontaneously*, or by the *efforts of nature;* but, properly speaking, recovery is *spontaneous* only when it is altogether independent of new influences of any kind, accidental as well as intentional; in other cases recovery is *by art*, or is *accidental.*

Spontaneous recovery may take place in two ways, viz.—1, in certain cases by the simple expulsion, or cessation of the cause; 2, in other cases in which the morbid process has continued, notwithstanding the removal or cessation of the cause, by the supervention of some action or event to which the morbid process itself has at last led, such as hemorrhage, the bursting of an abscess, &c. In all this there is no ground for the admission of a *vis medicatrix Naturæ.* Even if the process from disease to recovery were less clear than it is, an explanation which, notwithstanding its modern metamorphoses, still betrays its mythical origin, should be rejected.

The formative power with which the different structures are endowed is not roused by irritation for the protection of the body, but is rather turned from its object, and that to the injury of the body. *After* the irritation the conditions are re-established, under which it operates normally. Every *reaction* is a morbid symptom, it is the rest and *restitution* which succeed the irritation which are curative.

In addition to this power, which operates in the preservation of the individual during the disease, inasmuch as it operates generally for the preservation of life, sympathy and antagonism are often the means by which prejudicial influences are rendered innoxious and diseases cured. The injury, by the accidents it occasions, often brings about the cure. Irritation of the mucous membrane of the stomach excites vomiting, irritation of the glottis, coughing, and by means of these sympathetical movements matters are sometimes expelled, the continuance of which in the body would have been injurious. But that sympathy is eternal, this use merely accidental! Every poison does not occasion vomiting, nor every

noxious gas coughing. Coughing may arise from superficial inflammation of the trachea before there is anything to expectorate, and vomiting from injection of tartar emetic into the veins when evacuation of the stomach is quite useless.

Crisis. This name has been given to the sudden disappearance of the symptoms of a disease after some marked evacuation. The idea of crisis is a remnant of the mythical beginnings of medicine. According to the ancient views, crisis is the expulsion, by the curative powers of nature, of a *materia peccans*. The symptoms of excitement of pulse, increased heat, delirium, spasm, &c., were viewed as manifestations of the action by which the *materia peccans* was elaborated and its evacuation brought about, and therefore called critical exacerbation, or *molimen criticum*.

Testing the question of crisis empirically, Henle shows, from the statistical observations of late years, how small the number of cases is in which what could be called a crisis occurred in comparison with the number of those in which there was nothing of the kind.

Transition into another disease. The disease which passes into another undergoes a change of form or place.

In regard to change of form (*Metaschematismus*) many different distinctions have been made, as, for example, the conversion of acute into chronic disease, of one general disease into another, of one local disease into another, &c.; but all that has been or can be said on the subject is worthless, if regard be not at the same time had to the cause of the disease. If a particular combination of symptoms is changed, in consequence of new causes, into another, it is evidently a figure of speech merely to call this the transition of one disease into another. It is really a new disease in the place of another. But if the second disease is developed out of the first, without new influences, as, for example, when congestion becomes inflammation, both are merely stages of one and the same process, and it is improper to separate them from each other under specific designations.

The idea of change of place of disease or *metastasis* had its origin, like that of crisis, in the mythical pathology. The materia peccans, supposed to be wandering about in the body in disease, was, according to that pathology, in crisis excreted by an organ not itself diseased, whilst in metastasis it was supposed to throw itself on an organ and occasion disease in it, a disease, however, sometimes curative, and then called *critical*. The transference of a disease from within outwards, with benefit to the patient, was thus also called crisis; and transference of a disease from without inwards, with injury to the patient, metastasis, in the limited acceptation.

Modern humoral pathology has sought, in accordance with the enlargement of chemical and physiological knowledge, to determine more accurately the nature of the supposed materia peccans, and the way in which it wanders. In regard to the first point, it has scarcely done more than institute hypotheses; and as to the second, it has agreed that the blood-vessels and absorbents convey the morbid matter, taking it up from one place and depositing it in another. When a general disease passes into a local, it supposes that the blood deposits in a particular organ the matters which occasioned the impurity of it, on which the general disease depended. When, on the contrary, a local disease passes into a general, the blood takes up the morbid matter from the particular part affected.

According to this modern view, a disease, in order to be recognized as

metastatic, must be the consequence of the cessation or non-appearance of another, and that what in the case of the one disease is repelled, and breaks out in the other, is a constituent of the blood. In the application of that idea of metastasis, a double error has been committed:—1, the causal connexion has been, often on too light grounds, presupposed, and any *second* disease, without farther inquiry, taken for a *secondary* one; 2, on the strength of the principle of explanation adopted, a number of hypothetical morbid matters, has been admitted, and the multifarious means by which the anormal action might be transferred from one organ to the other overlooked.

It is thus necessary to strike out from among the metastases the cases in which the supposed causal connexion does not exist, and in which the disease, erroneously put down as metastatic, is not owing to the first having ended. Having excluded the pseudo-metastases, Henle adduces the different modes in which true metastasis comes about, i. e. the different modes in which the arrestment of morbid processes gives occasion to the origin of morbid vital manifestations in other places; but he observes in regard to them that there is not one which perfectly corresponds to the definition of metastasis as given by the humoral pathologists. The common character is indeed transference of the morbid process, but in no case does this take place by the vessels taking up and transporting an already existing morbid deposit from one place to another. He does not indeed call in question the possibility of such a process, but says that its actual occurrence must be only under rare conditions, and suggests, as an example, certain of the so-called purulent metastases; those cases, viz. in which the sudden collapse of unopened abscesses is followed by the appearance of a sediment in the urine, purulent effusion into the serous sacs, or copious expectoration of matter from the lungs. In such a case, however, he remarks, that it could be the plasma only of the pus which is absorbed and again deposited; the corpuscles must remain where they were, and the corpuscles in the newly deposited pus must be of new formation.

Death. Death should be called natural, necessary or normal only when it occurs after the completion of the typical period of life. Death under any other circumstances should be called unnatural, accidental, or abnormal.

Death is cessation of the nutritive interchange. The sign of it is definitive stoppage of the function; if the function of an organ is striking, its death is soon recognised; in the opposite case the death of a part may remain for some time undetected. When the hairs in typhus fever die, the circumstance is not recognised; until they begin to fall out during convalescence; death of the brain, on the contrary, is instantly recognised.

All possible causes of accidental death, as well of the whole organism, as of individual organs, may be grouped under the two following heads.

1. Want of vital excitants, viz. oxygen and heat, which are supplied to the individual organs by the blood.

2. Alteration of the organic substance by physico-chemical influences, by which it becomes incapable of the typical reception of vital excitants.

Death may be like disease, general or local. Death is general when the destructive influence strikes all parts at the same time, as in the case of electricity and prussic acid, which at once destroy irritability in all the tissues. Local death is the result of a locally acting influence, or a

local disease. Local death, even the inactivity of organs, occasioned by temporary conditions, by the action of which the vital excitants are supplied to the others, necessarily induces general death. Such organs are the heart, lungs and medulla oblongata. These organs stand in a peculiar reciprocal relation: each sends to the other and receives from it the conditions of its vital manifestations. The innervation of the respiratory muscles depends on the medulla oblongata, the energy of the medulla oblongata on the decarbonization of the blood, in effecting which circulation and respiration must combine. Thus the loss of any one of the three links of the chain is fatal.

The outline which has now been given of Professor Henle's work will, we believe, afford the reader an idea of its spirit and scope, and justify the opinion as to its general excellence, expressed at the commencement of this article. We have thought it unnecessary to enter into any criticism of details.

Art. II.

Medico-Chirurgical Transactions. Vol. XXIX.—*London*, 1846. 8vo, pp. 352. Eleven Plates.

I. *On the minute anatomy and pathology of Bright's disease of the kidney, and on the relation of the renal disease to those diseases of the liver, heart, and arteries, with which it is commonly associated;* by George Johnson, M.D.

The secreting cells of the kidney, in a healthy state, contain a variable quantity of oil-globules, always considerably less than those of the liver. In kidneys affected with Bright's disease, the quantity of oil-globules is very greatly increased. The true definition, therefore, of the affection is, in Dr. Johnson's opinion, "an exaggeration of the fatty matter which exists naturally in small quantities in the epithelial cells of the healthy organ;" the disease being precisely analogous to the fatty degeneration of the liver. The accumulation does not take place simultaneously and equally in every part of the tubes. Those portions which form the pyramids do not become gorged in any great degree, except in cases where the disease has been of long duration, and in which the cortical portion of the kidney has become wasted. Neither is it common in the expanded portion of the tube which, as Mr. Bowman has shown, forms the investment of the Malpighian plexus.

The cells and tubes, thus engorged, comprise the surrounding capillary plexus, and give rise to congestion of the Malpighian plexus. This congestion leads to transudation of the serum of the blood, and sometimes to rupture of the delicate vessels, and the consequent escape of the colouring matter and fibrin of the blood. These constituents of the blood pass into the tubes, and so become mixed with the urine. Their escape from the blood-vessels is the result therefore of a mechanical impediment to the return of the blood through the veins, precisely as in the experiments performed by Dr. George Robinson, formerly noticed in this Journal. (Brit. and For. Med. Rev. XVIII, 366.) The atrophy of the kidney is also a result of the same compression, which materially interferes with the nutrition of the organ.

It has been frequently observed by pathologists that some affection of the liver coincides with the presence of Bright's disease: this affection Dr. Johnson believes to be most commonly fatty degeneration. In twenty-two cases examined by him, this state of the liver was most marked in 17; and in four of the remaining there was a decided increase of fat in the hepatic cells. He has also observed that in most cases the arteries have been found more or less affected with *atheroma* and *steatoma*, deposits which Mr. Gulliver has shown to be of a fatty nature.

All these facts point in the same direction, and would lead us to look for the source of the disease in some disorder of the processes of digestion and assimilation.

"The processes of primary or secondary assimilation, or both, fail with regard to this fatty matter, which, not undergoing the changes requisite for its ready elimination from the system, or for its application to the nutrition of the tissues, is thrown into the circulation. An effort is made to carry it off by the liver and kidneys; the fat finds its way into the secreting cells of these glands; its escape from these parts in a free state is a slow and uncertain process, and, finding no material in sufficient quantity with which to pass off in a state of combination, the fat accumulates in, and obstructs, the glands."

Such is a sketch of our author's views on this very important subject; they are sufficiently ingenious, but we refrain from giving our assent, until more extended observation shall enable us to decide with more assurance of having found the truth.

II. *Report of a case of ligature of the left subclavian artery, between the scaleni muscles, attended with some peculiar circumstances;* by J. C. Warren, M.D., Professor of Anatomy and Surgery, in Boston, U. S. A.

This is a very remarkable case. The patient, while in a state of intoxication, fell and struck his left shoulder against the kerbstone, on the evening of December 23, 1843. His shoulder was dislocated, and violent efforts were made for its reduction; but as these were supposed to fail, he was sent to the hospital. The parts were so much swollen and bruised, that the condition of the joint could not be ascertained for two days. It was then found that no dislocation existed. On the night of the 20th, he had a violent fit of coughing, and felt something give way in his shoulder. On the 30th, it was discovered that there was no pulse in the left arm, and that sensation and motion were destroyed. There were also some appearances of mortification. On the 27th January, a swelling formed in the axilla, and advanced gradually, till the 4th February, when it burst, and a gush of coagulum, with some fluid dark-coloured blood was discharged. Three days afterwards, there was a sudden and most profuse hemorrhage, which almost proved fatal. He rallied a little during the night, and next morning the subclavian was tied, but with great difficulty. During the operation the pleura was wounded, and a small quantity of air entered the chest. The condition of the patient now gradually improved, and the arm began to assume a more healthy appearance. The ligature separated on the thirteenth day: and seven days afterwards there was some secondary hemorrhage, but it was easily controlled. After this he had two attacks of congestive pneumonia, from which he recovered. Distinct pulsation was not felt in the radial artery until the 4th of February, 1845, 361 days after the operation. On June 15th, his general health

was pretty good, and sensation and motion of the arm were slowly improving.

III. *Two cases of disease of the brain, following the application of a ligature to the carotid artery;* by J. P. Vincent, Esq., Surgeon to St. Bartholomew's Hospital.

In the first case, death ensued six days after the operation, and the right hemisphere of the cerebrum was found soft and cream-like. The aneurism was on the right side.

In the second, the right carotid had been wounded by a piece of tobacco-pipe driven through the right side of the root of the tongue. The artery was tied seven days after the accident, on account of profuse bleeding. After the operation, the right side of the body was frequently convulsed, and the left paralysed. This state of matters continued until his death, which occurred on the twelfth day. On dissection, irregular cavities, filled with ash-coloured effusion, and shreds and particles of a greenish hue, were found in the right hemisphere of the cerebrum.

IV. *Case of punctured wound and ligature of the posterior tibial artery, in the upper third of its course;* by James Moncrief Arnott, F.R.S., Surgeon to the Middlesex Hospital.

Experience has shown that the hemorrhage from wounds of this vessel cannot be effectually restrained in all, nor even in a large proportion of cases, by ligature of the femoral artery, and that the general rule holds good that it is advisable, where such a proceeding can be adopted, to secure the vessel at the point of injury. To accomplish this, two methods have been proposed. Most surgeons recommend that the vessel should be reached by incision made on the inner edge of the tibia, between this and the gastrocnemius muscle, which is to be pulled aside, so that the soleus muscle alone is divided. Mr. Guthrie, and some others advise that the incision should be made directly through the whole thickness of the muscles of the calf. The latter was the plan adopted in the case here narrated, and with the most perfect success.

V. *An account of a case in which the corpus callosum, fornix, and septum lucidum were imperfectly formed;* by James Paget, F.R.C.S., Warden of the College, and Lecturer on Physiology at St. Bartholomew's Hospital.

We must refer our readers to the original for a description of the deficiency in this case; without the plate we could scarcely make it intelligible. The patient during life presented no symptoms whatever that would have led to the supposition of any malformation of the brain.

VI. *Case of aneurism, presenting some peculiarities. With remarks;* by Prescott Hewett, Esq., Lecturer on Anatomy at St. George's Hospital School.

In May 1839, Sir B. Brodie tied the external iliac for an aneurism in the groin. The patient, after recovering from a severe attack of peritonitis, was discharged cured. At the latter end of November, slight pulsation returned, but was removed by local pressure applied during two months. In November 1841, there was a slight recurrence of the pulsation, but no increase of size in the tumour. In January 1842, the tumour was larger, but had no pulsation. From this period, it gradually but steadily increased in size for the ensuing twelve months, during which time it grew to the size of the egg of an ostrich; its surface was somewhat irregular, and

softer in some parts than in others, although the tumour itself was perfectly solid. During the whole of the time neither pulsation nor sound of any kind could be detected. In January 1843, the tumour became stationary, and some time afterwards it began to diminish; the decrease was continued until July of the same year, when the patient died of phthisis.

On examination after death, the tumour was found lying upon the superficial femoral artery, at about a quarter of an inch below the point where this vessel comes off from the common femoral. It was as large as the head of a full-grown fœtus, slightly irregular on its surface, but perfectly solid. Upon being cut into, it presented the characteristic layers of coagulated blood observed in aneurisms which have been cured. These coagula, which had, for the greater part, lost their colouring matter, were disposed in very thin layers, closely packed together, and completely filling up the aneurismal sac, which was formed by the outer coat of the vessel, and remarkably thin towards its anterior part. The collateral vessels were much enlarged, but there was no abnormal distribution; the return of pulsation, therefore, must be explained by the situation of the tumour, which became affected by the large current of blood brought into its immediate neighbourhood.

VII. *Case of cyanosis of forty years' standing, depending upon congenital obstruction in the pulmonary artery, and patulous foramen ovale;* by Robert S. Spitta, M.B. London, House Surgeon to St. George's Hospital.

VIII. *On the ganglionic character of the arachnoid membrane of the brain and spinal marrow;* by George Rainey, M.R.C.S.

Mr. Rainey conceived the first idea of the resemblance between the arachnoid and sympathetic, from observing at the junction of two of the chords situated between the arachnoid and pia mater, a triangular body of the form and general appearance of a ganglion. He also observed a branch going from the chord connected with this body to the arachnoid membrane, along which it ran for a considerable distance, dividing and subdividing in its course, in the manner of a nerve; the successive subdivisions becoming more and more minute, and at the same time interlacing and inclosing areolæ filled with corpuscular matter. These corpuscles were so blended with the ultimate filaments of this chord, as to render indistinct their exact mode of termination. On further examination, he found that the opposite extremity of such a chord terminated by ramifying either on an artery or on a cerebro-spinal nerve.

The disposition of the ramifying filaments of the arachnoidal chords, and the form and size of the gangliform plexuses connected with them, bear some proportion to the number and size of the vessels in their vicinity. Hence, about the base of the brain, where the branches of the arteries are large, the plexuses are also large, and of an irregular shape, whilst on its upper surface, where the vessels are comparatively small, and more equal in size, and have a more uniform distribution, the plexuses also are smaller, more numerous, and more regular in their shape and size.

Besides the plexuses situated in the course of the chords of the arachnoid, there are others which are more intimately connected with its cerebral surface, and which, in some situations, appear to compose the entire

thickness of the membrane. In these plexuses, the filaments interlace very much in the same manner as the nerves do in the plexuses of the cerebro-spinal and sympathetic systems.

On comparing the filaments composing the arachnoidal chords with filaments of undoubted sympathetic nerves, Mr. Rainey found a very close resemblance. The nuclear fibre, described by Henle, was very rare; but the other two forms which exist in the sympathetic were exceedingly numerous, viz. that which consists of bundles of minute wavy filaments, intermixed with small particles of granular matter; and that which is made up of similar filaments, not always united in bundles, but apparently interwoven, and often totally destitute of granular matter.

Some of the plexuses on the cerebral surface of the arachnoid have the interstices, formed by their interlacing fibres, completely filled up with small roundish corpuscles, about the size of blood-discs; whilst, in others, these fibres are covered with irregularly oval masses of them. On this surface also, in various situations, there are well-defined round or oval bodies, having in their centre a granular nucleus surrounded by fibrous tissue, intermixed with more or less corpuscular matter. Of the nature and office of these bodies Mr. Rainey professes himself ignorant.

It is only those portions of the membrane which are made up of soft corpuscular matter, and the ramifications of the chords going to the arteries and nerves, which the author considers to be of a nervous character. We feel that much more extended observation is required before general assent can be given to views so novel. Two plates are given in illustration.

IX. *An account of a case of partial double monstrosity.* (*Ischiopage symelien of Geoffroy Saint Hilaire, Heteradelphia of Vrolik*); by W. Acton, Esq., Surgeon to the Islington Dispensary.

The subject was a healthy male Portuguese child, six months old. We quote Mr. Acton's description.

"The child is exhibited lying on its back in a little cot; is lively and good-looking, and well-proportioned, both in the upper and lower extremities, the peculiarities being confined to the parts below the umbilicus; a truss is worn on account of an umbilical hernia. Below the umbilicus, and to the right and left of the mesial line, are two distinct penes, each as large as the penis of a child of six months old: their direction is normal. I may mention that water passed from both organs at the same moment during the time that Dr. Cursham and Mr. Perry were examining the infant with me. Each penis is provided with a scrotum, the outer half of each scrotum containing one testicle, the inner half of the scrotum is far removed from the outer; and the two inner halves appear like another scrotum between the two penes. Between and behind the legs of the child we see another limb, or rather two lower extremities united together in their whole length. The upper part of this compound limb is connected to the rami of the pubis by a short stem, half an inch in length, and as large as the little finger, apparently consisting of separate bones or cartilage, for, on moving the compound limb, at the same moment the finger is kept on the stem, crepitation is felt, but I could not detect any pulsation. Immediately beyond this stem, and concealing it, the compound limb assumes a size as large as the combined natural thighs of the child, and within the upper part irregular portions of bone may be felt (probably a portion of a pelvis and the heads of the thigh-bones), which may be traced down, united together into one mass, to a leg of comparative small size, though still larger than either of the healthy legs, and terminated by a double foot in the position of *talipes*, with the sole turned forwards, and furnished

with ten toes, the two great toes being in the centre of the others: the two outer toes on each side are webbed.

"When the child is placed on its belly, the spine and back present a perfectly normal appearance; the anus is in its usual situation; the functions of the bowels are duly performed. Viewed in this position the compound limb assumes a roundness and fulness equal to the buttocks of a young child, and a slight depression is observed, as if for the anus. Tracing the limb downwards, we find only one patella, which is on the same aspect of the limb as the anus; the joint bends freely, and the compound extremity terminates as above described. This compound limb is quite motionless, the upper portion alone appears endowed with sensibility; its vitality seems low, as the toes have a blueish appearance. The upper portion, however, is of the same temperature as the body of the child."

X. *Some remarks on wounded arteries, secondary hemorrhage, and false aneurisms;* by Robert Liston, F.R.S., Surgeon to the University College Hospital, and Vice-President of the Society.

Mr. Liston's valuable practical remarks have relation to the well-known case of Mr. S. who was wounded in a duel at Gosport, in May 1845. The ball traversed the course of the femoral vessels a little above, and in front of the trochanter major. There was great immediate hemorrhage, and this was succeeded by swelling and all the symptoms of false aneurism.

"The nature of the case was very apparent. A large false aneurism, not well bounded, and rapidly increasing over the abdominal parietes in the iliac region, and arising from a wound of the femoral artery, or of some branch, divided close to its origin, had to be arrested in its progress, otherwise the patient must be left exposed to the risk of perishing suddenly, and at no distant period.

"After consultations on the evening of the 30th and morning of the 31st, with Drs. Mortimer and Stewart, Messrs. Jenkins, Sampson, and Potter, the external iliac artery was tied, with the loss of not more than a table-spoonful of blood, and with the immediate effect of arresting the pulsation, and of removing in a great measure the tension of the tumour. The corresponding limb, which had been somewhat swollen, was found, on the day after the operation, diminished in bulk, and of its natural temperature.

"Symptoms of peritonitis supervened this same evening, and on the following afternoon the patient sunk." (pp. 108-9.)

On examination after death, it was found that the injured artery was one of the superficial branches of the femoral artery, and was divided about half an inch below Poupart's ligament, and about an inch from the main body of the artery. The following summary explains and amply justifies all proceedings had recourse to in the case.

"I have thus endeavoured to show—

"1st. That the case of Mr. S. was one of great and immediate danger.

"2dly. That some decisive step required to be taken, and that without a day's delay, in order to avert the danger that impended over him.

"3dly. That very great risk would have been incurred in searching for and attempting to put a ligature upon the wounded part of the vessel.

"4thly. That there is ample authority for adopting the step which was had recourse to in the case. Ligature of the external iliac was undertaken, in order to ward off the expected fatal termination, through a sudden escape of arterial blood.

"And it may here be remarked, that we were not apprehensive of a remote danger, or labouring under a vague impression that something untoward might

arise; the danger was manifest. An artery had begun to pour out its contents impetuously, and the blood was only prevented from escaping externally by rotten tissues, that were about to separate, and could not by any possibility resist the impulse many hours longer. It must also be borne in mind, in considering the bearings of this case, that it might have been absolutely impossible to effect even the operation of tying the iliac, had the effusion extended ever so little further. No one will pretend to say that the operation of tying the iliac artery of itself is not attended with danger to life. But the dangers of mortification, secondary bleeding, &c., likely to arise from the operation, were considered as weighing but lightly in the scale against those impending from the effects of the pistol-shot. Mr. S. died, it has been seen, from the effects of inflammation of the peritoneal cavity. But this was scarcely taken into account at all, in consulting on the case. When we look at the result of the cases published in this country, from the time that Mr. Abernethy first tied the vessel, about fifty years ago, to the present period, it will be seen that we were justified in not laying very great stress on this source of danger. It appears that out of 45 cases recorded, (and collected by Mr. Crisp, in an excellent paper on aneurism, to which was lately awarded the Jacksonian prize by the Council of the College of Surgeons,) 9, or one fifth, died; three of those from sloughing and bursting of the sac, two from mortification of the limb, two from secondary hemorrhage, one from disease of the chest, and one from sinking of the vital powers. Many unsuccessful cases have no doubt been left unpublished, and some in which the patients suffered from inflammation of the peritoneal membrane might be adduced.

"In the case of Mr. S., the sac no doubt might have sloughed, secondary bleeding might have occurred on the separation of the ligature, however carefully applied. But there was nothing to prevent the collateral circulation from being carried on freely, and nothing connected with the wound, or consequent aneurismal swelling, to prevent the free return of blood by the veins. In the opinion of those concerned, the step most likely to avert danger, and prolong the patient's life, was adopted; the only step, it has been shown, that could have possibly been resorted to with propriety or safety." (pp. 128-30.)

XI. *Case in which a large tumour was developed in the substance of the fifth nerve and its ganglion;* by James Dixon, Esq., Surgeon to the Royal London Ophthalmic Hospital.

XII. *On the capacity of the lungs, and on the respiratory functions, with a view of establishing a precise and easy method of detecting disease by the spirometer;* by John Hutchinson, Surgeon.

It is quite impossible to give anything like a full analysis of this most elaborate production, but we have no hesitation in recording our deliberate opinion, that it forms one of the most valuable contributions to physiological science that we have met with for some time. In all future investigations into the phenomena of the respiratory processes, the name of Mr. Hutchinson must receive honorable notice. His inquiries have been made upon a very large scale, 2130 individuals of various ages and employments, of both sexes, in health and in disease, having been subjected to examination.

The quantity of air which may be contained in the chest, under different circumstances, is divided by Mr. Hutchinson into four portions. 1. *Breathing air,* or that which is drawn in and expelled during ordinary respiration. 2. *Complemental air,* or that which may be introduced by the deepest possible inspiration. 3. *Reserve air,* or that portion which may be expelled by the fullest expiration. 4. *Residual air,* or that portion which always remains in the lungs, and over which we have no

control. The first three, taken together, form what he denominates the *vital capacity* of the chest, and it is this which is measured by the spirometer.

Now this vital capacity is found to be modified in a determinate manner by certain conditions, viz. the height, weight, and age of the individual, supposed to be healthy. Of these, by far the most important is height, *why* we cannot tell, but the number of observations is too great, and the results derived are too uniform to admit of the least doubt. It appears, then, that for every inch of height (from 5ft. to 6ft.) eight additional cubic inches of air, at 60°, are given out by a forced expiration.

Weight has a less decided influence. At the height of $5\frac{1}{2}$ feet, the vital capacity is not affected by any variations of weight under 161 lbs. or $11\frac{1}{2}$ stone; but above this point it is diminished in the relation of 1 cubic inch for every lb. up to 196 lbs., or 14 stone. At other heights, between 5ft. 1 inch, and 5ft. 11 inches, ten per cent. may be added to the mean height, before we allow the weight to affect the vital capacity in the relation of one cubic inch per lb.

In regard to age, from 35 to 65, there is a decrease of rather more than one cubic inch per year.

We subjoin a very useful table showing the *normal* vital capacity in relation to height. The third column gives the result of arithmetical calculation.

Height.				From Observation.	Regular Progression.	Height.				From Observation.	Regular Progression
ft.	in.	ft.	in.	cub. in.	cub. in.	ft.	in.	ft.	in.	cub. in.	cub. in.
5	0 to	5	1	174	174	5	6 to	5	7	229	222
5	1	5	2	177	182	5	7	5	8	228	230
5	2	5	3	189	190	5	8	5	9	237	238
5	3	5	4	193	198	5	9	5	10	246	246
5	4	5	5	201	206	5	10	5	11	247	254
5	5	5	6	214	214	5	11	6	0	259	262

In prosecuting his inquiries, Mr. Hutchinson has arrived at some results, which are totally at variance with generally received opinions. Thus, he does not find that width or depth of chest exercise any special influence over the vital capacity. Tall persons with narrow chests invariably expire more than shorter individuals with apparently more capacious chests. It appears that the mobility of the walls of the thorax has very considerable influence.

He has also shown that the ordinary descriptions of the movements of the chest and abdomen are erroneous, but for information on this, and many other points of great interest, such as the elastic power of the ribs, the action of the intercostal muscles, &c., we must refer the reader to the original; assuring him that by a careful perusal he will not fail to gain much new and useful knowledge.

XIII. *Hydatid cyst, either originating in, or pressing upon, the prostate gland;* by George Lowdell, late House Surgeon to the Sussex County Hospital.

XIV. *Cases of varicocele, treated by pressure, with observations;* by T. B. Curling, Lecturer on Surgery, and Assistant Surgeon at the London Hospital.

Mr. Curling relates several cases in which a cure was effected, and some

in which great relief was obtained by the application of a moc-main lever truss, at the external abdominal ring.

XV. *Account of a case of congenital deficiency of one kidney, with granular degeneration of the existing one;* by G. Busk, Surgeon to the Seaman's Hospital Ship "Dreadnought."

This is an interesting case. The subject, a gentleman æt. 27, had enjoyed good health until within the last three years of his life. At this period he became bloated and pale, and incapable of as much exertion as formerly. He did not come under medical treatment, until about $2\frac{1}{2}$ months before his death; but previously to this he had taken a considerable quantity of calomel, by the advice of a friend, and was salivated at the time when he was first seen by Mr. Ceeley, of Aylesbury. The urine was albuminous. He was very subject to epistaxis. When the ptyalism subsided, a small crack-like sore was observed on the frænum linguæ, from which blood constantly oozed. This never healed, and a few days before death the under side of the tongue and the inner surface of the cheeks became gangrenous.

On examination after death, the heart was found hypertrophied to about double its natural size. All the valves were healthy. The liver was about one third larger than usual. It did not grease the scalpel, but under the microscope, presented a large quantity of oil. There was no trace whatever of the left kidney or supra-renal capsule. The right kidney was about 2 inches in length, by $1\frac{1}{4}$ in width, and presented externally a contracted or corrugated aspect. It was very pale, speckled with white, and on a section, scarcely any distinction of substances was observable. The whole presented a dense cicatriform aspect, of a semi-transparent hue, sprinkled with small opaque specks, and, in the remains of the tubular portions, marked with white striæ, obviously vessels filled with a dense white material. The pelvis was remarkably small, and the maxillary processes appeared also to be unusually few in number.

Under the microscope, the tubuli uriniferi were found to be in parts indistinct or obliterated, and in others to be filled with a semi-opaque, white, granular material, soluble, or rendered transparent by acetic acid, and presenting none of the characters of oil, very few globules of which were found in any part of the gland.

This case evidently does not bear out Dr. Johnson's views of the pathology of Bright's disease.

XVI. *On a particular derangement of the structure of the spleen;* by J. B. S. Jackson, M.D., Boston, U. S. A. Communicated (with some introductory remarks and comments) by Thomas Hodgkin, M.D.

This pathological change was described by Dr. Hodgkin in 1832, and by him ascribed to the extravasation of blood caused by local injury. Professor Rokitansky, on inspecting his specimen, recognised the appearance as one familiar to him, but affirmed his belief that it was one of the effects of endocarditis, and was produced by the poisoned condition of the blood. Recently, Dr. Hodgkin has received a letter from Dr. Jackson, of Boston, recording his experience. Part of this we shall quote, and our readers will learn from it, both what the morbid appearance in question is, and what its connexion with cardiac disease.

"The whole number of cases is nine. The disease is found in regular masses,

varying from a quarter of an inch to one inch in diameter; and generally several were found in the same organ. These were situated in the edge, or very near to it, and not upon the two broad faces of the organ; and they appeared to extend from the surface into the substance of the organ, being never, I believe, found in the substance without coming to the surface. They were perfectly defined, and often surrounded by a little loose cellular tissue, but were never encysted. The colour was the striking characteristic; it was always more or less yellow, except in one case, where it was evidently stained with blood; sometimes a faint yellow, but more generally bright. In one case it is described as an 'ochre;' in the second, as a 'gamboge' (i. e. gamboge when moistened by water); and in a third, as being of a bright golden yellow colour, contrasting strongly and very beautifully with the deep red colour of the rest of the organ. Sometimes in the less perfected specimens there was an intermixture of red. The masses are perfectly opaque, the cut surface is smooth, and generally they are of the consistence of fibrin; blood-vessels were distinct in some specimens, but generally they could not be seen. The surface of the organ corresponding to these masses was opaque, the yellow colour showing through, more or less from beneath; but in one case, the disease was only marked externally by a deep red colour; the whole surface was sometimes depressed, and sometimes not at all so, but more frequently there was a narrow depression around the limits of the disease; old peritoneal adhesions were sometimes found in this part. The spleen itself was generally noted as of the usual size.....Nothing was ever seen in the organ like tubercular or malignant disease, nor was there any trace of pus either in or out of the diseased masses.....You are inclined to regard it as the effect of external injury.... but, in my cases, no allusion is made to any such explanation, the fact being overlooked, if it existed, as I was not aware at the time of your observations. In five of the cases it is expressly stated that there were no tubercles in the lungs—an interesting point, as bearing of the nature of the disease of the spleen; in two, who died of anything but pulmonary complaint, the fact is not noticed; in one there existed a few latent tubercles, and one patient only died of phthisis. .....One thing, however, was common to every one of the nine, whatever was the disease that caused death,—and that was, an organic disease of the heart; and, in five of the nine, there were found vegetations upon the valves; in one of these last there was fibrin adhering to the inner surface of the left auricle, and becoming organized; in another (not one of the five) there was, in the appendix of the right auricle, a mass of fibrin as large as the top of the thumb, and completely infiltrated with pus, as some would say; in one of the five cases of vegetations of the valves there was also arteritis."

XVII. *On a luminous appearance of the human eye, and its application to the detection of disease of the retina and posterior part of the eye;* by William Cumming, late House Surgeon to the London Hospital.

Mr. Cumming has observed that the healthy human eye, when properly examined, presents a luminous appearance, like that of cats, dogs, &c. To detect this, it is necessary that the eye should be at some distance from the source of light; the distance being greater in proportion to the intensity; that the rays of light diffused around the patient (and sometimes around the eye itself) should be excluded; and that the observer should occupy a position as near as possible to the direct line between the source of light and the eye examined. These conditions may generally be obtained by placing the person to be examined in a dark room, and looking at his eye through a half-opened door.

In a few cases of disease of the retina he has found this luminousness destroyed or lessened, and he proposes it as a means of diagnosis.

XVIII. *Account of a case in which an abscess in the neck communicated*

by an ulcerated opening with the arch of the aorta, and in which the hemorrhage did not prove fatal in less than forty-eight hours; by George Busk, Surgeon to the Hospital Ship, "Dreadnought."

XIX. *On the intimate structure of the human kidney, and on the changes which its several parts undergo in "Bright's Disease;"* by Joseph Toynbee, F.R.S., one of the surgeons to the St. George's and St. James's General Dispensary.

This valuable and interesting paper details the results of a series of investigations undertaken by Dr. Bright and Mr. Toynbee. Without the aid of plates, it is scarcely possible to convey an accurate idea of parts, so minute and so complicated. We shall, however, attempt at least to bring before our readers so much as will enable them to understand the pathological views of our author; and in doing this we shall presume them to have a general notion of the minute anatomy of the organ, and shall not, therefore, occupy either their time or our own by explanations, which to one unacquainted with the subject would be absolutely necessary.

The *parenchyma* of the kidney consists of active cells and corpuscles, which recent investigations lead us to believe bear an important part in the processes of development, nutrition, and secretion. It is probable that the blood is by them, in some measure, prepared for that further change which takes place during its diminution, by the agency of the epithelial cells, in its passage into the interior of the excretory duct. Mr. Toynbee is of opinion, from the results of frequent examinations, that the nervous filaments end by becoming continuous with the parenchyma: a fact, if established, of great importance.

Near the apices of the mamillary processes, the *tubuli uriniferi* are closely packed together; at the bases they expand, and in the cortical substance they are dispersed in all directions, and in a complicated manner. They are more regularly arranged on the surface of the kidney, to which, when successfully injected, they give a lobular aspect. Mr. Toynbee is at variance with Mr. Bowman as to the mode of termination of the tubuli, the latter denying that they ever communicate with each other. The following is Mr. Toynbee's view of the true relation of the tubuli to the *corpora malpighiana*:

"1st. The capsule of the corpus malpighianum, instead of being, as supposed, an expansion of the tubule, is a distinct globular investment, enveloping both the tubule and the tuft of vessels. 2d. This globular investment is neither continuous with the tubule, nor with the blood-vessels, but is expanded over them. 3d. Into one part of the capsule the artery enters, while the other receives the tubule. 4th. The artery divides and subdivides, so as to form a globular mass of capillaries in the interior of the capsule from which the efferent vessel emerges. 5th. The tubule, after penetrating the capsule, becomes tortuous, and twists into a coil, and after being in contact with the ramifications of the corpus, it emerges from the capsule."

The capsule of the corpus malpighianum is a thin transparent membrane, without fibres or cells. Its contents consist of convolutions of vessels closely aggregated together, and apparently attached to a central vessel from which they radiate.

The venous trunks of the kidney commence on the surface of the organ, in small stellated branches, which passing through the cortex, are there

considerably increased in size by the junction of the numerous ramifications at the bases of the pyramidal masses. The veins situated in the tubular region having entered them, they, by their union, form the larger vessels which surround the pelvis, and terminate at length in the renal vein.

Pathological observations. Mr. Toynbee adopts Dr. Johnson's views of the nature of Bright's disease, but differs from him in believing that congestion is the first step in the process. He recognises three stages of the disease. In the *first*, the kidney is enlarged, and innumerable black points are visible, which are the dilated corpora malpighiana seen through the capsule. The white spots, which derive their appearance from the collection of fatty matter, begin to be perceptible. The peculiar features of this stage consist of an enlargement of the arteries entering the corpora malpighiana; the dilatation of the vessels of the tuft, the capillaries and the veins; an increase in the size of the capsule of the corpus and of the tubuli, and a large addition to the quantity of the parenchyma of the organ.

In the *second* stage, the organ is very greatly increased in size, and its tissue flabby; the surface is smooth, and presents numerous white spots; and the capsule is but slightly adherent. The artery of the corpus malpighianum is so greatly enlarged, that frequently it equals the dimensions of the tube itself, and is eight or ten times its natural size. The vessels forming the tuft are likewise enormously enlarged. (This is the very opposite of Mr. Bowman's opinion.) The tubuli, also, are increased in size, aggregated in masses, and extremely convoluted.

In the *third* stage, the kidneys are smaller than natural, hard, white granules are prominent on their surface, which is more or less lobulated, and marked by the stellated arrangement of the veins; the capsule is adherent; vesicles of large size are frequently everywhere interspersed; and numbers of smaller ones stud the whole surface. The arteries are more contracted than in the second stage, and the malpighian tuft is so often changed from its natural state, that the greater part of its vessels are not capable of being injected. The capsule of the corpus has assumed a more contracted appearance. The tubuli are larger than in the preceding stage, and are gathered into rounded masses, which form the granules on the surface of the organ. The latter are of a white hue, and are most commonly fully distended with fatty depositions; though not unfrequently they appear like dark spots, the tubuli being full of blood. The parenchyma is hard, and is composed of elongated stellated cells, from the angles of which fine threads proceed, and communicate with each other.

The paper is illustrated by five beautiful plates.

XX. *On the relation between the constituents of the food and the systems of animals;* by Robert Dundas Thomson, M.D., Lecturer on Practical Chemistry in the University of Glasgow.

This fragment constitutes another step in the right direction. We sincerely trust the author will pursue his investigations, which cannot fail at length to lead to valuable results.

Art. III.

1. *On the Pathology and Treatment of Dysentery, being the Gulstonian Lectures, delivered at the College of Physicians in February,* 1847. By William Baly, M.D., Physician to the Milbank Prison, and Lecturer on Forensic Medicine at St. Bartholomew's Hospital. From the Medical Gazette.—*London,* 1847. 8vo.

2. *Observations on the History and Treatment of Dysentery and its Combinations, &c. &c.* By William Harty, M.D., Physician to the King's Hospital and to the Prisons of Dublin. Second Edition.—*Dublin,* 1847. 8vo. pp. 298.

3. *Notes on the Pathology and Treatment of Dysentery, as observed in the European General Hospital at Bombay during the Five Years from July,* 1838, *to July,* 1843. By C. Morehead, M.D. (Transactions of the Medical and Physical Society of Bombay, No. VII. Presented April, 1845.)

4. *A Dictionary of Practical Medicine.* By James Copland, M.D., F.R.S. Vol. I. Art. Dysentery.—*London,* 1844. 8vo, pp. 1056.

Perhaps there is no single disease that has received a more voluminous illustration at the hands of writers than dysentery. And although the European practitioner of the present day has seldom occasion to witness the disease in that severe and epidemic form which called forth the observations of Sydenham, Willis, and Morton, or, at a later date, of Huxham, Zimmermann, Durandeau, or Degner, although that concourse of unfavorable hygienic conditions which in London, in the 17th century, caused dysentery to be one of the most fatal diseases in the bills of mortality,* no longer exists, still, even in temperate climates, the active causes of the diseases are but dormant, not eradicated, and are therefore not incapable of development. A famine in Ireland may recall to us, for a time, those scenes which the general misery of the population in London rendered familiar to Sydenham; and the military surgeon of our own day has witnessed in the track of the French armies, or even of our own troops in Spain and Egypt, the same devastations and the same mortality described by Pringle in the campaigns in Flanders. And although, except in such circumstances, dysentery has ceased to be a complaint daily witnessed at our own doors, this disease is still well known to the subjects of a maritime power like England, in whose vast colonies, situated in every climate and on every soil, it is certainly the most fertile in bad consequences, if not the most rapidly fatal and destructive. If cholera and yellow fever are unparalleled in the tremendous rapidity of their action, and in their resistance to the powers of medicine, dysentery is no less formidable from its oftentimes insidious nature, from its tendency to recur, and from the after-influences it exerts on particular organs, or on the system at large. Therefore almost all writers on the diseases prevalent in the colonial possessions of England have placed dysentery at the head of the list of severe affections, and have referred to it as the cause and

* The deaths from 1667 to 1692, recorded under the heads of "bloody flux" and "griping of the guts," amounted to 2000 per annum. (Heberden on the Increase and Decrease of different Diseases in London, 1801.)

origin of many of those chronic and intractable abdominal diseases witnessed so often in Europeans resident in tropical climates.

In their accounts of this disease, the best writers have differed among themselves. Pringle remarks of Sydenham and Willis, that in their description of dysentery they scarcely agree in anything beyond the name; and if the differences among more recent authors are less conspicuous, they are still numerous and important. This discordance apparently arises from the fact—that dysentery, although *per se* a simple and uniform disease, is yet liable to constant changes in type, from differences in the causes which produce it; in other words, from alliances with other diseases. To a disquisition on dysentery, considered in this point of view, the first edition of Dr. Harty's work, published in 1805, was devoted; and the expected prevalence of dysentery at the present time in Ireland has induced him to publish a second edition, in which, with some important alterations, his former views are ably argued and sustained. The following propositions present a summary view of the conclusions he has arrived at:

"1st. That the genuine and simple dysentery is unattended by idiopathic fever, and is never of itself contagious.

"2d. That every other form of the disease, when epidemic, is a combination of the simple dysentery either with intermittent, remittent, or continued fever.

"3d. That the combination with continued fever alone is contagious." (p. 3.)

The same opinions had been previously adopted by Hoffmann, in his account of the epidemic dysentery of 1726. He distinguishes three forms —1st. Benign attacks without fever. 2d. Attacks with attendant fever, either quartan, or continued, or semitertian. 3d. Attacks attended by a most dangerous petechial fever, and this compound disease was highly contagious. Later writers have uniformly distinguished the simple and the complicated dysentery, and among these we may particularly specify Dr. Copland, who, in his elaborate article, has given us an admirable epitome of the accounts of writers, elucidated by his own experience of the disease. In the remarks we have to make, we shall follow the same order, discussing first the simple dysentery, and secondly, its combinations.

I.—SIMPLE A-PYRECTIC NON-CONTAGIOUS DYSENTERY.

The greater part of the difficulties connected with the inquiry as to what is "simple dysentery," arise from the imperfect study of the morbid anatomy of the disease by the older writers. Even Morgagni has left very few dissections of dysentery, as he declined to dissect any who died of the epidemic.* Pringle gives only five detailed dissections in his

* We are inclined to regard the opinions of Morgagni in a somewhat different light than some late writers on dysentery have done. Morgagni did not from observation deny ulceration in dysentery; on the contrary, he admits its frequency. In the 31st Epistle he writes thus: "Whether there are ulcers in the large or in the small intestines, it sufficiently appears from all these observations that the intestines were really ulcerated in those dysenteric bodies from whom they were taken." (He is referring to cases detailed by Brunnerus, Valsalva, Platerus, Bossius, Fantonus, Panarolus, &c.) "Yet," he goes on to say, "in those dysenteric cases whom we have it not in our power to dissect are we also to suppose ulcerations for this reason, that, as Celsus says, they have discharged some kind of mucous portion with blood and sometimes with portions of flesh, as it were? it is worth while accurately to consider this question." He argues this question at considerable length, and seems to agree with Sennertus, who regarded these excreted membranes as mere mucous concretions, using the term "mucus" in a loose sense, to express any effusion, even of the blood. Morgagni, however, states with Tulpius, that true membrane is sometimes discharged. He sums up the argument thus: "It is more than sufficiently shown above, that those bodies which are

'Diseases of the Camp and Army,' and Dr. Hunter informs us that Pringle told him he placed little reliance on any of the dissections made in the military hospitals. Durandeau, whose work contains a most learned discussion on the causes and symptoms of epidemic dysentery, knew so little of its morbid anatomy, that he believed the seat of the disease to be chiefly in the mesentery.* Of late years, however, the morbid anatomy of dysentery, both in tropical and temperate climates, has been studied with considerable care, and in speaking of the simple dysentery we shall confine ourselves almost entirely to this single topic. The admirable lectures of Dr. Baly, who has had the opportunity of extensively observing dysentery in the prison at Milbank, contain an able discussion on the morbid anatomy, and the attempt made in them to include in one general expression all the forms described by writers is particularly ingenious and interesting. If it should appear that this attempt has not quite succeeded, or rather if, after much consideration on this subject, we hesitate to accept Dr. Baly's expression of the morbid changes, in all its extent, we do so with the admission that he has introduced into the subject a new point of discussion, and has brought into a form which admits of an accurate and defined examination the chief post-mortem appearances ascribed to dysentery. Into this examination, we now purpose to enter as fully as our limits will admit.

We must, however, first allude to the chief differences in the accounts of observers at the present time. It is almost unnecessary to say, that all agree in considering dysentery to be a disease chiefly, and in some forms, entirely of the large intestine, and, in the first instance, of its mucous coat.

Healthy structure of the mucous membrane of the large intestine. We may briefly remind our readers that the elements of this compound mucous coat are as follows: 1st. A basement or proper membrane which doubles upon itself and forms the tubular follicles, whose orifices are seen when the surface is examined with a lens. 2d. A columnar epithelium lining all the convolutions of the membrana propria. 3d. A subjacent vascular layer containing the nutrient vessels, between the meshes of which lie cytoblasts and amorphous matter; and, 4th, certain bodies situated in the substance of the membrana propria, or even below this, and called the "solitary glands." The solitary glands, the basement membrane, and the epithelium are all extravascular, and derive their nourishment from the vascular layer beneath. The solitary glands are stated by Dr. Baly to be sometimes open and sometimes shut; to have thick walls not composed of a simple membrane, and to contain granular matter. Like the glands of the small intestine, these glands are comparatively much larger in the fœtus and in young children than in adults or in old people. In the fœtus Dr. Allen Thompson described them as simple closed vesicles. This appearance we have seen in the colon of a middle-aged man who died of Bright's disease, and who had taken elaterium before death. The glands resembled exactly the vesicles of varicella.

discharged by dysenteric patients in the form of membranes are no proof of the intestines being ulcerated, unless they are real membranes." He then asserts the same thing as regards the fleshy excrescences, and concludes that mere discharge of blood is no proof of abrasion of the mucous membrane, and he argues this point from analogical reasoning, derived from the stomach, uterus, &c. He follows, in fact, the same train of reasoning which has been used by many later writers on hemorrhage.

* Traité de la Dysenterie, vol. i, p. 173. Bruxelles, 1789.

In some cases it has appeared to us that the tubular orifices can be traced over these glands; at other times, they seem only partly to cover them, becoming shortened apparently as they pass towards the centre of the gland, till they cease altogether. When the glands are much enlarged, however, the follicles are seen round, but decidedly not on them.

Morbid Anatomy. Inflammation of the solitary glands has been considered by some late writers as the first morbid condition in dysentery, and the existence of these bodies only in the large intestines has been considered to be the cause of the limitation of the disease to these parts, except in long-continued or complicated cases. On the other hand, several eminent anatomists describe the process as always one of rapid, and, in the first instance, superficial inflammation, leading inevitably and speedily to mortification, and unattended by any special disease of the solitary glands. At least, we know not how else we can interpret the description given by Rokitansky, who states that even in the first or slightest variety, the mucous membrane is swollen and red, and may be removed in the form of a pulp from beneath the furfuraceous and vesicular epithelium; while in the after stages, and in the severer varieties, the mucous membrane is transformed into a gelatinous and easily separable substance; or is in the worst form in a state of complete sphacelus, black, friable, and offensive. Lebert's description sufficiently agrees with this to prove that he has observed the same pathological state. (Physiologie Pathologique, par H. Lebert, tome i, p. 216. Paris, 1845.) Between these two extremes we should perhaps place the description given by Dr. Morehead, which, as few probably of our readers have seen, we will give more in detail.

The morbid appearances of the mucous membrane are arranged by Dr. Morehead under four heads:

1. Changes in colour and texture of the membrane. The changes in colour are described as witnessed chiefly in chronic dysentery, when there is a dark, red, gray, or black discoloration, with softening or more generally thickening of the mucous membrane. The changes in texture consist in enlargement of the solitary glands. On this point Dr. Morehead remarks:

"Under circumstances of increased secretion the mucous follicles [the solitary glands are implied by this term] of the colon become more or less prominent, and their orifices very distinct. Under these circumstances the enlargement of the mucous follicles is as yet unaccompanied by inflammatory action, but there can be no doubt that this action very quickly succeeds, and is marked by a circle of vascularity attended with softening, surrounding the orifices of the follicles, associated in some cases with thickening of the mucous membrane, in others with ulceration." (p. 139.)

2. Effusion of granules of lymph in the colon, or in the small intestines in chronic dysentery, in persons of cachectic constitutions. The effusion of lymph for some extent over the surface of the mucous membrane in shreds or tubular portions. Dr. Morehead observes that this "is a morbid lesion which takes place, but," he adds, "I do not think it can be frequent in tropical dysentery. In the cases reported by me, I find it present only in one, and that was not a case of dysentery." (Op. cit., p. 140.)

3. Ulcers of the large intestines; these are transverse or circular.

"The first form, either in separate bands, or in several bands coalescing, is generally found after acute attacks of dysentery, and most commonly is associated

with more or less thickening of the walls of the intestine. The appearance of the ulcer varies according to the circumstances of stage, state of the contiguous tissues, and condition of the constitution of the subject of the disease. The bed of the ulcer may be occupied with a slough, or the slough having been thrown off, the muscular coat may be exposed, and the edges of the ulcer be irregular and thickened; or the surrounding mucous tissue may be in a more healthy state in itself and in its relation to the subjacent tissues, and the ulcer may show a tendency to cicatrize." (p. 141.)

. This form of ulceration is produced, according to Dr. Morehead, by effusion of serum and lymph in the subcellular tissue of the transverse folds: "Serum and lymph become effused into the subcellular tissue, and the relation of the mucous coat to those tissues on which its nutrition depends being thus altered, its vitality is lowered, and a predisposition to ulceration is created. The circular ulcers plainly originate in the mucous follicles." (p. 141.)

4. The separation of the mucous membrane in shreds or tubular portions. Dr. Morehead formerly doubted the occurrence of such separation of the mucous coat, but is now satisfied that it does occur, and attributes it to diffuse inflammation of the submucous cellular tissue, "analogous to the diffuse inflammation of the subcutaneous cellular tissue."

The lesions of the other coats of the intestine are stated to be thickening from "effusion of lymph into the submucous cellular tissue;" and from the general impairment of the nutritive process thence resulting, is produced "the easily lacerable condition not unfrequently found after death in the worst forms of the disease."

Combining together the several accounts of these and other recent writers on acute dysentery, and assuming, for the sake of the argument, that they all belong to the same form of disease, we find the chief changes in the large intestines to be an affection of a special structure, viz. the solitary glands, and ulceration commencing in these bodies; ulceration produced by effusion of serum, lymph, or pus beneath the mucous membrane; general thickening of all the coats of the intestine from effusion of lymph and hypertrophy of the muscular fibres; an acute inflammation of the general mucous membrane, producing even in the slighter form a failure in nutrition, as indicated by the pulpy state of the mucous membrane, and leading rapidly to the production of sphacelus.

All these forms have been met with by Dr. Baly, in the prison at Milbank. In the slightest or most elementary cases of dysentery, he found the solitary glands enlarged, red, and sometimes disorganized on the summits; the mucous membrane round them was also rough, and covered with an aphthous layer composed of epithelium, mixed with an amorphous matter, probably fibrin; in forms more severe, the changes were still most marked in the solitary glands, which had lost their vitality, and were converted into small sloughs; in addition, the mucous membrane in which they were imbedded was inflamed and thickened, and likewise altered on its surface; this alteration of the intermediate mucous membrane seemed to consist in a loss of vitality of part of the membrana propria; in one case these sloughs of the membrana propria formed "strips of a rough substance, which might be taken for portions of false membrane, or fibrinous exudation," but on examination with a lens, the orifices of the tubular follicles were seen on these patches. In some

cases this loss of vitality in the intermediate mucous membrane, was equally prominent with the affection of the solitary glands, so that these bodies had evidently perished simultaneously with the adjacent mucous membrane, and not previously. These gangrenous portions, implicating all the elements of the mucous membrane, were generally situated on the transverse folds or plicæ. In the third or most severe form of dysentery, the same process of sphacelation was found in its most intense degree; the mucous membrane was swollen and dark red; the submucous tissue was tumid and formed prominent masses; crusts of disorganized epithelium and lymph lay on the diseased surface, and rapidly dark sloughs were formed, involving the whole thickness of the inner coat.

It is Dr. Baly's opinion that all these appearances are to be referred to the same disease; that the special affection of the solitary glands, and the general sloughing of the whole mucous membrane are only degrees of the same action; and that the solutions of continuity in the mucous membrane, of whatever size and shape they may be, are all derived from the same process, viz. one of mortification and sloughing.

In acute dysentery, according to this view, the earliest changes occur in the vascular layer, which is subjacent to the extravascular basement membrane, glands, and epithelium. This tissue being attacked with inflammation, the blood stagnates and coagulates in the capillaries; thus deprived of nourishment the extravascular parts perish, and are thrown off as sloughs; if the inflammation be moderate, the solitary glands chiefly suffer, because they are the most destitute of vessels. "The glands lose their vitality, and together with the portion of surrounding tissue in which the circulation has ceased, are thrown off as small sloughs." (Baly, p. 11.)

If, however, the inflammation be more severe, the glands only suffer in common with the other extravascular parts, and are thrown off in common with them; thus are formed the larger ulcers in the severer forms. An ulcer once formed by the detachment of a slough may, however, Dr. Baly thinks, increase in size and depth by true ulcerative absorbtion, but this chiefly occurs in cases when the inflammation has become inactive.

"It is probable, that when the inflammation is no longer active, the mucous membrane dies in small portions, and that these dead particles of tissue are wholly, or in great part, destroyed by the fluid exudation, and not cast off as solid sloughs." (p. 11.)

This is, however, according to Dr. Baly, only an after occurrence; and the proper expression of the process is, that it is one only of sloughing.

The difference between this mode of regarding dysentery and that entertained by some other writers is greater than at first sight appears. For instance, we will take the anatomical appearances as detailed by Dr. Parkes, whose work we reviewed in a late Number. According to this writer, the anatomical signs of true dysentery are primarily derived from inflammation of a peculiar structure, the solitary glands of the large intestine; the first effect is an enlargement of the glands, and if the inflammation continue unabated, ulceration speedily sets in; if the inflammation still remain unchecked, the ulceration, after destroying the gland, spreads into the general mucous membrane, and afterwards implicates all the coat of the intestine.

"The inflammation spreads from the glands into the surrounding membrane, and then implicates the muscular, and it may be, the peritoneal coats. Lymph

is in this case effused copiously between and among the different tunics, and upon the rapidly extending ulcers, forming irregular, gelatinous, whitish or dark-coloured projections and masses; the whole gut may be covered in this way, and the ulcers are then disposed to become gangrenous."*

After the inflammation has extended to the submucous coat, according to Dr. Parkes, ulcers may be formed by effusion under the mucous membrane which is then raised in spaces of greater or less extent, and finally detached. In this way, he thinks, are thrown off in all probability those portions of mucous membrane of greater or less size which are occasionally discharged. This result, however, is regarded by him as only an epiphenomenon, and consequent on the true dysenteric process.

The chief peculiarity of this description is the prominence given to the special affection of the solitary glands; this is declared to be the elementary and essential change, although general inflammation, sloughing, or effusion of fluid into the submucous cellular tissue may follow in severe attacks. If, however, it could be proved that ulceration occurred on the transverse plicæ, or in other parts, or that there was general sloughing, without any particular development of the solitary glands at the commencement of the case, then it follows, either, that these are not cases of common uncomplicated dysentery, or that the description above given is partial and incomplete.

In Dr. Baly's opinion, the solitary glands are only primarily diseased, because they are the most extravascular parts; at least this is a corollary from his doctrine of the process being always one of sloughing; when the inflammation is intense, the other parts of the membrane suffer simultaneously, and sloughs are formed without any special disease of the glands. In the severer forms, and not in the mildest, the true type of dysentery is seen, and to explain the elementary steps of the process, it is necessary to consider chiefly the advanced phases.

It is true, however, that this is not the view formally proposed by Dr. Baly, who, in several places, distinctly intimates, that the solitary glands are primarily implicated, and in his third lecture, writes as follows: "In dysentery as well as in fever, the intestinal lesion is the effect of a chemical action taking place between the glands or the tissue immediately surrounding them, and a morbific matter circulating with the blood." (p. 28.)

Possibly then, unless we are interpreting Dr. Baly's opinion erroneously, this doctrine of sloughing does not, when critically examined, fully express Dr. Baly's own conviction of the part which the solitary glands play in the dysenteric process.

But it is necessary to examine this view a little more minutely; we have considered it as a generalization; perhaps we might almost say an hypothetical generalization. Dr. Baly has attempted to define a process which should satisfactorily account both for those cases in which the solitary glands are wholly or chiefly affected, and for those in which the general mucous membrane perishes without any special superabundant affection of a particular structure. Does this generalization then explain all the phenomena? are all the shades of morbid changes explicable by its aid? and is the classification of these changes, and of the symptoms which announce them, simplified by its application?

The questions we have to ask, are, whether there are proofs that the solitary glands perish, and are thrown off as solid sloughs; or, supposing

* Remarks on the Dysentery and Hepatitis of India. By E. A. Parkes, M.B. Lond. 1846, p. 13.

the solution of continuity to be thus formed in the first instance, is it a fact that the further progress of such solution of continuity occurs by a repetition of the same process, and not by true ulcerative absorbtion?

It appears to us that either of these positions requires more evidence to prove it than we find in Dr. Baly's lectures. It is true, he describes the summits of the solitary glands as occasionally presenting marks of disorganization or minute yellowish sloughs; but he gives no series of cases proving the existence of the same process in the case of the whole gland, as well as of a part of it, or of the tissue round it; and he details no gradual transition into those cases in which the general mucous membrane perishes.

If we consult authors who describe the commencement of dysentery with sufficient minuteness, we find that they never appear to have met with cases in which the whole solitary gland sloughed out, but to have found the gland enlarged, and then ulcerated at the apex in a minute point. This is nearly the description given by Dr. Hunter,* in his work on the Diseases of Jamaica, and his description is so clear and accurate, that although it has often been quoted, we shall make no apology for again inserting it.

"Upon a first view," says Dr. Hunter, in speaking of the morbid anatomy of dysentery, "the bowels, particularly the colon, appear irregularly contracted, and redder than natural at the contracted part. On cutting out portions of the gut and examining the internal coats, the appearances of disease become more evident. There are to be seen small tubercles like pustules, sometimes in a smaller, sometimes in a greater number; and *they are to be found in different stages, so that their progress can only be collected from several observations combined.* The same subject will frequently furnish in different portions of the gut examples of the several stages. Their progress appears to be nearly as follows: there is first, a small round tubercle of a reddish colour, and not more than 1-10th of an inch in diameter; it increases gradually till it be near a quarter of an inch in diameter, and becomes paler as it grows larger. In this stage, there appears a slight crack on the top with a slight depression, which gradually increases, and on examining the contents of the little tumour, I have generally found them to be of a cheese-like substance. The pustule, for though it contains no pus, I do not know any name more expressive of its appearance, is seated under the villous coat, between that and the muscular coat. As the opening enlarges, the edge becomes prominent, and the base becomes rough and scabrous, from which matter oozes out that is sometimes tinged with blood. Such is the progress of one, but they are often in clusters, and become confluent so as to form a rough, unequal, ulcerated surface, with a hard and thickened base. These morbid appearances probably take place, more or less, in all cases of epidemic dysentery."†

Hunter afterwards remarks: "Sir John Pringle has indeed mentioned mortifications, gangrene, and abrasions of the villous coat, none of which I have ever seen." (Op. cit. p. 184.)

In this description it will be observed that a special affection, evidently of the solitary glands, is described, and the intermediate mucous membrane is left altogether unnoticed. Considerable importance also is attached to

* On the Diseases of the Army in Jamaica. By John Hunter, M.D. London, 1796. p. 184.

† Morgagni had evidently seen the glands enlarged in chronic dysentery and diarrhea. In the 65th Epistle he speaks of the case of an old woman worn out by diarrhea. "The rectum was almost universally livid in consequence of inflammation, and was here and there tumid internally, but especially at the lower part, where a circular patch, extending to the extent of a man's thumb, was soft and prominent, and above this patch were protuberant, either true lenticular glands, or glands similar to them, lying at a distance from each other, reddish and brown in colour."

the fact, which Dr. Baly has passed over almost without notice, that great enlargement of the glands precedes in many cases any further change. These enlarged glands are not then thrown off, but a "crack forms on the summit," which slowly increases. Now whether this increases by true ulcerative absorbtion, or by some mode more analogous to sloughing, but of which we have not any very clear idea, is difficult to determine, and is perhaps unimportant. The great matter is that there are no "solid sloughs," or at least none were noticed by Dr. Hunter.

Dr. Parkes describes the early changes in the glands, without seeming aware that solutions of continuity are produced by their entire destruction and detachment.

"If the inflammation be slight," he says, "the glands acquire a considerable size, without any ulceration, and when this does occur it is chiefly at the apex, and the thickened contents of the gland are not immediately discharged; in the more acute cases, the glands are suddenly increased in size, and press equally upon the mucous membrane covering them, thus causing its uniform destruction, and the immediate discharge of the glandular secretion."*

Now in the case of these two writers, it is hardly conceivable that they should not have noticed the state of the surrounding membrane, if it had been in that stage of engorgement, which Dr. Baly's description would lead us to consider necessary, in order that the supply of nourishment to the extravascular solitary gland might be suspended.

Dr. Parkes proceeds further. In the very next paragraph to that we have extracted, he says:

"In the majority of instances, the intermediate mucous membrane is little affected until ulceration has advanced to some extent. The exceptions to this rule occur in those cases generally complicated with gastro-enteritis, or with scurvy, in which there is a superficial erysipelatous inflammation implicating the whole of the mucous membrane."†

These descriptions refer only to the commencing and slight cases; let us take another case in which sloughing was really going on. In his 31st livraison, Cruveilhier gives a plate of what we, as well as Dr. Baly, consider to have been a case of dysentery. Numerous sloughs are depicted of various sizes, but the majority are roundish, and in several places the sloughs are quite round and minute, being evidently produced by the destruction of only one or two solitary glands. Now in this case, which, Cruveilhier remarks, merits the name of Dothinenterite, the intermediate mucous membrane was perfectly sound. "Du reste, je ferai remarquer l'integrité parfaite de la membrane muqueuse dans l'intervalle des eschars."‡

Dr. Baly cites this case as an example of the diffuse gangrenous inflammation, but this "perfect soundness" of the intermediate mucous membrane seems to us to be irreconcilable with such an opinion. We should rather be disposed to believe that the gangrenous condition of the inflamed solitary glands was owing to a particular constitutional taint or disposition, for in the same case (one of puerperal fever) there was pus in the uterine veins, and erysipelas of the surface.

If we sum up our argument so far, it will be seen that the points most adverse to Dr. Baly's description are—1st, the enlargement of the glands,

* Op. cit., pp. 8-9. † Op. cit., p. 9. ‡ Cruveilhier, op. cit.

which mere stagnation in the surrounding capillaries could not accomplish; 2d, the perfect state of the intermediate mucous membrane in many cases, which membrane, as far as the mere hyperæmia or collection of blood is concerned, ought to be as deeply involved in the process as the solitary glands; and 3d, the total want of evidence that the glands are, in all cases, thrown off as sloughs.

Although we doubt, therefore, whether the whole gland is ever thus at once detached, there can be little doubt, that occasionally the first solution of continuity is produced by a small slough at the summit of the gland, as described by Dr. Baly. The enlarged gland, with its small slough, presents so great a resemblance in such a case to the smallpox pustule, that almost all authors have used it as a term of comparison. Thus Sir John Pringle, in his 5th dissection, describes protuberances of a roundish figure, and adds:

"We all agreed that we had never seen anything so nearly resemble the smallpox; they differed from variolous pustules in this, that as far as we examined them, they were of a firm consistence without any cavity. Mr. Hewson told us that he believed they took their rise from the cellular membrane which lies immediately above the villous coat, for that some days before, having opened another person who had died of the dysentery, he found the appearances much the same as in this subject, and particularly with regard to these tubercles which he had examined at more leisure. These protuberances were only in the larger intestines, for though we likewise opened the smaller, we could observe nothing similar to them there.*

It must have been a similar state of the glands which led Nyander to remark that: "Dysenteria epidemica scabies est intestinorum interna, ut dissectionibus cadaverum, Dissenteria defunctorum patet."†

Zimmermann‡ says: "Sometimes small pustules have been seen in the bowels, and in the intestina crassa little aphthæ, that bled when pressed, and looked like the flat kind of smallpox, only they are solid and without any cavities."

Chisholm, Ballingall, and a host of subsequent writers have also made similar remarks respecting the resemblance to the smallpox.

We do not think, however, that it is a fair induction to conclude that the process is always the same even in this respect; in many cases the dark spot on the summit, whether slough or orifice, which gives the resemblance to the variolous pustule is wanting, and the first solution of continuity appears as a minute ulcer; thus Dr. Parkes writes of one case: "All the stages could be seen here; there were glands distended with white substance, ulceration of these at their points, then destruction of glands, and production of small ulcers with raised edges, then the edges flattening as the ulcer increased in size, then the usual ulcers of all sizes." And again he writes of another case: "Some of the glands were a little reddened and hard, others were redder and harder; others had a minute orifice at the apex, and others were completely converted into small ulcers."

If these descriptions are correct, we do not see how it can be doubted

* On Diseases of the Camp and Army. 5th edition, 1765. p. 246.

† A Treatise on the Dysentery, with a Description of the Epidemic Dysentery of Switzerland in 1765. Translated from the original German of J. G. Zimmermann, M. D., by E. W. Hopson. London, 1781. p. 35.

‡ Amœnit. Acad., vol. v, Dissertat. lxxxii, p. 97.

that the glands in these cases were destroyed, either by true ulcerative absorbtion, or by a process indistinguishable from this.

Moreover, Dr. Baly so far qualifies his position, as to allow that the ulcers may increase "by a solution of the tissues" when the inflammation is subdued in severity. How can we determine the frequency of this occurrence, and decide when ulceration or sloughing is going on; or if ulcers will increase in this way, how are we to feel certain that when the inflammation is moderate, they may not even commence through the agency of the same process? And even when the inflammation is intense, the appearance which a fully-formed ulcer presents, with its sharp, jagged, overhanging, and vascular edges, here and there eaten away like a common active, cutaneous ulcer, is much more like what we have been accustomed to refer to true ulceration than to sphacelus.

We must now proceed to notice, very briefly, some of the more severe varieties of dysentery.

It is a fact, admitted by all, that there is a form of disease of the large intestine, characterised by the inflammation being intense, and leading rapidly to gangrene. Rokitansky's description applies more particularly to this form, and Dr. Baly relates a well-marked case "in a feeble young man, who, when he had been two years in prison, was attacked with dysentery, and died after nine days' illness." The other case related by Dr. Baly was one complicated with typhus, and therefore should be excluded from examination.

The great peculiarity about this disease is that, as a general rule, the solitary glands are not peculiarly and primarily diseased. Occasionally, however, the special affection of the glands is conjoined with the diffuse gangrenous inflammation. The lower part of the ileum is also more frequently diseased in this form, on which account Chisholm was disposed to believe there were two varieties of dysentery, one confined to the large intestines, the other extending to the ileum.

The absence of all affection of the glands in some of these cases is of course attributable, according to Dr. Baly's hypothesis, to "complete stagnation of the circulation and gangrene of the tissue;" the consequence being, that "no small points indicating the situation of the affected glands are seen, all these are lost in the extensive and inflammatory congestion and mortification."*

But it is evident that although this complete destruction may involve the intermediate membrane as well as the glands in the parts chiefly affected, it is highly improbable that at some parts the disease, in its slighter and more elementary shape, should not show itself, and that the several stages should not be seen in different parts of the intestine. To this obvious objection, Dr. Baly replies as follows:

"This hypothesis," he says, "describes correctly the kind of inflammation which exists in the cases of dysentery here referred to, but it may appear not quite satisfactorily to account for the total absence of all enlarged and solitary glands on those parts of the mucous membrane of the colon, which are only in the state of inflammatory congestion; such as are seen in some parts of these drawings. To explain this, however, I would suggest that possibly only the inflammation set up by the primary cause of the disease has the tendency to produce especial tumefaction and mortification of these glands, and that inflammation secondarily excited

* Op. cit., p. 487.

does not affect them more than other parts of the mucous membrane. Assuming this, we may explain the cases in question by saying, that in these instances all the glands attacked by the primary cause of the disease are involved in the gangrenous parts, while the inflammation in the portions which are not disorganized has arisen secondarily by sympathy with the neighbouring intensely affected portions, and not from the direct operation of the original cause of the disease; and that on this account the glands are not apparently affected."*

Now we must say that this explanation does not at all satisfy us. Are we, for the sake of an hypothesis, to assume that the inflammatory congestion and the diffuse sloughing, which are surely but degrees of the same process, are produced by two different causes? Are we to distinguish two diseases in the sloughing dysentery, one the real affection, and the other its consequence? Dr. Baly himself seems to admit the weakness of his position, for he immediately proceeds to adduce another reason:

"It should be borne in mind, too, that the solitary glands of the large intestine are not at all times in the same state of development, and that when dysentery attacks them in their lowest condition, as to size and activity of function, they will, in all probability, show themselves less conspicuously in the inflamed mucous membrane."

Put this explanation to the test; suppose an intestine, whose solitary glands are at their minimum of size, to be attacked with dysentery. What is the consequence? Why, that the solitary glands would become immediately enlarged, as activity of function is the earliest change in the disease. And if we could suppose in the severest forms, a part to die so rapidly that the solitary glands had not time to fill with contents, we could not imagine any similar cause to prevent the development of the glands on the less affected portions of the mucous membrane. In these portions why should we not see, as Dr. Baly expresses it, "the distinctive character usually given by the especial activity of the diseased process in these glands?"

Are we then to conclude that the diffuse gangrenous form is a different disease from that in which the solitary glands are primarily and principally affected? Dr. Baly remarks on this point:

"The difference of anatomical characters in different cases is sufficiently great to accord with such a view; if the affections of other parts complicating the disease in the large intestine, if the symptoms and the influences causing the disease in the respective cases, should present well-marked differences." (p. 437.)

After an examination of these several circumstances, Dr. Baly decides that the two forms are the same disease, differing only in degree, and the difference between them is only like that existing between the malignant and the benign form of scarlatina.

Our space will not permit us to enter into the examination of this question, but before finally leaving it we wish to direct the attention of inquirers to a phenomenon which has not yet been sufficiently studied. We have witnessed several times in dysentery, and in other cases in which there was dark inflammatory congestion of the mucous membrane tending towards diffuse sphacelus, roughness of the mucous membrane with effusion of granular lymph upon it. This lymph has been in many cases evidently effused over the course of the larger vessels, and it has generally occupied the summits of the plicæ. Although it has often been effused

* Op. cit., p. 437.

upon the mucous coat, in some instances it has seemed to press on and destroy this coat, and then by its disintegration and detachment to form apparently sloughing ulcers. We would recommend attention to this phenomenon, and to the condition of the larger vessels in these and similar cases. It may be that the state either of the vessels or of the blood has an effect on the type of the inflammation. But it is our part to probe and examine the views of others, and not to intrude our own, and we must now quit this portion of our subject with the expression of our hope, that we may have occasion at no long period to notice a further discussion on dysentery from the able pen of Dr. Baly. We doubt not that he will be able to explain his opinions, and to adduce his evidence with greater minuteness than the short space of the Gulstonian lectures has allowed him time for. We have criticised his description in no unkind or captious spirit, but simply with a desire to arrive at truth and certainty. We are sure Dr. Baly will agree with us when we say, that in dysentery, as in all other diseases, much remains to be examined and to be understood, and that the vantage ground we have gained, and the fresh light we boast, show us not the end of our labours, but a fresh country and an unexplored and untrodden soil. If no writer has yet given the full expression of the morbid anatomy of this disease, we must attribute it to the necessary imperfections of our science and to the inherent difficulties of the subject. But the advances lately made by Drs. Baly, Morehead, and others encourage us to hope that our progress will still be uninterrupted; if difficulties have arisen, we can recur to that great book of Nature in which lies the solution of our doubts and hesitations; and, as has been beautifully remarked by an eloquent writer on a very different subject, like the engraver, who, cutting deeply into his gem, trieth the impression upon the tender wax, and finding the lines not deep enough, is not disheartened, but with earnest labour returneth again to his pleasing task, so we, finding our definitions too narrow, and our descriptions too restricted, must return again to the great teacher, and with humble and attentive eyes, scrutinize anew the knowledge upon which she permits us to gaze.

II.—COMBINED AND COMPLICATED DYSENTERY.

The combinations of diseases are deserving of the closest study, and constitute indeed some of the most important as well as difficult points in medicine. This combination may be of different kinds; there may be mere accidental co-existence, with or without temporary modifications of symptoms and manifestations. Or the relation may be of a more intimate kind; diseases may arise from the same cause, or may act as causes to each other, or may produce that condition of body in which a previously latent cause becomes operative. To prove, however, this last intimate connexion between diseases requires no little care, and no slight amount of evidence. Even when the great number of times in which one disease follows another, proves that there is something more than accidental concurrence, the successive steps of the production of the secondary disease can at present be seldom determined. As certain diseases seem thus to compound themselves, so also some seem rather to antagonize and nullify each other, and of these "incompatible diseases," lists have been given by Rokitansky and others. But here greater caution even is necessary than in the former case, and conclusions should be deduced only from a

most extended series of numbers, otherwise errors result from limited observation, and from the occasional absence of the cause of the presumed incompatible disease. For example, Rokitansky asserts the incompatibility of dysentery and typhus, yet not only is this combination very common, but the express purpose of one of the works before us is to prove that the union of the two diseases gives its contagious character to dysentery.

The chief diseases with which dysentery complicates itself, in the proper sense of the term, are intermittent, remittent, and continued fevers. In these cases the active cause apparently produces both diseases. In other cases, according to some authors, dysentery tends to the production of other diseases; the most prominent of these are an affection of the liver in some shape, and less commonly, diseases of the kidneys, pancreas, spleen, bladder, or small intestines. The diseases with which dysentery occasionally allies itself by some nearer link than that of mere co-existence are rheumatism, erysipelas, scurvy, or purpura, delirium tremens, peritonitis, and phthisis. In all other cases the prevalence of dysentery with another disease is accidental.

The most important fact which results from the true complications of dysentery, is that they cause modifications either in the symptoms or in the post-mortem signs of the disease. Thus in one variety attended with remittent fever, the stools are not truly dysenteric, but are watery, yellowish, often without blood, while the anatomical signs in the large intestine remain the same; in the scorbutic variety the post-mortem appearances are changed, sloughing is more common, and the ileum participates constantly in the disease. A peculiar adynamic type is also often impressed on dysentery by malarious fevers, on which account several writers have assumed that uncomplicated adynamic dysentery has no existence, but that the lowered type is attributable always to the attendant fever. This is opposed, however, to the experience of other observers, and particularly of Dr. Copland, to whose views on this point we shall have occasion presently to allude.

The combinations which we shall now discuss are, 1st, with malarious fevers; 2d, with typhus; 3d, with diseases of the other abdominal viscera.

1. *With malarious fevers.* So common is the union between remittent fever in all its forms and dysentery, and so evidently are both diseases excited by the same cause, that some writers, as Williams, Bancroft, &c., have believed dysentery to be always, or in the majority of instances, of malarious origin. But this is perhaps an extreme opinion, since dysentery is often found to prevail without a single case of ague being seen at the same time. In temperate climates this complication was formerly much more common than it is now, and many of the recorded epidemics must be considered to have been of this variety. Thus the epidemic dysentery, recorded by Morton, was attended by a violent remittent fever, which indeed prevailed much more extensively than the dysentery, and which, apparently from its tendency to pass into typhoid fever, he terms "febris Συνεχης." It was of this fever that Cromwell died, and it raged from 1658 for several years, being interrupted only by the great plague in 1665, before which the lesser pestilence for a time seemed to disappear. The most useful treatment in the dysentery was bark and opium, nearly the same which, with the addition of local bloodletting, we observe to be

recommended by a late writer on this disease occurring in the tropics. The "febris dysenterica" of Sydenham was perhaps of the same kind. Dr. Harty remarks on this point:

"If this be not Sydenham's meaning, and this the origin of his opinion, I confess I cannot understand him, for if we take his phrase of "febris introversa" in its literal and obvious sense, the opinion he adopted must appear unfounded, there being no evidence of a "febris introversa," because in such a case the fever should disappear, the dysentery being substituted in its place; on the contrary, the fever does still exist in combination with dysentery." (p. 46.)

It did not always so exist, however, for Sydenham expressly mentions the occasional absence of all fever, and by his "febris introversa" we should rather infer that he intended merely to intimate that the active cause of the fever evidently produced occasionally dysentery and not fever, or, as Sir G. Blane remarks, that the two diseases were vicarious. (p. 449.) And later observers have noticed that in the same individuals dysentery and remittent fever will alternate.*

Hoffman describes two epidemics in 1719 and 1726; the first of these prevailed chiefly in low marshy places, and was associated with intermittents of the semitertian type, consequently it is referred "to some malignant miasma formed by the peculiar effluvia of the earth." The second epidemic was totally different, and did not prevail much in marshy places, but raged more in hilly and elevated spots; it was also sometimes contagious. The "putrid dysentery" of Bengal, described by Clarke, was in all probability this complicated form, or a combination of this with scurvy. Pringle noticed that exposure to the same cause produced marsh fevers and dysentery alone or in combination.†

Perhaps one of the most instructive examples of the combination is given by Roederer.‡ Intermittent fevers and dysentery were epidemic, in one row of houses, he proceeds to say, the epidemic intermittent fever raged alone; in another row, situated on higher ground, the dysentery alone raged, and destroyed many; while in a third row, situated between the two former, both diseases were very rare. (p. 31.) Of late nearly every writer on dysentery has noticed its combination with malarious fevers, in almost all parts of the world; and Jamaica, India, west coast of Africa, Walcheren, and the Low Countries generally, and some parts of Spain and Portugal, are the places which have appeared chiefly subject to it. The chief conclusions to which we arrive by a study of this combination, as far as it is known, are these: 1st, that marsh fevers and dysentery often unite in the same individual; 2d, that when they appear in different individuals, they yet often testify, by their simultaneous increase or decrease, the presence of a common cause; 3d, that they may supersede each other in some cases, or may alternate with each other in point of prevalence, as if they were produced by analogous but not identical causes.

The symptoms of dysentery in the malarious variety, either combined with intermittent or remittent fevers, or separate from these, present several varieties: 1st, symptoms of true bloody flux—dysenteria cruenta; 2d,

* Buel on Fever in Massachusetts; Hargreave on the Walcheren Fever.

† Cleghorn says, "A tertian is sometimes changed into a dysentery, or the converse." (Diseases of Minorca. 4th edition, 1779, p. 134.)

‡ Tractatus de Morbo Mucoso, J. G. Roederei et Car. G. Wagleri. Edited by G. H. A. Wrisberg. Gott. 1783.

symptoms more approaching to cholera, viz. loose, watery, copious stools, without much tenesmus—approaching to the dysenteria incruenta of Willis; these two forms are generally attended with manifest remittent or intermittent fever; 3d, adynamic symptoms, with loose, bloody, and grumous stools, and rapid failure of strength: a remittent fever often accompanies this form also, but in other instances, although undoubtedly produced by malaria, there is a total absence of all fever.*

The anatomical signs in the large intestine are apparently similar to those witnessed in the simple form of the disease; or rather no differences have been yet described.† The small intestines are often diseased. The dysentery at Milbank would appear to be of malarious origin, and Dr. Baly remarks, that the description given by Davis of the anatomical appearances in the dysentery attending the Walcheren fevers is "identical with the disease from time to time observed at Milbank."‡

* Morgagni was attacked with the dysenteria incruenta, and, in his 31st Epistle, art. 11, relates his own case admirably. He was travelling through a marshy district, and towards evening began to be attacked with purging—during the night he discharged "at least sixteen pints of almost limpid water; the pains were slight, the stools not frequent, but very large." On the following day he perceived that his face and body generally were grown lank and thin, as if from an acute disease, and his mouth and fauces were excessively dry. He distinguishes this flux from dysentery, although he states that the watery and the true bloody flux are both usually comprehended under the same term. "So I remember," he says, "that epidemic fluxes of this kind were called dysenteries by the physicians of Modena when they wrote to the physicians of Bologna, which appellation the latter disapproved not of." (Epistolæ xxxi.)

† Lebert indeed describes the "dysenterie blanche" as only an irritation, "très superficielle," of the mucous membrane, with an abundant exfoliation of epithelium and a great serous secretion (op. cit., p. 216); but there is no doubt that this is true only of the very slightest forms.

‡ The account given by Davis, although an admirable description of the morbid anatomy of malarious dysentery, is true only for that form, and in his work on Walcheren Fever he gives no dissection of uncomplicated dysentery. We will extract from his work (p. 155) the references he makes to the large intestine—in many cases it is altogether unnoticed. Case 9—"Slight dysentery, rectum thickened and ulcerated, spots of effusion, and many rough, irregular, and inflamed lumps." No. 11—"Inner surface of colon covered in several places with tubercles, and in others with small ulcers resembling chancres." No. 13—"Coats of colon thickened, and inner surface of rectum black and covered with ulcers." No. 16—"Inner surface of colon slightly inflamed." No. 17—"Colon and rectum contracted, tuberculated, and ulcerated on their inner surfaces." No. 19—"Colon and rectum formed one mass of disease; their coats retained nothing of their natural appearance, neither could they be separated from one another. A vast deal of coagulable lymph had been deposited between them, and had become condensed. Their whole internal surface was completely ulcerated, and when cleared of feculent matter was found to be rugged, in consequence of some portions of the inner coat having sloughed, and of others having received an additional lining from condensed coagulable lymph. Before these intestines were washed pus exuded from numerous small ulcers spread upon their inner surface." No. 20—"Colon contracted, and had numerous small ulcerations on its inner surface, rectum slightly ulcerated." No. 21—"Caput coli as large as the stomach, being distended with air, slightly inflamed on its internal surface, which had also a granulated appearance in several spots; colon contracted, and having transverse lines with small red eminences." No. 23—"Colon lessened in diameter, and bore marks of recent ulceration; the rectum had been ulcerated, but was healed." No. 24—"Rectum and colon completely gangrenous, of a dark colour on their inner and outer surfaces, and excessively offensive." No. 25—"Transverse arch of the colon inflamed on its inner surface." No. 28—"The colon was contracted, and had red stripes round it, like rings, upon which were numerous red eminences, of the size of a pin's head, placed in rows." No. 29—"Colon ulcerated, rectum much ulcerated, and had sloughed near its termination." No. 30—"Colon and rectum had numerous tubercles and ulcerations, the villous coat of the rectum had sloughed extensively, and was so much altered everywhere as to be scraped away with the slightest effort." No. 35—"Colon ulcerated within to the extent of three or four inches." No. 37—"Colon was lined with ulcers and tubercles of the kind I have elsewhere described." No. 41—"Colon red and scabrous." No. 42—"Colon slightly ulcerated."

It thus appears that, in 19 cases of Walcheren fever (out of 42 recorded dissections), the large intestines were diseased from attendant dysentery. In 6 cases the intestine is merely called ulcerated, or slightly inflamed. In 9 cases appearances are described which leave no doubt that the disease of the solitary glands was very conspicuous. In four cases only is sloughing mentioned. In 2 of these cases the rectum only was sloughing; in the rest of the intestine the solitary glands were diseased. This slough was doubtless from the severity of the inflammation, and is a common occur-

The complications of other organs in malarious dysentery are so far peculiar, that the spleen is often diseased; the liver is also affected, being generally described as enlarged and engorged, or shrivelled and black.* It has also appeared to us that abscess of this organ is seldom mentioned, and it is a curious subject for inquiry whether the changes which occur in the liver in this variety of dysentery, assume some form distinct from that condition which predisposes to abscess of the organ. If this were so it would reconcile some discrepancies to which we shall presently allude.

Two questions of great importance arise in connexion with this subject:

1. Is the miasmatic agent producing this dysentery identical with the true febrile malaria? It evidently owns the same conditions, being developed in marshy and humid situations; but it is a very remarkable circumstance that ague, the index of malaria, is so often disjoined from it. Rollo, who found a remittent or intermittent fever to be almost an invariable accompaniment of the dysentery of the West Indies, believed that the former only arose from malaria, while the latter was produced by a simultaneous or prior exposure to cold and moisture. But this doctrine will, in all probability, find few supporters, and it is more reasonable to suppose that the virus of dysentery may be only analogous to the true febrile malaria, and may be capable of existing either isolated or combined with this.

2. Are there any grounds for the belief entertained by Bancroft, Williams, and others, and which seems to be gaining ground at the present day, that dysentery always arises from a specific virus of malarious origin?

"From these facts," writes Dr. Baly, "as well as from the consideration of the whole character of the disease, I infer that dysentery is always produced by a poison introduced into the system from without, and that in most instances this poison is generated by the decomposition of matters contained in the soil."

The chief arguments against this view are: 1. The prevalence of dysentery in its acute sthenic form in places where no marsh diseases are prevailing, and where there is no apparent development of malaria. 2. The immediate reference often made by patients to some chill or exposure to cold and moisture at times and places when marsh fevers are unknown. 3. The occurrence of the disease on hills and in cleared spaces, and also at sea, where bowel complaints of different kinds often occur on sudden changes of weather, and cannot reasonably be referred always to effluvia from bilge-water. 4. The evident production of the disease by other effluvia than those from marshes; as, for instance, from animal effluvia, as proved by the observations of Sir James M'Grigor, Vignes, the authors of the article Dysenterie, in the 'Dictionnaire des Sciences Méd.,' Copland, Chomel, &c. Dr. Harty writes on this point:

"I do not say that marsh effluvia alone are not competent to induce dysentery, but this I maintain, that the influence of wet and cold in its production is more cleary, satisfactorily, and universally demonstrable than that of marsh effluvia." (p. 57.)

rence in dysentery. In the 3d case (No. 19) there can be no doubt that it was one of intense inflammation of the usual kind, with great effusion of lymph and consequent gangrene, as described by many writers on tropical dysentery. The 4th case (No. 24) is the only one resembling the diffuse gangrenous inflammation, but the account is so short, consisting merely of the lines above given, that it is difficult to decide. At pages 190-191, where Dr. Davis sums up the appearances in the intestines, he describes minutely the appearance of the "tubercles" forming little ulcers, but he only cursorily mentions gangrene, saying, "These ulcers always occasioned a contraction of the gut, which was mostly inflamed round them, and, in advanced stages of the disease, gangrenous."

* M'Grigor, Ferguson, Davis, Fallot, and many others.

An attempt was lately made by Cornuel* to prove that malaria had nothing to do with the production of dysentery. He states, that while the "Basse Terre" of Guadaloupe was ravaged with dysentery in 1837 (1·078 treated—123 died, or 11·41 per cent.), the "Grande Terre," separated only by a river, was totally exempt, (p. 102). He states that, from the situation of the town on the Basse Terre, the cold winds from the mountains cause frequent and rapid transitions of temperature. In these mountains also are numerous torrents which become swollen, "blanchâtres et troublés," after the rains and pour down into the plains. The water which is drank at this time has a peculiar though not a disagreeable taste. Dysentery only appears at the places where this water is drank. At the Grande Terre there are few rivers, and they drink chiefly rain-water. Miasmatic exhalations, M. Cornuel adds, are more common at the Grande Terre, where dysentery is less frequent.

The influences here alluded to, viz. sudden transitions of temperature, and unwholesome water, have been always assigned by writers as causes of dysentery. Van Swieten even says, with reference to this point, that cold seizing the body when heated has killed more than the plague.† Even in epidemic dysentery, obviously connected with fevers of some sort, Zimmermann, Roederer, Huxham, Durandeau, Willan, and many others, have also admitted the operations of vicissitudes of temperature, in producing epidemic as well as sporadic attacks. The French and English military surgeons during the last war admitted these changes, and also animal and vegetable exhalations as the active agents. (Vignes,‡ Sir J. M'Grigor,§ Ferguson,‖ and many others.)

We have not time to enter into this important subject, but we can assure our readers that the evidence in favour of both epidemic and sporadic dysentery arising from several causes, besides marsh exhalation, is incontestable, and we cannot but consider any opinion as doubtful and restricted, which regards peculiar anatomical characters as capable of production only by one particular cause or agent.

The question may be discussed in this place as to the real nature of Willis's "Dysenteria Incruenta." Willis states, that almost every autumn, but particularly in the years 1670 and 1671, two forms of dysentery prevailed. "In one the stools are watery and almost limpid, with a sudden prostration of the forces; in the other the stools are bloody, but the powers remain tolerable." In 1670 the first species, or dysenteria incruenta, was exceedingly fatal about the autumnal equinox, invading suddenly, and being attended with great vomiting, and frequent watery stools. In twelve hours some were moribund, with a weak and minute pulse, with cold sweats, and a gasping respiration. These symptoms are so similar to cholera, that Dr. Harty concludes that there can be no doubt on this point. "The season of the year, the mode of attack, the con-

* Mémoire sur la Dysenterie observée à la Basse Terre (Guadeloupe). Par M. le Docteur Cornuel. Mémoire de la Académie Royale de Médecine, 1840.

† Stoll says, "I have never seen this disease to occur except when the patients had to reproach themselves with having been exposed to cold while sweating."

‡ Traité complet de la Dysenterie. Par P. Vignes. Paris, 1825 pp. 204 et seq.

§ Medico-Chirurg. Transactions.

‖ Ferguson says, "True dysentery is the offspring of heat and moisture—of moist cold in any shape after excessive heat. Nothing that a man can ever put into him would ever give him true dysentery." (Op. cit., p. 188.)

junction of vomitings with copious stools, the debility and other effects immediately consequent, the rapid fatality of the disease, and the mode of treatment, all unite in pronouncing the disease to be cholera." (page 60.) And he quotes Hoffman, who distinguishes very accurately between cholera and dysentery, and who refers Willis's dysenteria incruenta to the former disease. Moreover, it appears that Willis did not confound his two species of dysentery, but states them to be "duas et *longe diversas* species."

It is rather singular, however, if this be true, that Willis did not call this disease "cholera," a term used constantly by all the writers of the period, and did not refer to the spasms so particularly described by Sydenham* (p. 277), in his account of the cholera of 1676. It must be remembered also that the combination of a malarious fever with dysentery sometimes alters its type, and impresses upon it more or less of a choleric character. This appears to be the case at the present day in the tropics. In speaking of this combination in India, Dr. Parkes says:—"The fever appeared always to impart a peculiar adynamic type to the dysentery; thus the non-effusion of lymph, the loose, dark, serous nature of the stools, *approaching in some cases to cholera*, looked more like a passive flux than an acute inflammation of the solitary glands." (Op. cit. p. 129.)

Now we know that at the time of the "dysenteria incruenta," malarious fevers were common, and in 1671, after an exceedingly cold winter and a very dry spring, they were so prevalent as to constitute an epidemic fever, which ravaged nearly all England, preserving an intermittent character for some time, but finally becoming remittent, and being complicated with true bloody dysentery, as well as with the dysenteria incruenta. It is possible, therefore, that this latter disease may have been a form of adynamic dysentery, in which, from some reason or other, the fever, usually developed by the common cause, was suppressed. But there is no doubt that there is some connexion or alliance between these forms of malarious dysentery, or even between intermittent and remittent fevers, and common cholera, and perhaps even the malignant Asiatic cholera. "The cholera morbus," writes Cleghorn, "is attended by a tertian," and in several places he describes very accurately this alliance of the two diseases. It would reconcile all difficulties if we could suppose the "dysenteria incruenta" to include in it a form of malarious dysentery, verging upon, and finally ending in common but severe cholera.

The disease described by Degner under the term "dysenteria biliosa contagiosa," was certainly the same, or nearly the same, as the "dysenteria incruenta" of Willis. Dr. Harty indeed considers it to have been an epidemic of true malignant cholera. There were copious discharges from the bowels, which were "flavæ aut potius subrufæ, sed sensim aut fiebant porraceæ, æruginosæ aut subcruentæ." The quantity of these was immense. "Almost the whole body was dissolved into liquid, and poured out by the intestines." There were also violent vomitings, epigastric pain, and rapid failure of the powers, testified by constant faintings, and diminution, with intermission of the pulse. He describes four cases: two deaths, each on the 8th day, and two recoveries.

The most characteristic symptoms of the Asiatic cholera, viz. the coldness and colour of the surface, the loss of voice, the cramps, the embarrassed respiration, and the *early* death, are left undescribed by Degner. In one

* Edition of the Sydenham Society, 1844.

of his cases, he says, on the 4th day "the face indicated approaching death, algebat per totum corpus;" and in the 4th case (a recovery), in an old woman, he speaks of "intercurrent convulsions." But it is unlikely that he should have overlooked these symptoms had they existed in the majority of cases, and it is, moreover, unlikely that true cholera should have been prolonged to the 8th day. We do not feel much doubt that Dagner's epidemic was one of modified malarious dysentery, the relationship of which, both to common sporadic and epidemic malignant cholera, we have, however, already admitted.

2. *Dysentery with fevers of a typhoid character.* Cullen's definition of dysentery, which includes "pyrexia contagiosa," has been severely criticised by some more modern writers on dysentery. There is no doubt, however, that this definition is as correct when applied to certain well-marked epidemic attacks, as it is inapplicable to the common form of the disease. Cullen seems to have particularly referred to the epidemics recorded by Zimmermann and others; epidemics compounded of true dysentery with "a putrid fever." (Zimmermann, p. 10.)* In these and in similar cases, of which a very great number are on record, there is no doubt that dysentery was propagated from one person to another. The consideration of this point constitutes the chief and most important part of Dr. Harty's book, and we shall make no apology for considering it as fully as our limits will allow.

Dr. Harty starts with two propositions: 1st. That the combination above referred to really exists. 2d. That this combination alone is contagious. Our space will not allow us to give the evidence in detail, but the first proposition will, we think, meet with general assent.† The second is more doubtful, but we may anticipate our conclusions by saying, that in the great majority of cases in which there is clear evidence that the dysentery was contagious, there has been an accompanying low fever of some kind or other. Exceptions to this statement, which threw a doubt on its accuracy, are, however, brought forward by some late writers, particularly by Dr. Copland, who states, in his article on Dysentery, that under unfavorable circumstances among the dark races, "a putrid dysentery spreading rapidly through all breathing the impure air is developed, instead of an infectious typhus, which would be the result in the European constitution."

The belief that dysentery owes its contagious character to an accompanying fever is not only not a new one, but many of the best writers have taken great pains to enforce this point. We have already quoted Hoffman's opinion in the earlier part of our article. Etmuller‡ is no less explicit; he distinguishes dysentery into the benign and the malignant. "The benign form," he says, "is without fever, and also without contagion, and only sporadic; but the malignant dysentery is, for the most part, joined with a malignant and sometimes pestilential fever, and increases by contagion." This fever, he adds, is sometimes petechial. Zimmermann says, "Our dysentery was of itself not contagious" (p. 18.); but he after-

* The other chief authors to whom Cullen refers are Degner, Roederer, Grimm, Helwitch, Bontius, Huxham, and Cleghorn.

† Those of our readers who wish for more evidence on this preliminary position, and on the proofs of contagion, than our limits will allow, will find a short and well-chosen summary in the article Dysenterie, in the Dict. des Sciences Médicales, by Fournier and Vaidy.

‡ Opera Omnia, tom. ii, p. 149. Gen. 1697.

wards adds, "without possessing of itself any real malignity, still a dysentery becomes truly pestilential in foul and crowded hospitals, and of consequence much the more infectious." (p. 149.)* Bontius,† in his admirable definition of "True Dysentery," does not mention contagion, while in his account of the epidemic dysentery which arose when they were beseiged in Batavia by the people of Java, and which was attended by a petechial fever, he expressly mentions that it was contagious. It is needless to multiply quotations on this point; we shall merely add that several recent authors, particularly Vignes, have derived, from personal witness of the contagious form of dysentery, opinions almost identical.

The contagion of dysentery, therefore, is of that kind which has been lately termed "contingent," that is, not belonging to the disease itself, but impressed upon it by the presence of another disease, which at all times possesses the power of propagation. The fact that a disease may thus acquire a temporary power of propagation through the animal system appears undoubted; its explanation is a much more difficult point, and one which at present cannot be completely effected. It would be unwise in the present article to enter into a consideration of this subject, which would lead us at once into the whole question of contagion, considered with reference to continued fevers.‡

We pass on to a consideration of the nature of the fevers which, as far as can be determined from the accounts of writers, have accompanied the contagious form of dysentery. It would have pleased us better if Dr. Harty had used the term "contagious fever" instead of "typhus." The word typhus is employed in different senses by writers; some restrict it to a fever springing only from a specific contagion; others extend its application to any contagious fever, without regard to its origin. Dr. Harty seems to us to use it in both senses, and somewhat to obscure his meaning in consequence. Thus using the word in its restricted signification, he excludes certain epidemics from the list of contagious dysentery, because it is evident that in these cases the combined fever was of malarious origin. We allude more particularly to the epidemics recorded by Bontius and Clarke. (Harty, p. 109-18.) Again, when no doubt can be felt that a particular epidemic was really contagious, he asserts, unconditionally, that the attendant fever was typhus, and therefore, according to his notion, always arising from a single contagious virus; whereas, in certain instances, there is strong reason to believe that the fever, even when continued, was of malarious origin. As an example, we will select our author's examination of Roederer's account of the Morbus Mucosus, into which he enters at considerable length. We must, however, premise that we have no intention of entering into the previous question, as to

* In another place, however (pp. 133-4), Zimmermann, in reference to the distinction made between non-febrile and non-contagious, and febrile and contagious dysentery, remarks that "it has a dangerous tendency," but this opinion is evidently at variance with the whole bearing of the rest of his argument. On this point we are glad to find we agree entirely with Dr. Harty.

† Bontius, De Medicina Indorum, lib. iv, pp. 24-38. Paris, 1666.

‡ The words with which Roederer commences his work on the morbus mucosus are unfortunately too true, even at the present day: "Miræ sunt naturæ operationes, abdita atria, immensus rerum campus, limitata intellecta non emetiendus. Facile tamen persuademus omnia ad certas et perpetuas fieri leges, nihilque in tota rerum natura casu quodam fortuito et citra suas causes contingere. Sed quantillum in hoc amplissimo spatio ex hausit cognitio humana. Quot adhuc altissimis tenebris oblata, sibi soli intellecta natura nobis occultat. Quantus morborum numerus, quorum causæ, proxima æque et remota, abditæ in tanta rerum caligine procul dubio nos semper latebunt." (page 1.)

whether a contagious virus does arise during the prevalence of a malarious fever. We have already stated that the examination into this point is not our present purpose; we merely select for examination a particular epidemic which Dr. Harty admits to have been contagious.

The "morbus mucosus" which ravaged Gottingen in 1761, was a peculiar disease, allied, says Roederer, both to dysentery and to intermittent fever.* Various circumstances show, he says (p. 31), dysentery to be the offspring of intermittent, and as the dysenteric fever is the daughter of intermittent fever, so the subsequent epidemic morbus mucosus is an offshoot from this; and in subsequent pages he enlarges on the analogy both of dysentery and of the morbus mucosus to marsh fevers generally. Roederer asserts that both the dysentery and the morbus mucosus were highly contagious, and he does not limit this contagious property to one form of the disease but extends it to all. And it is evident that the morbus mucosus was allied to every grade of fever from a comparatively mild intermittent up to a most severe form, which he styles "febris mucosa, acuta, maligna, biliosa, putrida, soporosa." (p. 100.) A chapter is also devoted to its complication with scurvy. (p. 37.) Dr. Harty has taken so far the same view:

"The simple inference I draw from the rather complicated details of Roederer's work is this, that epidemic dysentery set in at the usual season; that this disease lapsed into the morbus mucosus, attended with every variety of fever, remittent, inflammatory, and malignant; that the dysentery affected chiefly the large intestines, while the other was an affection of the mucous follicles of the stomach and small intestines; and that both were highly contagious when in combination with typhus or malignant fever." (pp. 94-5.)

Now we cannot find that Roederer ever suspected that only one form, viz. that attended by the highest degree of malignant fever, was contagious. The concluding sentence is indeed Dr. Harty's own interpretation; it may be a correct one, that is not our point, it was not Roederer's opinion.

With regard more particularly to the dysentery, Roederer describes carefully two forms (pp. 5-8); the one slighter, with evening exacerbations; the other very severe, with bloody stools, dry fauces, red, rough, and dry tongue, or if moist, covered with a white and purulent mucus; frequent, small, feeble, impeded pulse; nausea, vomiting, and, at last, tympanitis; urine scanty, red or yellow, without cloud or sediment. In

* Roederer's description of the post-mortem appearances of this malarious and febrile dysentery agrees to some extent with Rokitansky's: "Intestina multum inflammata passim, gangrænosa, et quo ventriculo erant remotiora, eo majori in gradu depravata. Tunica villoso tenuium, veluti arte anatomica injecta, vasculis pictis distincta, copiosissimis punctis parvisque areolis nigris conspersa. Superficies interna crassorum lacera, inequalis, *igne quasi combusta*, obscure rubra quia nigricans."

We shall also insert his account of the large intestine in the morbus mucosus, as given in the dissections at the end of the volume (p. 255). In the 1st case, the intestine was gangrenous; in the 2d, not inflamed, but contracted; in the 3d, the intestine being macerated in water for some days and the blood washed off, the lining membrane was found rough, with many little elevations or papillæ, irregular or wartlike, quasi verrucosis." In the 4th case, slightly inflamed "inflammata;" in the 5th case, numerous "copiosissimi folliculi coagmentati" were found in the ileum, cæcum, and ascending colon. They had black orifices, and were both aggregated and distinct. In the 6th dissection, the mucous follicles were copious, large, distended (repleti), surrounded by a red circle. In the 7th and 9th dissection he mentions merely the abundance of the worms (Trichinodes); the same also in the 8th case, but also "in the cæcum and appendix vermiformis the follicles were seen with black points." In the 10th, the tunica was without notable inflammation. In the 11th case, the ascending colon was gangrenous. In the 12th, "inflamed." In this morbus mucosus the small intestines were much more diseased than the large. There was a great formation of mucus and an abundance of small worms, resembling ascarides; lumbrici were also common.

the worst forms there were black gangrenous maculæ, and other signs of a most malignant fever. In speaking of contagion, no distinction is made between these two forms.

Another epidemic of decidedly malarious origin was that described by Morton, and to which we have already alluded. That Morton believed this to be contagious is certain, for he tells us, that in the month of August he was himself attacked after incautiously inspecting the stools of a dysenteric patient, and in the following autumn he was again attacked from the same cause.

With regard to the "putrid dysentery," witnessed by Clarke,* in Bengal, and which was evidently a combination of dysentery with the common Pucca or endemic remittent fever, Dr. Harty decides that Clarke's positive assertion, that the disease was contagious, must have been erroneous. The chief reason which induced him to form this opinion is, that Clarke held heterodox notions about fevers generally, believing even intermittents to be *slightly contagious*, and asserting that malignant remittents became continued. Therefore Dr. Harty concludes "that the person who entertained such opinions could have no hesitation in pronouncing the remittent form of dysentery to be always contagious. Having thus disposed of his testimony, we proceed to others of a less equivocal character." (p. 120.) We are afraid that if Clarke had written "typhus" instead of "remittent" fever, his testimony would have been dealt with more mercifully. Other epidemics, however, have been manifestly attended with a fever unequivocally belonging to the "typhus family." We need only mention those described by Sennertus, Zimmermann, Grimm, Rouppe, and, in our own times, Vignes and Dewar. We conclude, therefore, that contagious dysentery, prevailing epidemically, has been accompanied always by some kind of contagious fever. But if we are to receive the accounts of highly respectable writers as sufficient evidence, there is reason to believe that this contagious fever was occasionally of malarious origin. In several places, indeed, Dr. Harty appears to adopt this view; thus he says, in reference to Morton's epidemic: "We may fairly conclude that its contagious property arose under similar circumstances, that is, by the frequent conversions of bad remittents into continued fevers of a contagious type, a species of conversion, as we have just seen, strongly insisted on by Clarke." (p. 121.)† And yet in the previous page he has strongly condemned Clarke for holding this very opinion. In several other places Dr. Harty admits unconditionally that a fever arising from malaria, and consequently of remittent type, in the first instance, may become contagious. "That fever may, as we have repeatedly proved, be either originally a malignant typhus, or it may have been a *remittent* of bad character, converted into one of a continued contagious type. Such a change, though doubted by Bancroft, is too well authenticated to be called seriously into question." (p. 154.) This is quite in accordance with our views, but we object to the term typhus being applied to a fever of malarious origin, and which does not present the anatomical signs of true typhus.

It is worthy of notice, that when this contagious fever has apparently arisen from malaria, there have been present symptoms of scurvy in many cases, showing the general prevalence of causes exerting a manifest de-

* Observations on the Diseases which prevail in Long Voyages to Hot Countries, p. 318. 2d edition. London, 1792.

† Diseases of Hot Climates, 1809

teriorating effect on the nutritive processes. And sometimes symptoms have occurred which seem to show that there were tendencies towards other blood-diseases, for example, sweating sickness and a species of cholera. Several instances of this occur in the works of Roederer, Grimm, and others.

Dr. Copland's opinions are somewhat different from those we have now stated:*

"Many writers," he says, "conceive that the asthenic varieties described above are complications of simple dysentery with different kinds of fever, and that when they are infectious it is not the dysentery, but the fever which possesses this property. Some authors suppose that the typhoid variety especially is a complication of this description; but if such be the case, wherefore should the disorder which is communicated be always dysentery and not fever? Moreover, this form of dysentery is often present where a case of typhus cannot be found." (p. 705.)

With regard to the first statement, that the disorder communicated is always dysentery and not fever, Dr. Harty opposes the observation of Zimmermann, who remarks: "When hospitals are filled with dysenteric people, some of the assistants are attacked only with the dysentery, and others with the gaol or hospital fever, that ends in bloody and gangrenous stools." (p. 151.)

Dr. Copland's second argument, that the low or so-called typhoid dysentery is often present without any accompanying typhus, is thus answered by Dr. Harty:

"For this statement no authority is given, such as might be investigated; but is not Dr. Copland well aware that, under favorable circumstances (such as those recapitulated by Frank and others), there is no disease more readily superinduced or spontaneously generated than typhus, a position fully supported by the several authorities already quoted in illustration of this very typhoid variety." (p. 139.)

We have so much respect for any statement made by Dr. Copland respecting dysentery, that we have thus placed his opinion in opposition to the almost unanimous evidence on the other side of the question. We believe that this opinion arises in part, at least, from Dr. Copland's view of the contagion of typhus, which he does not allow to be generated so easily by the concurrence of certain circumstances as Dr. Harty supposes. But we confess that we feel little doubt that on this point Dr. Copland is mistaken, and that Dr. Harty's position, with perhaps the modification we have ventured to introduce, is the correct one.

The question whether the asthenic varieties of dysentery are produced by the concurrent action of a febrile disease, is answered in the negative by Dr. Copland:

"The fact is incontrovertible, that the asthenic forms, some of which are considered as complications by many writers, are direct and necessary and uniform results of certain diversified but concurrent causes, and not contingent associations of two diseases capable of separate existences." (p. 705.)

These concurrent causes are stated to be "cold and moisture, malaria, unwholesome food and water, or emanations contaminating the fluids."

We are convinced that this is the true mode of viewing the asthenic dysentery, viz. in reference to the causes which produce it. "It sufficently appears," says Van Swieten, "that the dysentery is multiplex as the causes whence it proceeds" (tom. iii, p. 193); and he proceeds to add, that

* Dictionary of Practical Medicine, art. Dysentery.

when the powers of life are prostrated in the commencement of the disease, this is not from exhaustion on account of the quantity of excreted matter, but from some poisonous miasma. Admitting that these causes do really produce a low form of dysentery, without concomitant fever, we can readily believe that occasionally, and indeed, often, fevers are present, because these causes are also active in their production. Sydenham's expression may with equal justice be applied here, and the asthenic dysentery be called a "febris introversa."

Doubts still exist whether the contagion of dysentery is derived from the stools only, or from exhalations given off indifferently from the lungs, skin, &c., as is presumed to occur in typhus or the contagious exanthemata. Dr. Harty, of course, adopts this last view, although he does not deny that the contagious virus may be also developed from the excretions. These certainly seem in some cases to be the "foyers" of the contagious effluvia. Thus Pringle relates, in the epidemic dysentery which prevailed after the battle of Dettingen, and which arose apparently from sudden changes of temperature and great exposure of the troops, and afterwards became contagious from development of typhus or hospital fever, that when they left the village which had been converted into an hospital, and which was filled with the dirty straw covered with the dysenteric excretions, the disease ceased immediately to be propagated by contagion. Most of the older writers believed the effluvia from the evacuations to be contagious. Fabricius Hildanus, in his work, 'De Dysenterica,' states this very strongly; so also Sennertus, who believed also that the breath of a dysenteric patient could communicate the disease. Vignes remarks: "We have seen many sick contract the disease in going to the common places where the dysenterics render their stools." (p. 205.)

Chomel holds the same opinion. (Diction. de Médecine, tom. x, p. 129.)

It must be remembered that it is only in the dysentery with malignant fever that the stools have been declared capable of communicating the disease. In common sporadic cases this never occurs, as is proved by daily observation. It is true that Lind, in his first paper on "Infection," tells an extraordinary story of a patient with chronic flux infecting almost all who used the same necessary with him, but this is a very doubtful case, and some few others, of a similar nature, which are recorded, are equally apocryphal.*

3. *Dysentery with other abdominal diseases.* We have entered at such length into the previous complications with fevers, that we must shorten our remarks on the combined abdominal diseases. A connexion between dysentery and certain diseases of the liver has been long taught by writers, although the exact steps of the connexion, and even the relative bearing to each other of the two diseases, are still matters of doubt. Dr. Harty, rather to our surprise, passes the whole subject without notice. Dr. Morehead notices as complications: 1st, with peritonitis; 2d, with morbid lesions of the small intestines, which, we are told, "are not very common, but when it does occur, is generally found at the termination of the ileum,

* At p. 260 Dr. Harty quotes Hildæus Podoanus: "Eo malo, inquit, sæpius videas corrupi, quibus clyster infunditur, instrumento non bene abluto quo antea dysentericus usus fuit; in sedili etiam seu loco excretionis contagii vestigia aliquando remanent." "The analogy of hospital gangrene," observes Dr. Harty, "in which both a general and local infectious power exists, would make me hesitate in refusing to admit a similar power in dysentery." (p. 260.)

and consists in ulceration of Peyer's glands; there is also sometimes a granular lymphy secretion." (Op. cit. p. 146.) 3d. With abscess in the liver, which is most common in the form of the disease leading to circular follicular ulcers. (p. 143.) On this subject Dr. Morehead remarks:

"Complication with abscess in the liver constitutes a great proportion of the fatal cases of chronic dysentery. In one set of cases, those ranged by me under the head of dysentery, the affection of the bowels was the primary and prominent disease; that of the liver coming on more or less obscurely as a secondary event. In the other set, the symptoms of hepatitis were the primary and most prominent, those of ulceration ef the bowels succeeding, and being not very clearly indicated." (Op. cit. p. 149.)*

Of 30 fatal cases of dysentery, recorded in the same number of the Transactions by Dr. Morehead, we find twelve to have been attended by hepatic abscess. In contrast with this statement, we may put the following remark of Dr. Baly:

"Among the many hundreds of cases of dysentery which have occurred in the Milbank Prison, during the last seven years, not one has been complicated with hepatic abscess. The medical records of the establishment too, which reach back to the year 1824, affords no grounds for even a suspicion that such cases ever occurred among the prisoners." (p. 16.)

Dr. Baly explains this discrepancy of statement by supposing that "the malaria causing the dysentery, has at sometimes, and in some places, the property of predisposing to abscess of the liver, and at other times, and in other places has not this property." (Op. cit. p. 17.) He also attributes considerable influence to the effect of simple and spare diet in counteracting the tendencies towards hepatic disease. Into a full consideration of this important question, it is impossible now to enter. A superficial inquiry would be worse than useless, but we believe, that did our space allow us, we could explain some of the difficulties of the inquiry.† We will at this time merely remark, that too much attention seems to have been given to abscess merely of the liver, without reference to other diseased conditions of the organ, such as simple congestion, enlargement, granulation, &c. Those who contend that secondary hepatic abscess is owing to absorbtion of pus, or of some hurtful matter from an ulcerated mucous membrane, or to a true phlebitis, seem to us too narrow in their views of dysentery and liver diseases. They have confined themselves to one phenomenon, and have disregarded other necessary conditions of the question.

That abscess of the liver occasionally, and indeed often arises from the causes now specified, viz. phlebitis and purulent absorbtion, is an undoubted

* With reference to the cicatrization of colonic ulcers with hepatic abscess, Dr. Morehead says: "It is a satisfactory fact, that cicatrization of ulcers of the mucous coat of the intestine will go on under circumstances which *a priori* would be considered most adverse, e. g. under the coexistence of abscess of the liver." This agrees with the observations of Parkes and others.

† The experience of dysentery of the hospital ship "Minden," during the Chinese war, as detailed by Dr. Wilson, has been considered decisive as to the non-existence of hepatic abscess with that form of dysentery. But on this point it is necessary to wait for more evidence. It is premature to consider the peculiar and multiform diseases occurring during a campaign as proper cases for inquiries of this kind. Sir James M'Grigor has, with his accustomed liberality, permitted us to look over the admirable Reports sent home from China. It is evident from these, that during the campaign, the troops suffered from the several well-marked forms of malarious dysentery, either alone or combined with scurvy. Since they have been in barracks and cantonments, the Chinese dysentery is stated by several gentlemen to be assuming gradually the forms of common Indian dysentery, and to be complicated with abscess and other diseases of the liver. But the subject is too important to be discussed here.

fact—that they will account for all cases of abscess we cannot admit. Even Cruveilhier, who may be considered as one of the originators of this doctrine, never carried it to such an exclusive pitch as some of his followers have done. (See Livraison xl, p. 5.)

We cannot close our examination of this subject without noticing some extraordinary nervous symptoms observed by Dr. Baly at Milbank. Similar symptoms prevailed in the Penitentiary in 1823, and are detailed by Dr. Latham. These were at that period chiefly, "headache, vertigo, cramps, and twitchings of the limbs, delirium, convulsions, and apoplexy." Dr. Baly has noticed cramps, chiefly of the extremities, and diminution of sensation in several cases; in other instances, there has been stupor and pseudo-hysterical symptoms. In one case there was a kind of modified cataleptic ecstacy. A man, aged 22, was attacked with profuse serous diarrhœa, possibly that modification of malarious dysentery to which we have already alluded. Bleeding, calomel, and opium proving inefficacious, metallic astringents, (sulphates of copper and zinc, we presume,) with opium, were employed.

"The new remedies immediately arrested the diarrhœa, but now a fresh train of symptoms presented themselves. The patient's mind seemed no longer to take cognizance of the impressions made on his senses. He sat up in bed frequently repeating half aloud a sentence consisting of two or three words. His eyes were open, but he did not seem to observe surrounding objects; when questions were put to him, he did not answer, or only repeated the last word or two of the question. When his chin was depressed, he mechanically protruded his tongue and kept it protruded until his lower jaw was raised again; he then withdrew his tongue and allowed his mouth to be closed. When his arm was raised, he kept it in the position given to it, till it was returned to its former place by another person. Food and medicine he swallowed when they were put into his mouth, but never expressed repugnance for the one or desire for the other. He appeared conscious of no suffering; his skin was cool, his tongue moist, pale, and nearly clean, and his pulse slow and rather full. In this state he continued three days; the nervous symptoms then passed off, and merely slight diarrhœa remained." (pp. 22-3.)

Dr. Baly remarks, that these nervous symptoms were merely "functional," that is, unattended by appreciable organic changes after death; and he ascribes their prevalence at Milbank to the general increased excitability of the nervous system, induced by long confinement. Prisoners, who have been long in prison, and are then suddenly transferred to the convict ships in the river, and thrown at once from perfect order and repose, into scenes of excitement, are occasionally subject to violent epileptiform convulsions. The same excitability which renders them liable to these attacks, may render them more liable to excito-motory actions when attacked by diarrhœa or dysentery in prison.

But we must bring our remarks to a conclusion, and regret that our limited space allows us to go into no detail on the many interesting subjects we have been compelled so cursorily to review. The symptoms and treatment of dysentery, detailed by Drs. Baly and Morehead, although highly important, present nothing unusual, or that call for special notice. Dr. Harty devotes 100 pages to the treatment of dysentery, and gives us a valuable account of the method adopted by many of the best writers. But those who have time to refer to this subject will do well to consult the work itself.

ART. IV.

1. *Saggi di Clinica Medica, basati specialmente sopra le Osservazioni della Clinica del* Professor MAURIZIO BUFALINI. Compilati dal Dottor F. BINI, Ajuto alla Clinica Medica della Scuola di Complemento e Perfezionamento nell' I. E. R. Archispedale di S. Maria Nuova di Firenze, e dall Dottor GHINOZZI. Vol. I, Fasc 1, 2, 3, 4, e 5 ; Vol. II, Fasc 1 e 2.—*Firenze,* 1843-5.

Essays on Clinical Medicine, founded especially on Observations of the Clinique of Professor M. BUFALINI. Compiled by Dr. F. BINI, Clinical Assistant of the School of Completion in the large Hospital of Florence, and by Dr. C. GHINOZZI.—*Florence,* 1843-5.

2. *La Clinica Medica del* Professore G. A. GIACOMINI. Esposizione compendiata del Dottore G. B. MUGNA.—*Padova,* 1836.

A Short Dissertation on the Clinical Medicine of Professor G. A. GIACOMINI. By Dr. G. B. MUGNA.—*Padua,* 1836. 8vo, pp. 147.

WE have placed the above works together with the view of contrasting the practical results of two opposite systems of medicine, both tested in the clinical wards of large Italian hospitals, under two physicians who are regarded by their fellow-countrymen as the rival leaders of what may be termed the vital and the organic schools, assisted by students and junior physicians, to whom we are indebted for this record of their observations.

We reserve for another opportunity any examination of the theories or doctrines of Bufalini and Giacomini, but may now state that the former is a principal opponent of the Brownian doctrine of excitability which obtained such favour in Italy, and a supporter of the theory of the local origin of all diseases ; all constitutional changes resulting from material or organic alteration in some part to which it is necessary to direct the treatment. Giacomini also is an opponent of Brown, but on different grounds ; regarding most diseases as an increase of vital activity generally manifested under the type of inflammation. He combats the humoral pathology, and his commentator Mugna perfectly agrees with his views as to the inflammatory origin of disease, regarding delirium tremens as encephalitis, neuralgia as *neurilemitis,* tubercular phthisis as nothing more than chronic pneumonia, fevers as gastro-enteritis or arteritis ; hectic fever, rheumatism, gout, chlorosis are all chronic arteritis ; intermittent fever is intermittent subacute arteritis, and the constitutional irritation which follows severe operations is a diffusion of the inflammation from the divided ends of the vessels. His practice is in perfect accordance with his theory, bloodletting being his grand panacea for all human evils. The quantity of blood drawn in his wards is never less than eight ounces in the adult, more frequently fourteen or sixteen ounces. Very low diet was the general rule, barley-water or lemonade serving as ordinary beverages. On looking over the table of the cases and their treatment, which came under observation from 1830 to 1834, we were surprised at the extraordinary number of patients bled. 650 were treated, and after taking the trouble to add up the number stated to have been bled, we find this to be 504. We also added up the number in which the bleeding was repeated, and from the result it appears that 257 were bled once, 132 twice,

65 three times, 24 four times, 9 five times, 10 six times, 2 seven times, 4 eight times, 1 ten times. Besides this, leeches, calomel, tartar-emetic, and purgatives appear to have been employed with heroic vigour. Let us take a case, for example, of acute pneumonia in a labouring man, aged 33, of sanguineous temperament and strong constitution.

"The difficulty of respiration carried to the state of orthopnœa, dull deep-seated pain over the whole thorax, speech interrupted by great anxiety, mucous rattle, which never ceased, because the cough could not expel the mucus from the air-passages; dry and tremulous tongue, meteorism of the abdomen, livid countenance, eyes fixed and prominent, and the conjunctiva injected; pulse very rapid and small, skin dry and arid; all these were phenomena which announced the case to be almost desperate.

"Bleeding to 15 ounces was practised, and 2 drachms of the cherry laurel-water given. On the second day no improvement, but the orthopnœa was more severe, mental aberration, carpologia. Bleeding to a pound, and the same dose of cherry laurel-water. He died on the evening of the third day." (Mugna, p. 42.)

Injection, adhesion, and effusion of the pleuræ were discovered; hepatization of one lung and gangrene of the other. We may learn from this case, and remarks scattered through the volume, that none of the symptoms usually regarded here as expressly contradicting bleeding, were so interpreted by Giacomini. Indeed we are told that—

"Whether inflammation be acute or chronic; if suppuration have or have not taken place; if it be accompanied by marasmus, or even if it be about to extinguish life, it is still always the same, and requires the same means to subdue it; that expectoration in pulmonary diseases can never be assisted by stimulant remedies, but may always be promoted even in the last extremity by bleeding and antiphlogistics; that nutrition is not supported in the sick by so-called nutriments, but by the means which check the disease which causes the imperfect nutrition." (Mugna, p. 129.)

We have seen from the numbers bled how these views were carried out in practice, and our readers will doubtless look with interest for the statistical results. These are given in the annexed table:

Number of patients treated in the clinical ward	650
Total number discharged	617
Total number cured	585
Number discharged into other wards, not perfectly cured	22
Number of deaths	33
Number of slight diseases, or those easily curable	204
Number of severe and dangerous cases difficult of cure, incurable excluded	409
*Number of absolutely incurable diseases	15
Mortality per cent. on the total number treated	$5\frac{1}{13}$
Mortality per cent., those cases excluded which did not complete the cure in the clinical ward	$5\frac{1}{4}$
Mortality per cent. upon the severe and dangerous diseases alone, the slight cases and those easily curable being excluded; the incurables also excluded	$4\frac{1}{5}$
Mortality per cent. upon both severe and slight diseases, incurables excluded	$2\frac{14}{15}$

We regret that no similar table is appended to the work of Bufalini, in order that we might compare them together. Neither can we gather from the work any account of his treatment, except in the essays on lead colic, delirium tremens, chorea, and fevers, written by Drs. Bini and Ghinozzi, which fill up the text; but we know from the other writings of their master, and from having followed his hospital visits at Florence, that he practises

* 13 of these died on the day of admission or the day following; the two others were cases of hydrophobia.

a cautious and rational empiricism much opposed to the indiscriminate bloodletting of his rival. His pathological views also render him much more strict in his diagnosis and logical in his deductions from symptoms. This is especially seen in the care with which thoracic diseases are discriminated, while Mugna never refers to physical diagnosis, apparently establishing the character of these diseases by general symptoms alone. The total number of cases treated in the clinique of Bufalini in seven years was 1026. Of these 102 died, 732 were cured, and 192 are described as "incomplete result," that is to say, either imperfectly cured or discharged into other wards. The numbers of slight and serious cases are not given, but however considered, it appears that the per centage of deaths is much greater, and that of cures much less than in the practice of Giacomini. Probably this may be partly explained by the latter, as we should judge from the table of his cases, having selected acute diseases as a general rule for his clinical wards; Bufalini those of a chronic character, with a greater regard to their diagnosis, as illustrated by their pathology. We must also in fairness add, that much looseness appears in the manner in which diseases are stated to be *cured* in the tables of his rival. Thus we see a case of ascites cured in 10 days, cases of rheumatic fever in 3 and 4 days, peritonitis in 2 days, quartan intermittent in 6 days, hæmoptysis in 9 days, hepatitis in 6 days, and so on. Now this alone is enough to make one sceptical, when called upon to admit the extraordinary success of treatment; at the very best, if the diagnosis of the above cases was correct the result could only have been what Bufalini would style "incomplete."

We fear that we cannot carry the contrast between these teachers much farther, because each selecting his own cases for the clinique, would naturally prefer examples bearing out his own ideas. Thus, in the tables of Giacomini, a large proportion of cases are seen to be inflammatory, aud his depleting system appears to have worked well, whereas in those of Bufalini, organic diseases occur in a much greater number. Without any common starting-point, therefore, we cannot form a correct opinion of relative success. To do this it would be necessary to obtain information as to the severity of each case; the intensity and duration of the disease before and after the admission of the patient; the various complications of the disease, and the treatment before and after admission. Then, again, varieties of climate, manners, and customs, diet, and social position of the patients must be considered. This is a kind of information which it is impossible to afford in a statistical table, and that which constitutes the real difficulty in the study of medical statistics.

In another paper we have alluded to the successful treatment of pneumonia on the homœopathic system, or perhaps we may say, have there shown that the natural progress of this disease in the majority of cases is towards recovery.

We should have made a calculation of Giacomini's success with his bleedings, but as it does not appear that physical diagnosis is much practised in his wards, we might be reasoning on an unsound basis, but it may be interesting to show the result of Bufalini's more mild system—142 cases of pneumonia were treated by him. Of these the result gives 113 cures, 18 deaths, 11 incomplete result. In 82 of these cases the disease was complicated by bronchitis or pleuritis, or gastric disturbance. Of the

cases of deaths, many were received when the disease was in a very advanced stage, having run from six to fourteen days without treatment. The treatment is not described, but appears to have been mildly antiphlogistic and expectant.

Of 29 cases of pleuritic effusion there resulted 19 cures, 7 deaths, 3 incomplete result. In 5 of these cases paracentesis was performed, and of these 5, two died. In most of the cases the effusion was very large, filling the whole pleural cavity. The right side was affected in 9 cases, the left in 20.

We do not find other matters of interest in either of the tables before us. The text of Dr. Mugna's work is taken up by short dissertations on inflammations of the brain and nervous system, and of the organs of the chest and abdomen. They are far below mediocrity. The essays of Dr. Ghinozzi on delirium tremens and tetanus, with an unfinished essay on fevers; and of Dr. Bini on lead colic and chorea, although creditable as theses, contain nothing worthy of translation or analysis, nor are they by any means to be considered as *clinical*.

It may be well, before concluding, to give a short account of the clinical system of the Italian Hospitals, which are, generally, very large establishments, containing from one to three thousand beds. There are four clinical wards, for the male and female medical and surgical cases. Each of these is under the direction of a clinical professor, who takes his turn of duty for a year or session. The students are obliged to attend punctually at the morning and evening visits. Each patient is consigned to one of the students, who, before the visit, obtains from the patient or his friends his name, country, condition, age, progress of the disease, stage of the disease, exciting causes, treatment that has been adopted, and present symptoms. This is all written upon paper ruled for the purpose, and when the professor or his assistant visits the patient, the student reads it, and explains the reasons of his diagnosis. Afterwards, every morning and evening, he registers the most important changes in symptoms and treatment. Before the patient leaves the hospital, the student reads his account of the case to the professor, and it is then deposited in the records of the clinical ward. If the patient dies, before the body is opened, the student reads the history of the case with the diagnosis, and remarks of the professor; and then, other students examining the various organs, he writes an exact account of what is observed, and remarks upon the points in confirmation of, or in opposition to, the diagnosis made during life. If any student refuses to attend the patient consigned to him, or does not keep the history of the case, and consign it to the clinical archives, he is not admitted to scholastic examination; while assiduity and exactness are favorably considered at his examinations, and in the distribution of honours and profit.

This system is, in the main points, similar to that introduced in Dublin from Germany, with such marked success by Drs. Graves and Stokes, and is admirably adapted for training up a school of diligent practical observers. Its general adoption here is the only mode of checking the grinding system, and transforming the student into the man of experience.

Art. V.

On Cataract, Artificial Pupil, and Strabismus. By F. H. Brett, Esq., M.D., F.R.C.S.—*London,* 1847. 8vo, pp. 89. With four Plates.

The subjects discussed in this little work, are Cataract, Artificial Pupil, the Cure of Squinting by Operation, and the Phenomena displayed by Congenitally Blind Persons restored to Sight.

The author sets out by saying, that Celsus informs us, that among the physicians of Alexandria, there were many who practised the removal of cataract by operation, "especially one named Philoxene." Why Frenchify the name? Why not say Philoxenus? Perhaps Dr. Brett follows some general principle on this head, for we find he transmutes Mr. De la Garde, of Exeter, into Monsieur De la Garde; and even our worthy friend, Dr. John A. Robertson, of Edinburgh, appears as M. Robertson. After all, the whole of this Frenchification may be intended merely to prepare the reader for the coming announcement, that Dr. Brett has been in France, witnessing operations by M. Velpeau. But what is more important, we cannot agree with Dr. Brett's character of Philoxenus. He may have operated for cataract, but Celsus does not say so. Celsus merely states, that, being separated from the other parts of medicine, surgery began to have its peculiar professors, and received considerable improvements in Egypt, principally from Philoxenus, who treated of this part, (i. e. surgery), fully, and with great accuracy, in several volumes.

Besides having been in France, witnessing operations by M. Velpeau, Dr. Brett informs us, that he has been "in the vast peninsula of India; not only in Calcutta, but from thence to Lahore, Delhi, and other populous cities of the East." Need we wonder, when we are told, in true Oriental style, that he has "restored many thousands to sight?" And yet he joins M. Velpeau, in laughing at poor Lusardi, whose successful cases of couching amount to no more than 4168, out of 5034! What is this to "many thousands?"

We are next introduced by Dr. Brett to some "Anatomical Considerations;" the most remarkable of which is, that the figure of the crystalline "is that of a double convex lens, more convex anteriorly than posteriorly." We have always found it the reverse.

When objects appear multiplied to a cataractous eye, Dr. Brett thinks this may be attributed to opaque radii, passing from the centre to the circumference of the lens, which "act as the surfaces of a kaleidoscope;" —a similitude than which nothing could be more pat, provided we could only once understand how opaque radii are to act as surfaces.

Dr. Brett believes in the efficacy of medical treatment in cataract. This is rather ticklish ground to tread on, but as his remarks on the subject seem judicious, we think it right to let him speak for himself. We may just mention, as rather confirmatory of his views, that we have met with several cases, in which, on examining a suspected eye catoptrically, either no inverted image was discovered, or it was seen very obscurely; yet, on treatment being had recourse to, such as Dr. Brett recommends, vision has greatly improved, and the inverted image has appeared distinct. No doubt, the change might depend on the removal of some obscuration of the aqueous humour, or of the anterior surface of the anterior capsule, as

well as of the lens itself, or of the posterior capsule; for it is to be borne in mind, that, in estimating the changes that occur in the appearances of the images reflected from the eye in its several diseased states, it is necessary to take into account the state of the media through which we see them, as well as that of the surfaces which form the images. But let us hear Dr. Brett:—

"When the surgeon is consulted in the earlier stages of the disease, I am far from coinciding with the too prevalent opinion, that all medical treatment is of no avail. On the contrary, I believe that, under favorable circumstances, we may often succeed, if not in removing, at all events in greatly retarding, or even arresting, the further progress of the affection. If cataract is a disease of the constitution, if it is often hereditary, it is surely a rational practice to attempt to retard the progress of the disease by constitutional treatment. In the first class, whether of a palpably or obscurely inflammatory character, I believe that derivatives, the local abstraction of blood, counter-irritation, and a mild alterative course, combined with assiduous attention to the general health and constitution of the patient, is decidedly efficacious. On the other hand, whatever imparts tone to the system, improves the general health, and increases the capillary circulation, (not omitting the warm bath, with mild alteratives and tonic aperients,) is calculated to retard the advancement of the second or senile class of cataracts. Where there is a gouty or rheumatic diathesis, or a tendency to the predominance of lithic or phosphoric depositions in the urine, such remedies and regimen as tend to correct these states will be indicated. In debilitated habits, quinine, and other tonics, nutritious diet, with wine, regular exercise, and early hours, and the shower-bath." (p. 13.)

Dr. Brett recommends extraction to be performed with an instrument of the general figure of Beer's knife, but "increasing *suddenly* from the point to the shoulder, so that the section of the cornea may be completed by a single thrust before the point of the knife reaches the nose." He accordingly represents the edge and the back of the knife forming an angle of not less than 28°. This being a topic which we formerly discussed pretty fully (No. for April, 1844, p. 337, et seq.), we shall merely observe, that as the quantity of resistance to the penetration and progress of a knife through the cornea increases with the augmented angle of inclination of its edges, with one formed, as Dr. Brett recommends, it will be always difficult, and often impossible, to complete the section of the cornea. The resistance will often make it stick fast and immovable, before the section is half executed.

In perusing Dr. Brett's sections on cataract, from p. 15 to p. 26, we were struck with the suspicion, that surely we had met with the same matter, and in the same words before. On turning over some of the general treatises on the diseases of the eye, we found Dr. Brett had made a very unwarrantable use of Dr. Mackenzie's work—as the following parallel passages will show:

Mackenzie, 3d Edition, p. 663.

"Double extraction decidedly exposes the eye to greater risk of inflammation. If we operate only on one eye, and allow it to recover, we may possibly observe, in the course of the operation and recovery, some particulars which will be essentially useful to us in conducting the second operation, or will even lead us to select a different and more suitable mode of operation for the second eye."

Brett, p. 15.

"Double extraction exposes the eye to greater risk. But by operating only on one eye at a time, we often observe, in the course of the operation and recovery, some particulars which will be essentially useful to us in conducting the second operation, or will even lead to a more suitable method of operating on the second occasion"

Ib. p. 664.

"In cases of congenital cataract, ought the operation to be delayed till the patient has attained an age sufficient to enable him to understand the importance of an attempt to restore sight, or ought it to be practised during infancy? The answer decidedly is, operate in infancy."

Ib. p. 681.

"The operation of extraction divides itself into three periods. In the *first*, the cornea is opened with the knife. In the *second*, the anterior hemisphere of the capsule is opened, or destroyed. In the *third*, the exit of the cataract, or the extraction properly so called, is accomplished."

Ib. p. 684.

"In traversing thus the anterior chamber, let the operator bend his eye steadily on the point of *counter-punctuation*; if he do so, the point of the knife will be sure to follow, whereas, if he allows himself to be diverted to anything else, for instance, to what the edge of the knife is doing, he may miss his aim, and bring out the instrument at a wrong place."

Ib. p. 16.

"In cases of congenital cataract, ought the operation to be delayed until the patient has attained an age sufficient to enable him to give his assent, or ought it to be practised during infancy? It is greatly preferable to operate in infancy."

Ib. p. 20.

"The operation may be conveniently divided into three periods. In the *first*, the incision of the cornea is effected. In the *second*, the anterior hemisphere of the capsule is opened, or rather destroyed as much as possible. In the *third*, the exit of the cataract, or the extraction, properly so called, is accomplished."

Ib. p. 21.

"In traversing the anterior chamber, the operator should be very particular in directing his attention solely to the point of *counter-punctuation*; by so doing, the point of the knife is sure to follow; whereas, if he allows himself to be diverted by any object, he will very probably miss his aim."

It was not till we had noted four other plagiarisms fully as glaring as the above, viz. the passages commencing with "On pushing the knife, &c."—"If the aqueous humour has been, &c."—"In cases of capsulo-lenticular cataract, &c."—and "The posterior capsule of the lens, &c."—in pages 21 and 25, and corresponding with passages in pages 685-7-8 of Mackenzie, that coming to page 26, where Brett proceeds to consider the accidents during or after extraction, our eye caught the following announcement: "Here the rules recommended by Dr. Mackenzie correspond so much with my own, that with but few alterations or omissions, I can scarcely do otherwise than transcribe them." The next four pages and a half, then, are taken, with some slight verbal changes, from Dr. Mackenzie, till our author comes to inflammation as a consequence to be dreaded after extraction, when we find him disapproving, in acute cases, of mercury, lest it should check the adhesion of the cornea, and in sub-acute cases, recommending nutritious diet, alcoholic stimuli, carbonate of ammonia, and opium—following on this subject the distinctions and treatment of Mr. Tyrrell. It is sufficient to show the difficulty of distinguishing, by the symptoms, Mr. Tyrrell's acute from his subacute inflammation, that this gentleman recommends the giving of a dose of carbonate of ammonia, with or without opium, as may be thought proper, in order that the case may be pronounced sub-acute, if the patient experiences relief; acute, if the symptoms are aggravated; and the future steps of the treatment directed accordingly. (See Tyrrell's Work, vol. ii, p. 428.) The truth is, that old debilitated dram-drinkers seldom do well

after any operation for cataract, and ought in general not to be meddled with. They are the subjects who suffer chiefly from inflammation after extraction; sometimes with the symptoms of Mr. Tyrrell's acute variety of the disease, sometimes with those of his sub-acute.

In making the section of the cornea, Dr. Brett advises the knife to be entered about a line from its margin, or midway between the margin of the cornea and the edge of the pupil. A line is too far from the sclerotica, so that the section will generally be too small, which is one of the most fruitful causes of inflammation.

For reclination or depression, Dr. Brett uses Scarpa's needle. Having passed it through the sclerotica, and carried it into the anterior chamber, he turns the point of the instrument backwards, and endeavours to break up the anterior capsule "near the ciliary processes, by carrying the point of the needle first in a circular direction around the periphery of the lens, and then by a crucial incision of the centre,"—a manœuvre which we believe to be impossible. The needle is held so fast by the sclerotica, that its point cannot be made to travel round the circumference of the lens. If it could, and the capsule was thus divided, no crucial incision could afterwards be made across its centre.

Dr. Brett must consider the retina as lining the whole extent of the choroid, for he tells us (p. 36), that to injure the retina is inevitable, at whatever distance we penetrate the sclerotic. Our opinion is, that the true or sentient retina ceases at the ora serrata. That some of the elements of the membrane, called retina, pass on to the ciliary processes, or even to the edge of the iris, we do not mean to deny; but we consider the wounding of the retina behind the ora serrata as likely to be injurious to vision, while puncturing it anteriorly to the ora serrata, or in other words, within $2\frac{1}{2}$ lines of the cornea, as of little or no consequence. Huschke will have it that the retina advances over the ciliary processes as far as the iris, but that there, at least in the fœtus, it does not form a free edge, but is reflected within itself, to form a second layer like a double nightcap.

In order to avoid the long ciliary artery, Dr. Brett directs us to enter the needle about a line below the transverse diameter of the eye; but as about a quarter of an inch from the iris, the long ciliary artery of the temporal side divides at an acute angle into two branches, an upper and a lower, it must be obvious that the surest way to avoid either of the branches is to enter the needle in the equator of the eye.

After describing the usual operation of division of the cataract through the cornea, our author goes on to state, that in fluid, soft, and capsular cataracts, he uses a sharp flat needle, and purposely allows the aqueous humour to escape, while he is performing a drilling movement of the needle. He says this is Mr. Tyrrell's "operation of drilling." Now Mr. Tyrrell's operation differed from this, not merely in preventing the escape of aqueous humour, as Dr. Brett mentions, but in being limited to cases of contracted pupil, adhering to an opaque capsule. For ordinary cases of soft cataract, we consider division through the sclerotica as the operation to be preferred, but were we induced to operate through the cornea, most assuredly we would follow the rule generally laid down, and particularly insisted on by Dr. Jacob, to retain, if possible, the whole of the aqueous humour. From an escape of this fluid, in division through the

cornea, and from the iris being consequently pressed forwards into contact with the cornea, we have seen severe iritis ensue, ending in closure of the pupil.

At page 53, Dr. Brett says, "Although we must admit the truth of the general doctrine of the power of adjustment depending chiefly on the shifting of the lens, and although I acknowledge its doubly-refracting structure, and that its peculiar texture and gradually increasing density as it approaches its centre enables it to correct the spherical aberration of light, yet I must think that the loss of this body is not attended with so great a degree of deficiency with respect to these powers of the organ generally, *provided* the remaining parts of the eye, and especially the transparent media, are in the highest possible state of health." This last statement we may admit to a certain extent. No one has been known who could read an ordinarily printed book without the aid of a convex glass, after the removal of the crystalline; while such instances as that of the postilion, related by Sir William Adams, confirms the belief, that an eye, if young and otherwise uninjured, may accommodate itself in a very considerable degree to the loss of the crystalline, so far as distant objects are concerned; but pray, what has the doubly-refracting structure of the lens to do in the matter? Just as much as the double refraction of Iceland spar or the height of Strasburg steeple.

There seems to be a fatality attending every attempt to eulogise Cheselden.

"If astronomy," says Morand, "assures a kind of immortality to him who by chance becomes the discoverer of a single star, the same honour may well be bestowed upon a surgeon who knew by his genius to discover the whole heaven to a man born blind." In his haste to do justice to the great English surgeon, Morand has fallen into an error: for it was not in a case of congenital closure of the pupil, as Morand here supposes, that Cheselden first performed the operation for artificial pupil, but in a case of closed pupil after an operation for cataract.

Little less awkward is Mr. Guthrie. For though Cheselden published in the 'Philosophical Transactions' for 1728, two cases, in which he had formed an artificial pupil by incision of the iris, the one above and the other below the place of the natural pupil, which circumstances, with several others, are minutely related by him, Mr. Guthrie, in his work on 'Artificial Pupil,' published in 1819, after announcing that "the idea of forming an artificial pupil owes its origin to Mr. Cheselden, previous to whose time a closed pupil was considered irremediable," an assertion not quite correct,—gravely tells us that Cheselden "does not seem to have performed the operation on the person whose history he relates," a remark egregiously absurd.

"The illustrious Cheselden," says Dr. Brett, "had the distinguished merit of showing, in the last century, that sight might be restored in cases till then considered beyond the reach of art, by a new opening formed in a closed pupil. Various improvements and modifications have been introduced by modern surgeons since that period; and by the skill and ingenuity of a host of surgeons of the German, the Swiss, the French, and the English schools, following up the one idea of that one master-mind, we are now enabled to enrol the various operations for artificial pupil amongst the most brilliant in ophthalmic surgery."

Now, in the first place, the forming of "a new opening in a closed

pupil" was no part of Cheselden's plan. This had been previously tried by Woolhouse, but without success. Cheselden's operation was a new opening in the iris, above or below the closed pupil. In the next place, to say that artificial pupil was "the one idea of that one master-mind," is a senseless oratorical flourish; for with no wish to set aside the old proverb, *Non unus omnia videt,* we may venture to affirm, that Cheselden had very many wise ideas in his head besides this, while what we have just mentioned about Woolhouse, shows that Cheselden was preceded by that oculist in attempts to restore sight in cases of closure of the pupil.

The first cases demanding the formation of an artificial pupil, noticed by Dr. Brett, he styles "occlusion of the pupil by an opacity of the cornea." This is a misnomer, as in such cases the pupil is not occluded, but merely covered. In the whole of the chapter on artificial pupil, there is an exceeding degree of negligence in the application of technical terms, and even in their spelling. Thus iridectomy, which means the formation of an artificial pupil by excision of a portion of the iris, is used as synonimous with incision; and we have coretomy and ceretomy, in place of corotomy. These are barbarous terms, but it would better become Dr. Brett to spell them correctly, than talk magniloquently of the "iridian diaphragm!"

To return to the first case requiring an artificial pupil, Dr. Brett states, that a central leucoma "is frequently complicated with a central cataract," adding that "this double obscurity of the dioptric media is explained in the following manner:—When the cornea becomes the seat of a central perforating ulceration, the aqueous humour escapes, the iris falls forwards, and the lens, in obedience to the contraction of the muscles, advances towards the cornea. The centre of the anterior part of the crystalline capsule comes in contact with, and receives the morbid products of the ulcer, inflames, and becomes opaque. When the ulcer closes, the aqueous humour again fills the two chambers, and the lens resumes its primitive position; but the corneal ulcer has been replaced by a dense white opacity, and a circumscribed opacity likewise occupies the capsule and lens itself." (p. 56.) Now, this formation of central cataract is, we believe, altogether a piece of imagination. We have never seen a case of the sort, and even Dr. Brett does not say he has ever seen one. Central cataract is not unfrequently met with in infants recovering from ophthalmia neonatorum, in whom there has been ulceration of the cornea, but without perforation; and therefore is not owing, at least in such cases, to the centre of the capsule coming into contact with the ulcer. We have also repeatedly seen the cornea perforated by ulceration, both in such infants and in other cases, but by the assiduous application of the extract of belladonna, the ulcer was brought to heal, the pupil expanded, no adhesions were left, and no central cataract.

With most authors who have written on the subject, Dr. Brett lays it down as a rule, that previous to undertaking an operation for artificial pupil, we should assure ourselves that the eye is able to distinguish between day and night. "Whatever," says he, "may be the density of the obstacle, the individual must be able to recognise so much light as to remove all doubt as to the possible complication of amaurosis." He adds, however, that "this principle admits of some exceptions," and concludes

with the following observations, with which, with one small exception, we readily agree:—"In a patient, for example, labouring under a leucoma, the pupil being free behind the corneal obscurity, the operation would, doubtless, be contra-indicated, where the power of vision had become completely abolished. But if, under the same circumstances, a false membrane also obstructed the pupil, who can say that a soft cataract may not exist behind it, sufficient, of itself, to intercept every ray of light? An operation is, therefore, allowable in these doubtful cases." We grant that cases may occur, in which it may be proper to try the effect of forming an artificial pupil, although the patient does not perceive light. They must, however, be very rare, and we entirely dissent from the statement, that the presence of a soft cataract could, of itself, suffice to render the eye insensible to light. It must be a combination of partial opacity of the cornea, closure of the pupil, adhesions of the iris to an opaque capsule, and cataract, and such a combination only, which could produce insensibility to light, the retina remaining sound.

We are sorry, that we again meet, in this chapter, with whole sentences taken without acknowledgment from Dr. Mackenzie's Practical Treatise. Compare Brett, (pp. 60, 61, 64,) with Mackenzie, (pp. 738, 744.)

In those cases, in which an artificial pupil is demanded by a dense opacity of the cornea, with adhesion of the pupillary margin to the cicatrice, Dr. Brett recommends the cornea to be punctured with a broad needle, as the first step in the operation of excision. This we consider too narrow an instrument to make a sufficient incision. Mr. Tyrrell's hook is then to be passed through the puncture, "as far as the aperture of the pupil," and "introduced through the pupillary space." But how is this to be done, if the pupillary margin of the iris is adherent to the cornea? Under such circumstances, an incision, one fifth of an inch in length, should be made close to the margin of the cornea, the iris should then be punctured close to where it adheres to the cornea, and enough of the iris should be dragged out through the wound of the cornea by the hook, or a small pair of hooked forceps, to be cut off with the scissors.

Dr. Brett speaks very unfavorably of the operation of separation. The only case in which he admits this plan of operating is, when the cornea is the seat of a dense leucoma, with a small segment of iris uncovered. Here he advises a puncture to be made in the opaque part of the cornea, and if this is required towards the inner canthus, he does it with a curved lance-shaped knife, as a straight instrument would be interfered with by the nose. Yet he represents (Pl. 1, fig. 1), the straight cataract knife dividing the cornea towards the inner canthus, as if this were quite a customary thing, whereas it must be very difficult, from the prominence of the internal angular process of the frontal bone. He then introduces through the puncture a small sharp hook, draws out a fragment of iris, and knips it off. He calls this "the only allowable modification of iridodialysis;" but it is not at all clear that it is an iridodialysis at all, he does not say that with the small sharp hook he separates any part of the iris from its adhesion to the choroid, and we rather think the operation is just a variety of excision. No greater injury will be done to the eye in this variety of excision than in any other. Perhaps, however, Dr. Brett, in explaining the want of success which is apt to attend this operation,

refers not to the injury done by the operation, but to that inflicted on the retina by the original disease which has left the eye in so unfavorable a condition. On this point, he does not express himself very perspicuously.

The figures which accompany Dr. Brett's work are none of the best, but fig. 21 it is impossible to make anything of by itself, or to recognise, as we learn from the description (p. 73), that it is intended to represent a case where the cornea is transparent, and the iris, in consequence of iritis, adherent to the anterior capsule. For such a case, Dr. Brett recommends Mr. Tyrrell's operation of drilling, only he allows the aqueous humour to escape through the puncture made in the cornea. He adds to his account of the operation, that the pupil is preserved in a state of dilatation by the belladonna ointment; but as the pupil is adherent to the capsule, the belladonna will be of little use.

The chapter on Strabismus is very imperfect, occupying no more than five pages. The author tells us, that frequent observation convinces him that the operation "is not always successful," (p. 78); but in the course of the next six lines, he adds, that according to his "experience, the operation may be performed with almost invariable success." The latter announcement may lead one to expect some new and valuable hints as to the method of cutting for squinting, but nothing of the kind occurs. Dr. Brett seizes the conjunctiva, the subconjunctival fascia, and the tendon of the muscle, with two pairs of hooked forceps, one placed near the cornea, and the other near the caruncle, raises by this means a transverse fold, and snips it through with scissors, one blade being made to penetrate the fold and pass along the sclerotica, while the other is outside the fold. We consider this manipulation to be about as ugly a one as any which the squint-clippers have invented (and these have not been few), and as much more dangerous than some of them. Instead of the scissors "grating along the hard fibrous sclerotic coat," to use the elegant phrase of Dr. Brett, we can conceive the point, which must necessarily be sharp, to go suddenly through the sclerotic and choroid, so as to evacuate the vitreous humour, and we believe that this has happened more than once, in attempts to perform the section with scissors, or by a subconjunctival incision with a narrow knife. We doubt very much, whether the forceps will often raise up the muscle from the subjacent sclerotic; even when they raise the middle of it, the upper and lower fibres will be very apt to escape. There is not the smallest advantage gained by cutting the conjunctiva, fascia, and muscle, at one stroke. On the contrary, we think it not merely less violent, as well as safer, but greatly more conducive to success, to divide these textures separately.

Dr. Brett repudiates the hook for drawing the eye out of the inner canthus. It is seldom required. He separates the eyelids by an elastic wire speculum, which at once raises the upper and depresses the lower. The fingers of the assistant we deem preferable.

He cuts away the tendinous end of the muscle, which is left adhering near the cornea, after the section is made. We see no harm in this; but no part of the conjunctiva should be removed, as this leaves a broader cicatrice, permits fungous granulations to rise more readily from the bottom of the wound, and favours protrusion of the eye. Some operators

are so solicitous about the conjunctiva healing closely, that they bring its edges together with a fine suture.

Dr. Brett's work is wound up with an appendix, "Concerning certain interesting Phenomena, manifested in individuals born blind, and in those having little or no recollection of that sense on their being restored to sight at various periods of life."

Metaphysicians have been led to expect, that a great degree of light might be thrown upon the peculiar provinces of the senses, and especially upon what belongs to sight and what to touch, by examining patients immediately after being operated on successfully for cataract. It seems quite certain, that a man born blind, being made to see, would at first have no idea of distance, nor magnitude, by sight. The sun and stars, the remotest objects as well as the nearer, would all seem to him to be in his eye, or rather in his mind. He would judge his thumb, with which he could hide a tower, or hinder its being seen, to be equal to that tower. Though his previous conceptions of tangible extension, divisibility, figure, and mobility might be considerable, he could have no idea of colour, nor of visible figure, nor of visible motion. To be of any use, however, to the metaphysician as a subject of experiment, for determining the truth or falsehood of the Berkeleian theory of vision, *First*, the man must have been totally blind; and, *secondly*, the acquirement of sight being accomplished, the experiments must be made the very first time his eyes are exposed to the light. Upon these points, metaphysicians have been grossly deceived, not being aware that cataract never deprives a person entirely of sight, and that in the course of recovery from the operations for cataract some amount of new visual experience is always obtained. The most completely developed cataract never prevents the patient from being able to tell, whether a candle is a few inches only from him, or several feet, or several yards; whether it is at rest, or in motion; whether it is moved vertically or horizontally. The generality of cataractous patients can distinguish the size, and shape, and colour, of most single objects, of about the size of the hand, when presented to them so as to be well contrasted with neighbouring objects, and their eyes turned from the light. Congenitally cataractous subjects are still more unfit than any others for the philosophical experiments in question—for this reason, that in them, the diseased lens does not grow in proportion to the rest of the eye, it remains small, or even shrinks, so as to become what oculists call *siliquose*, the consequence of which is, that the patients see very considerably, sometimes even to read large letters, by means of the rays of light admitted to the retina around the edge of the cataract.

Cheselden's expression that the blind lad upon whom he operated, and whose case has so often formed the basis of discussion, "was so far from making any judgment of distances, that he thought all objects touched his eyes," has also done much to mislead philosophers upon this head—an expression which Cheselden never could intend to be taken literally, and which, in fact, has no meaning, as no object of sight ever acts upon the eye in any way resembling contact. Even in the cases best calculated for testing the Berkeleian theory; those, namely, in which the cataracts have been large and dense, and the success of the operation sudden and perfect, the astonishment and agitation likely to be produced in the

patient must be very apt to render him unable to give anything like an intelligent or satisfactory account of his new sensations.

None of the cases related by Dr. Brett are of any value in a philosophical point of view. It is amusing to find him remarking in regard to one of them, that "as his new sense could scarcely be said to have been fairly exercised more than fourteen days, further observations could not be made as to his judgment of distances, positions, forms, and motions!" Verily, the doctor has studied the subject pretty much in vain, if this is all he knows about it.

Art. VI.

Den Pathologiske Anatomie's Svar paa Spörgsmaalet: Hoad er Cancer? Ved Adolph Hannover, Lic. Med.—*Kjobenhavn*, 1843.

The Response of Pathological Anatomy to the Question: "What is Cancer?" By Adolph Hannover, Licentiate of Medicine. With a Lithographed Plate.—*Copenhagen*, 1843. pp. 106.

So much has recently appeared in the pages of this Journal, on the subject of Cancer, that we fear to exhaust the patience of our readers, when we present them with yet another essay on this important and obscure disease. Still we feel certain, that, in laying before the British public, the results arrived at by a laborious and conscientious investigator, by one, whose researches on respiration, and on the nervous system, have already earned for him a fair meed of fame; we are contributing to elucidate still further, a subject on which too much attention cannot be bestowed. Whether the work now before us tends to corroborate the researches of others, or to render doubtful some opinions which have been very generally adopted, we deem it of importance that our British readers should know what has been ascertained in regard to the pathology of cancer by a Danish writer, and how materially the investigations of Dr. Walshe are confirmed by the testimony of Dr. Hannover's experience. We shall confine ourselves, in this notice however, solely to the author's own opinions, only briefly adverting, where it is unavoidable, to the doctrines laid down, and defended by any other writer. Dr. Hannover divides his essay into three parts, in the first of which, he details minutely the anatomical and microscopical appearances of cancer; the succeeding part is devoted to the history of the disease, according to its locality in the various organs and parts of the human body; and the third and last comprises our author's own opinions on the origin and nature of the malady.

After a few remarks on the contradictory opinions, that have prevailed, regarding the true nature of cancer, Dr. Hannover proceeds to detail his own investigations with the microscope, into the phenomena of cancerous growth and formations.

It was not, as our author confesses, till he had overcome many difficulties, and experienced many grievous disappointments, that he decided upon rejecting the caudate cells, as a diagnostic sign of cancerous tissue, under the microscope, while, at the same time, he was enabled finally to determine, that another form of cell exhibiting several marked peculiarities,

may really be regarded as characteristic of the presence of this malady. Such too we know to be the conclusion arrived at by Dr. Walshe, in regard at least to the value of the caudate cells, and from a close comparison of his description with those of Dr. Hannover, and especially with the lithographed plate given by the latter, we are inclined to believe that the *flat parietal opaque body* regarded by Dr. Walshe as a probable essential product of cancer, is to be found in the nucleus of the cancer-cell, described by our Danish investigator.

In imitation of Dr. Walshe, who was the first pathologist* to admit encephaloid, scirrhous and colloid, as three equally distinct species of one genus cancer, to the exclusion of melanosis and various other ill-defined products, Dr. Hannover recognises three species of cancerous degeneration: 1, the medullary (cancer medullaris); 2, the schirrous, (cancer schirrosus); and 3, colloid cancer (cancer alveolaris). He however hesitates, as to the propriety of associating this last with the two former, as he has never found in colloid cancer, those peculiar bodies, which he has invariably observed to be present in the two first-named species. But he acknowledges that he has had comparatively but few opportunities of investigating the microscopical appearances in colloid cancer.

In our author's definition of cancer we do not find much that is novel:

"Anatomically speaking, cancer may be defined as a heterologous organic formation, distinguished under the microscope by one peculiar feature, viz., a unique cell, which we will call a cancer-cell. In a pathological point of view, it may be described as a formation (depending probably on a peculiar diathesis), which destroys or replaces the normal structure of the part in which it occurs, having the tendency to be reproduced when the affected part is taken away, and in general giving rise to constitutional disorder and to cachexia, which eventually proves destructive to life."

Dr. Hannover, describes, as follows, the proper cancer-cells.

"These cells are round or oval, and only appear angular when closely pressed together. They are pale and finely granular on the surface, and form vesicles filled with a very pale fluid, as can best be shown by setting the whole mass in motion, when the cells may be observed to roll (*upon themselves?*). Their contour is never sharply defined. Within the walls of the cancer cells, we observe one or more nuclei, which proportionally are of considerable size. They appear somewhat darker than the containing cell, and are more sharply defined. We almost always observe several nuclei in one cancer-cell. These nuclei appear also to be hollow bodies. They are sometimes grooved in the centre, and exhibit a tendency to subdivision. The molecules of which the nucleus is composed cluster about the little, round, semitransparent nucleolus, of which one or two are to be found inclosed in each nucleus."

When cancer is softening, the membrane of which the cancer-cells are composed disappears altogether, and the nuclei alone remain, though not unchanged, for they have a shrunken withered appearance, and the nucleolus is no longer visible. In this form they bear the closest possible resemblance to pus-cells, but may easily be distinguished from these by the addition of a little acetic acid, which does not alter the withered aspect of the cancer nuclei, while in the cells of pus, it causes the immediate reappearance of the one, two, three, or four nucleoli. The above

* Cyclopædia of Surgery, July, 1840.

described cancer-cells have not been met with by Dr. Hannover in any part of the healthy body, nor in any diseased tissue, saving in that affected with cancer. The chief characteristic of these cells is the large nucleus, with a brilliant nucleolus, and the existence of several such nuclei in the same cells.

The other chief constituent (Hovedelementardeel) of cancer is cellular tissue, which is of course not confined solely to this morbid growth, nor does it appear there under any peculiar form.

"Under the microscope, we observe completely-formed cellular membrane, consisting of pale, cylindrical, branchless fibres, which are peculiarly recognisable by their serpentine course, and watered aspect (valtret udseende); and again we meet with cellular tissue in its embryonal form, i. e. as fibres springing from formative cells. The embryonal cells and fibres of cellular tissue appear under two forms: the former is that of pale, finely-granular cells, of very irregular shape, round, star-shaped, flat, or sometimes elliptical, or even still more prolonged. The nucleus is also extended, many nuclei occurring in one lengthened cell, but this last is formed by the junction lengthways (efter laengden) of several cells, by which means the nuclei are compressed so as to form a continuous thread or fibre. The nuclei are generally of a darker hue, and contain from one to four very small nucleoli. From either, or from both ends of those extended cells, there proceeds fibres which are finely granular, and certainly paler and broader than those of cellular tissue, but still the latter undoubtedly is formed by these prolongations of the cells."

These are the caudate cells, which have been regarded by Müller and Valentin, as almost, if not quite, characteristic of cancerous degeneration. The other species of fibre does not originate from cells, but from nuclei, which are very small spindle-shaped bodies, and destitute of nucleoli. Fine linear threads arise from one or both ends of these nuclei; they are somewhat thicker at their point of origin, and two often spring from the same nucleus. This latter form is but rarely seen in cancerous tissues.

The cancer-cells then, and cellular tissue, are the two chief constituents of the cancerous mass, but in addition to these, we likewise constantly meet with other substances, according to the parts of the body in which the morbid alteration may have taken place. Crystals, pus-cells, and exudation-corpuscles (Betaendeloeskugler) likewise occur as occasional products, but they are not characteristic of cancer. Dr. Hannover has not as yet observed cancer-cells in the third form of cancerous degeneration, i. e. in cancer alveolaris.

After these general remarks, our author proceeds to describe his first species, cancer medullaris, the encephaloid cancer of Dr. Walshe and others; but we omit the well known anatomical description of this morbid alteration, to arrive at the author's views respecting the process of its softening and destruction. This process of softening, which, as we have already pointed out, is accomplished by the destruction of the cancer-cells, takes place in two ways in the medullary species. 1. The centre, and shortly afterwards the great mass of the diseased growth breaks down, and forms a soft, semifluid pultaceous matter, which becomes daily thinner in consistence, so that at length the central contents may almost resemble serum in fluidity, while the external portions are still, to a certain extent, hard and resistant, and, from the admixture of blood with the fluid within, the whole contents become of a dark brown or chocolate hue, mingled with detached pieces of the cancerous mass.

2. Softening may commence in many points at one and the same time, and in such cases, a number of small white specks show themselves, consisting of a white pultaceous matter, composed of altered cells; these points or specks increase in size, and may run together into large cavities or canals, filled with the pultaceous matter above alluded to. Dr. Hannover has often met with this form of softening where the ground colour of the diseased mass was grayish, where its consistence was diminished, and it was semi-transparent; the white points then are very plainly seen upon the grayer ground, they are usually very numerous, and form sometimes curved lines (Rader) or streaks, upon the cut surface, so that at first sight, the mass may be regarded as appertaining to schirrous cancer.

Dr. Hannover does not venture to decide upon the immediate cause of the softening of cancer, but he thinks it possible that it is preceded by a process akin to that of inflammation if not absolutely identical with it, for he has (like Dr. Walshe) frequently observed exudation-corpuscles in the softened mass. It is, however, clear that, as Dr. Walshe has shown the distinct evidences of inflammation are not to be detected in many softened particles of cancer, inflammation cannot be—or at least cannot, in the present state of knowledge, be admitted to be—the sole agency at work in producing softening. Our countryman thinks, and we believe with much argumentative evidence in his favour, that the process must in some cases be analogous to the well-known *self-softening* (if it may be so styled) of fibrinous coagula.

With the historico-critical description of this first variety of cancer, we need not detain our readers, as our subject is solely to acquaint them with Dr. Hannover's original investigations. With the opinions of almost all writers up to the date of his own essay, our author seems to be thoroughly conversant, and it is gratifying to our insular self-esteem to find English authors so repeatedly and copiously quoted by our Danish brethren. Müller's cancer reticulare Dr. Hannover refers to the medullary species, and the reticular configuration which Müller considers to be its chief characteristic, our author asserts to result solely from the softening of the masses by the destruction of their cancer cells; in the latter point his experience agrees with that of Dr. Walshe. The second species of cancer, the scirrhus of authors, is described by Dr. Hannover, under the name of cancer schirrosus. Cellular tissue is a much more predominant constituent here than in the former species, rendering the mass much harder and more tough and resistant.

When cut into it exhibits a well-defined double composition, viz. of cancer-cells congregated into an almost gelatinous, white, or reddish, semi-transparent, or opaque mass, and these are contained in the intervals between the strong meshes of cellular membrane which traverse the tumour in every direction. Frequently the cellular membrane may be found aggregated in close bundles, which often spring from the point to which the cancerous growth is attached. Here the cancer-cells are generally but few in number, and upon their relative proportion to the cellular membrane, depends the variable consistence and aspect of the whole mass.

The softening of schirrous cancer progresses in the same manner as described in regard to the former species, viz. by destruction of the cancer-cells, but the cellular tissue participates only at a later period. When the mass has lost much of its pristine hardness, small cavities are often found

within it, filled with a brownish ichorous fluid, in which Dr. Hannover frequently observed exceedingly minute crystals, of a character very difficult to determine, some of which, however, seemed to him to be prismatic. Our author considers the carcinoma fasciculatum of Müller to be merely a variety of this species. Dr. Walshe, on the other hand, having described specimens of this structure which fell under his notice, gives his opinion that it must be ranked as "a distinct and important variety of encephaloid." Time will settle the difference: *softness* is at all events one of the most remarkable characters of this cancer.

The second division of Dr. Hannover's essay (devoted to the consideration of cancer as it affects various parts of the human body) need not delay us: it contains nothing new.

In the third division of this essay, our author explains his own views of the nature and origin of cancer, and criticises with much fairness and candour, the opinions of others. These observations are here restricted to the two first-named species of cancer.

The origin of cancer from irritation or inflammation, as was maintained by Breschêt, Ferrus, and Broussais, is not now recognised by many pathologists, and we would gladly, had our space permitted it, have extracted Dr. Hannover's lucid account of the difference between cancer and the products of inflammation. This difference we think our author establishes in the most satisfactory manner, and we next find him opposing the doctrines of Cruveilhier and others, who maintain that the circulating system and, especially, the nervous system, have a great share in the original development and extension of cancer. Dr. Hannover rejects in toto the doctrine that the veins are the seat, i. e. the anatomical element affected in cancer. We have not space here to follow him through the six or eight pages he devotes to the consideration of this theory, and which has not found greater favour with our countryman Dr. Walshe. Dr. Hannover does not deny the occasional appearance of cancerous matter in the veins and sinuses, but he contends that this should not be regarded as conclusive evidence in favour of the venous system being its original seat. In fine, he adopts to a great extent the doctrines of Schleiden and of Schwann, as applied to cancerous growths. Müller and others have stated that they have not discovered any peculiar elementary form in cancerous tissue to serve as characteristic of their nature, but Dr. Hannover repeats the assertion, that he has never, save in cancerous growths, observed the peculiar cancer-cells, described in the first part of this essay. We have already noticed that our author is unwilling to speak decidedly as to the nature of cancer alveolaris, he states that he has recently had but two opportunities of examining this form of cancer; and in both instances, his examination, from causes he could not control, was more or less incomplete. However, he observes, that it is a remarkable fact, that both Gluge and Müller have discovered peculiarities under the microscope in this variety, which will, if further corroborated, entirely separate it from the medullary and schirrous forms. Nor has Dr. Hannover ever seen or read of a case where alveolar cancer, when extirpated by the knife, has been succeeded by the medullary species, an occurrence not altogether uncommon in the history of schirrous cancer. Indeed our author would wish to form two distinct genera of malignant disease, i. e. colloid and

cancer, but from the insufficient data he has hitherto collected, he dares not positively separate them.

With Dr. Walshe then, Dr. Hannover evidently believes that cancer is formed *external* to the blood-vessels; that it originates in nuclei formed in the plastic exudation of the blood, and that these subsequently form cells, some of which are afterwards developed into fibres, in the shape of cellular tissue, while others, which have not this property of originating fibres, are the true cancer-cells. These last, like the cells of pus, are always the most rapidly productive when their nuclei are most numerous: thus in cancer medullaris we find many nuclei, and it is precisely in this species that we observe the most rapid development of the morbid growth, particularly when the skin has given way, and the pressure upon the cells has been thus partially removed. Cancer schirrosus increases more slowly; for many years it may remain stationary, and is less liable to be reproduced when once extirpated.

Finally, Dr. Hannover touches briefly on the disputed question of the constitutional character of cancer, and he is evidently not disposed to admit of it to the full extent that is conceded by many writers. Indeed, he states that cancer, when it remains long stationary in only one part of the body, has no positive title (why then in his definition admit that the disease depends on a diathesis?) to be considered a constitutional disease. Dr. Walshe, we know, has taught a doctrine the very reverse of this, and upon the review of a mass of facts and arguments, as extensive, perhaps, as was ever adduced in illustration of any medical question, comes to this conclusion: "*A cancerous tumour, under all circumstances, even should it remain single and stationary for years, is but the local evidence of a general vitiation of the system.*" It occurs to us inquire of Dr. Hannover how it happens, if his notion be correct, that when a perfectly quiet and old standing cancer is cut away from a person in *apparently* good general health, it sometimes (too often as surgeons know) happens that multitudes of cancers almost immediately spring up into activity in certain of the internal organs?

Our readers will have observed ere this that the researches of Dr. Hannover agree in most of their material points with the previous* and likewise the more recent investigations of Dr. Walshe. The essay of the former, is of course a much less elaborate production than the volume of the latter, but it is most satisfactory to find so close a coincidence between independent observations made by these two distinguished pathologists at nearly the same time in different parts of Europe. Perhaps after meeting with the work of Dr. Walshe, Dr. Hannover may favour us with a more ample development of his own views, aided by accumulated observations and increased experience, but to whatever subject in the whole range of physiology or pathology he may turn his attentive and inquiring mind, we are quite sure that his investigations will receive that notice and appreciation they have hitherto so eminently deserved.

* It is just to both parties to state, that Dr. Hannover was acquainted with, and has liberally availed himself of, Dr. Walshe's article, "Cancer," in the Cyclopædia of Surgery, published in 1840.

Art. VII.

Della Mortalità e dimora media dei malati nello Spedale Maggiore di Milana dal 1811 *al* 1844, *ed in quello dei RR. Fatebenefratelli dal* 1604 *al* 1844, *coi prospetti del calcolo compelessivo sopra* 784,000 *infermi.* Memoria del Medico-Statista Dottore Guiseppe Ferrario, di Milano.—*Milano,* 1845.

On the Mortality and Mean Length of Abode of the Patients in the Large Hospital of Milan from 1811 *to* 1844, *and in that of the Brothers of Charity from* 1604 *to* 1844, *&c.* A Memoir, by Dr. G. Ferrario, Medical Statist, of Milan.—*Milan,* 1845. 8vo, pp. 16.

This pamphlet was prepared for communication to the medical section of the sixth congress of the Italian Association of Science, held in Milan in September, 1844, and the author has published it with the hope that others will be induced to follow his example, and that thus a vast series of facts may be obtained, upon which the hospitals of different regions may be compared. He thinks at least, that great practical utility must result from the establishment of various *laws of probability,* according to which males and females suffering from ordinary diseases, die or are cured monthly, and annually in different places. He would wish to establish in every place where an hospital or dispensary exists, a long and continued series of exact statistical annotations, showing the ordinary mortality and proportion of cures upon every 100 sick, monthly and annually. He thinks that from this practical corollaries might be deduced of great value, and then rational experiments might be made to determine the value of different modes of treatment.

The following conclusions are drawn from his first table of the mortality, &c. in the large hospital at Milan, during thirty years.

1. The mortality of the sick in the great hospital of Milan, monthly and annually, is constantly greater in females than in males, in the proportion of 2 to 6 per cent.

2. The mortality from November to the end of April (that is, the whole winter and spring) varies in males from 14 to 19 per cent.; in females from $16\frac{1}{2}$ to $24\frac{1}{2}$ per cent.; and the total mortality of the two sexes from 15 to 21 per cent.

3. The mortality from May to the end of October, (that is, summer and autumn), in males, varies from $7\frac{1}{2}$ to 12 per cent.; in females, from $11\frac{1}{2}$ to $14\frac{1}{2}$; and in both together, from $9\frac{1}{2}$ to 13 per cent.

4. During the whole period, the mortality of males, and of both sexes taken together, is at the minimum in September, it begins to increase in October, and continues to increase until it attains its maximum in January. It begins to decrease in February, and continues to do so in the succeeding months of spring and summer, and attains its minimum in August, and particularly in September.

5. The mortality of females calculated alone in the six months of winter and spring, proceeds in exact proportion with that of the males, but differs in the other months. The minimum mortality of females is in August, and begins to increase in September, proving that females suffer

more quickly than males from the morbid influences of autumn, and from the cold of winter.

6. The mean annual mortality of males was 12·41; and of females, 16·13; and of both sexes, 13·82 per cent., including those brought dead, or in the last moments of life to the hospital. It is to be observed, that about 400 chronic incurables are always in this hospital.

We need not follow the accounts from the other hospital, but it appears that the facts just stated exactly agree with those deduced from the second table.

The stay in hospital was always longer in females, exceeding at least two days that of males; and in August four days. The mean stay during the thirty years, was 13 days for males and 15 for females.

It is stated that the Milanese females are much less willing than the males to go to hospital, preferring rather to be treated at home by the permanent physicians for the poor. Their number in hospital is always less than that of males, and their diseases are in a more advanced stage.

In the other hospital the mean stay was $21\frac{1}{2}$ days. This increased time is owing to the convalescents being detained for a longer period, as there is a large ward set apart for them, and they are selected from the better class of the lower orders, poor but not destitute, whereas no distinction of this kind is observed in the larger establishment.

Dr. Ferrario is inclined to believe that the fact of such a mass of patients being discharged about the 14th and 21st day, supports the hippocratic doctrine of critical days. It must be remembered that patients are not brought to hospital until some days after the commencement of disease; but this is compensated for by the days of convalescence, and thus the mean stay of 14 days in one hospital, and 21 in the other, is supposed to be in relation with critical days on the seventh. But in our opinion the facts by no means warrant any such inference. Of 400 chronic incurables constantly in hospital, and who leave it only when death frees them from their sufferings, it must be quite clear that a large number must considerably exceed a stay of three weeks, and may probably remain several months. Now to bring the general average down to 14 or 21 days, it is also evident that a considerable number must be discharged within the lowest of those periods, and thus it appears very doubtful if the mean number would represent the actual stay in hospital of the majority of patients. If he had wished to examine this question, the author should have discovered the number of patients discharged from the 1st to the 7th day; the 7th to the 14th; the 14th to the 21st, and so on; and if any considerable majority were discharged on any particular day in either of these periods. This inquiry might be also advantageously employed comparatively in fever hospitals, and those in which other diseases are treated.

If patients are discharged on a particular day of the week in Tuscany as they are in England, this may account for the terms of residence being multiples of seven.

The author is of opinion that great benefit would result from trials of any given remedy, or plan of treatment, in the same months in all the principal cities of Europe; and that thus some appreciation might be made of meteorological influences in modifying the effect of remedies, by comparing the mean mortality and duration of treatment. But there are

many and obvious difficulties and impediments to the execution of any such plan. There might arise questions as to accuracy of diagnosis, previous condition of patient, and the endless modifications of diseases quite beyond the reach of the statistician, and referrible to many other moral and physical causes, totally independent of climate or weather. The inquiry is nevertheless quite as well worthy of simultaneous notice as the meteorological observations, suggested by Sir John Herschel. The relative success of various surgical operations in different localities, and at different seasons, should also be investigated. Something must evidently be done to relieve medicine from the uncertainties under which it labours; and we know of no other way in which this can be accomplished, than by making extensive observations on a uniform plan, of such facts as admit of exact determination and statement.

We need not say more with regard to the pamphlet before us, than that the tables appear to be prepared with much care and industry.

Art. VIII.

Ueber Schädel-bildung, zur festeren Begrundung der Menschen-rassen. Von Prof. Dr. A. Zenne.—*Berlin,* 1846.

On the Form of the Skull as supplying a Distinctive Character of the Varieties of Mankind. By Professor Zenne, M.D. Part I.—*Berlin,* 1847.

In the commencement of his work, the author objects to that view of the origin of the human race from one original pair, which is founded on the Old Testament. In the third chapter, entitled "Schädel-polarität," he complains that, on examining the rich collection of skulls in the Berlin Museum, in reference to Retzius's arrangement of the various nations, he, as well as other more experienced anatomists, found considerable difficulty in determining to which of the last-mentioned author's classes several of the specimens belonged. He says, for example, that the skull of a Swede, presented to the Museum by Retzius, was found to be shorter than the skulls of two Russian ladies in the same collection; further, that Retzius refers the Incas to the brachycephalæ, while, in the opinion of our author, their crania are remarkably long, and he appeals to the long Peruvian skull figured in Prichard in proof of the correctness of his criticism. In conclusion, he finds fault with the term "Tartars," as indefinite, and applied by turns to Turks and Mongols.

We must take the liberty, *in limine,* to point to the great difficulties connected with the formation of ethnographic collections of crania, and to the wide difference between these and collections of other objects in natural history. The race of man includes but one species; the different types are but varieties, presenting only differences in degree, together with hybrids, produced by crosses of such varieties. More than one national type is indigenous in most countries. In a large proportion of countries great migratory movements have taken place, and often the remnant of a former race has adopted the language of the more powerful. This appears to have been the case in North Germany, where the people,

originally Sclavonic, have in process of time adopted both the language and the customs of the Germans, and thereby become amalgamated with the German race. Similar phenomena obtain in many parts both of the old and new world. Moreover, we must not overlook the influence of smaller immigrations of foreign nations, as well as of the considerable number of individual peculiarities of form, which present themselves in highly civilized countries.

Under these circumstances very extensive investigations are necessary to the determination of primitive forms, and they should be made as far as possible both on living individuals and on prepared crania; not forgetting that such investigations require an eye accustomed to the examination of objects of natural history. Rightly to study the forms of crania, we ought, if possible, to have a large number of specimens before us, which must be examined in different positions, and all in the same position at the same time. As arranged in museums, the face is generally the only part visible. To make a more complete examination costs a great deal of trouble, and easily puts a whole collection into disorder, for which reason such can seldom be made in a large museum without damage; and yet, by an examination of one or more specimens only in the usual manner, no certain opinion can be formed, unless the observer has previously acquired very considerable experience.

In the great museums of Paris, Berlin, and London, the number of national skulls is very great, and, if we do not mistake, a large number of them have been collected on the battle-fields of 1812-1814; a still larger number have been contributed by travellers, or purchased of dealers in objects of natural history; *but the necessary ethnological information is wanting with respect to the majority.* As we have already remarked, it is not sufficient to know that a skull is that of a Frenchman, Englishman, Russian, and so forth, for France is peopled by Basques, Germans, Normans, several Celtic varieties, &c.; the case in England is much the same, and the learned author knows well, that the number of different races is still greater in Russia, that vast empire, to which most European nations are thought to owe their origin. Thus, Count Demidoff reckons fourteen different races as belonging to the Crimea alone. We cannot, therefore, attach any importance to the remark, that two Russian crania were found to be longer than a Swedish; nor should it be forgotten that Retzius does not use the term Russian, but Sclavonic, and our author evidently agrees with him touching the form of the head of these latter when he says, pp. 12: "Der weit verbreitete Stamen der Slawen welche vorherrschend Turanische Schädelbildung haben;" and p. 20, where he speaks of the Turanian race as synonymous with the Mongolian, in his class "Breitschädel," while, on the other hand, he places his Iranian or Caucasian race in the class of "Hochschädel."

Our author's objection to placing the Incas among the brachycephalæ is quite as much a mistake, the more remarkable as he refers to Prichard's 'Researches into the Physical History of Mankind,' vol. i. plate I. The skull there figured, from the valley of Titicaca in Peru, which, together with a number of similar specimens, was brought to Europe by Pentland, and deposited in the Hunterian Museum, is not asserted by Prichard to be that of an Inca. As far as we know, all ethnographers consider these

long crania as having belonged to a people inhabiting Peru before the advent of the race of Incas. Tschudi, who found families of these long-headed Peruvians still living in the department of Junin, and who, like Pentland, examined a number of ancient tombs of this race, calls these aborigines, Huauca. We imagine that the best authority on this subject is Morton, whose classical work 'Crania Americana,' Philadelphia, 1839, our author quotes on two occasions, and who says of the Incas, p. 115,—"the skull in this people is remarkable for its small size, and also, as just observed, for its quadrangular form. The occiput is greatly compressed, sometimes absolutely vertical; the sides are swelled out, and the forehead is somewhat elevated, but very retreating." We think it quite unnecessary to add any reasons of our own in justification of the arrangement complained of.

Prof. Zenne considers the appellation Tartars inexact, being applied sometimes to Turks, sometimes to Mongols; and to Tungusians, he might have added, among whom the Maudschu Tartars are also included. The term Tartar is certainly not a very exact one, no more is Tartary, but on the other hand, the name is so remarkable in history, and so generally in use, that we think it possesses a just claim to a place in the list of nations, especially as we feel convinced that all the races designated by the name, are brachycephalic. He also disapproves of Retzius's arrangement, as being founded on a simple polarity or antithesis of cranial forms, contending that the cavity of the skull must be measured in a threefold manner—namely in relation to its length, breadth, and depth. We have only to reply that this arrangement is based upon extensive and laborious researches, and seems to us all the more likely to be correct, as it is found to be intimately connected with the development of the cerebral lobes; and we may add, that at the Christiania meeting of Savans in 1844, Retzius mentioned it as his opinion, that several transition classes between those already determined in his scheme, would be necessary, but that their formation would require the most accurate and probably extensive examinations. For example, the people called by Dieffenbach, (Travels in New Zealand, London, 1843,) "True Polynesians," and considered by him as a branch of the Malay stem, have almost square skulls, not short but high, with large tubera parietalia. They form a transition from brachycephalæ to dolichocephalæ, but are most nearly allied to the first named.

It is doubtless within the bounds of possibility, to make a really good arrangement of cranial forms, by including in the investigation the three proposed measurements of the cavity, provided always one goes the right way to work, which our author would scarcely seem to have done, when he confounds the face with the skull proper. Thus he says that one recognises "die Formen der Hochschädel" by the oval form of the face of south and west Europeans, as well as of the inhabitants of south and west Asia as far as India; but it is well-known that this oval face belongs as much to the long-oval as to the short-square and round skulls. It may be all very well to flatter one's self on the possession of a high skull like the Apollo Belvidere, but truth obliges us to confess that the skulls of the German, Dutch, Norman, Scandinavian, Celtic, and several other west and south European, as well as west and south Asiatic nations, are long-oval, not high, nay, often even very low, though this circumstance does not by

any means prevent the face from presenting a handsome oval. Anatomy teaches us that only a small portion of the skull or brain-pan enters into the composition of the face, forming the forehead; all beneath the eyebrows belongs to the face and jaws, not to the brain-case. If the anterior lobes of the brain are well developed, the forehead is high, and if the face be of a regular oval, the individual may present in front, the characters of our author's "Hochschädel," while in fact he has a long, low, skull. The same erroneous views present themselves with regard to the "Langschädel" concerning which, he says, "endlich in der Form der in den *Kiefern* verlängerten, und im *Gebiss* nach vorn gerichteten Schädel der neger, erschauet man die '*schnauzenartigen Formen der Langschädel.*'" Here the face and brain-pan are confounded in a striking manner.

Our author considers Papuas and Alfourous, as mixed races of Turanians and Sudanians, as also Hottentots and Bushmen of Sudanians and Malays. With respect to the first named, the forms of their skulls are different, and the proposition is besides founded on very uncertain grounds; and as to the latter, it is all but demonstrated that the Hottentots are the aborigines of south Africa, and that the Caffres have at a later period taken possession of their country.

Our author assumes a polarity or antithesis in height and flatness of of the cranium, between the Northern and Southern, as well as a similar contrast between the inhabitants of the Eastern and Western hemispheres; principally shown in the presence of an interparietal bone among the inhabitants of the New World. He likewise asserts that the straight facial line is paramount in the North, while prominent cheek bones characterize the inhabitants of the South. But the so-called "True Polynesians" have high skulls, while, on the contrary, Scandinavians have in general long and low ones; flat heads, the lowest of all cranial forms, are found in Oregon, while the Araucanians of Chili have high skulls. The second assumption is equally far from the truth, for Greenlanders have large and prominent cheek bones, and, according to Meyen, the Tagalians of the South Sea, and, according to Dieffenbach, several Polynesian nations have a form of face resembling that of Europeans. ("In their features they approach the Caucasians"—"On true Polynesians." p. 4.) One thing at least we may be sure of, that their cheek bones are not more prominent than those of Greenlanders.

The interparietal bone, which Zenne thinks characteristic of Americans, has been as yet only observed among Peruvians in the New World, and he allows that, though rarely, it does now and then occur among Europeans. Our opinion is, that this bone will occasionally be found in the skulls of all the varieties of man, certainly quite as often in all other as in European.

That the change in the form of the skull brought about by artificial means, among several American tribes, is connected with an observed natural tendency towards the form so much admired by them, we are quite prepared to admit.

Zenne considers that the highest portions of the globe were first inhabited; namely in the east, Iran, Turan, and Sudan; in the west, the Bolivian, Guianian, and Appalachian chains. He places the high-skulled race in the north, the broad-skulled in the middle portions, and the long-skulled in the South, according to the following scheme:

NORTH.

	Western Hemisphere, or New World.	Eastern Hemisphere, or Old World.	
	I. *High Skulls.*		
	4 Appalachian, or Natches Race.	1 Caucasian, or Iranian Race.	
	II. *Broad Skulls.*		
WEST.	5 Guianian, or Carib Race.	2 Mongolian, or Turanian Race.	EAST.
	III. *Long Skulls.*		
	6 Peruvian, or Inca Race.	3 Ethiopic, or Sudanian Race.	

SOUTH.

That the above arrangement is not a happy one, is evident from the remarks we have already made; we will only add, that Zenne places his high skulls, the Iranian and Natches races, farthest north. In the Old World, at least, the Turanian ("broad-skulls" of Zenne), including Lapps, Samoiedes, Yakutans, and Kamschatkans, is the recognised Northern race. The Flatheads of Oregon already mentioned (very low), are found further north than the Natches, who, emigrating from Anahuac, have in later times dwelt in Florida and Louisiana. Still further north do we find races with skulls, differing, it is true, in form, but yet not high ones, namely, Esquimaux, Hurons, &c.

To place Guianians or Caribs in the same class with Mongols is also an evident mistake, as the former have long, the latter short skulls.

According to our author, "long skulls" are represented in the Eastern hemisphere by the Negro race, while in the Western, Incas (Tschudis "Huancas") present the typical cranial forms of the inhabitants of the South; but the Malays and Papuans of the South Seas have *not* long skulls, and must be placed in the author's class "Breitschädel;" and he seems to have overlooked the fact, that the Puelches and Araucanians, the former in the Argentine Republic, the latter in Chili, do neither of them belong to his "Langschädel," but, like the Malays, have a Mongolian form of skull.

At page 22 we find the diagnoses of our author's six races of men, but he does not mention what nations are included in each race; had he subjected the correctness of his system to this trial, he would doubtless have himself observed the very same objections to it, which we have pointed out.

The plates are good and true to nature—no slight praise,—and just on that account are they invaluable witnesses to the truth of our criticisms.

Those races, both in the Old and New Worlds, which our author refers to the same class, present in these plates nearly opposite forms; for example, the short, high, flat-necked Natches is placed alongside the long, lower German skull, remarkable for its prominent neck, and the long

Macusian forms a striking contrast with the short high Mongolian cranium. The same thing would have been observed if a *real* Inca skull had been placed by the side of a Negro's [we have already remarked that our author confounds Huancas with Incas.]

Towards the close of the work Retzius is mentioned as having adopted the theory of several primitive races; we beg to remind our readers that he has, in his published works, hitherto only treated of cranial forms, and is of opinion that the subject of races cannot be investigated with a chance of success before these forms have been more exactly determined and arranged, fairly acknowledging that the scheme of races he has proposed is only an attempt at or approximation to a final arrangement.

Although we have been obliged to find considerable fault with our author's production, we feel pleasure in bearing witness to his learning, and his very respectable attempt to improve the yet undeveloped science of ethnographic craniology. Had he paid more attention to details, and possibly been more at home in anatomical investigations, and more accustomed to the examination of objects of natural history, he would certainly have been more successful in his researches, and have earned a greater meed of praise than we have felt justified in bestowing.

ART. IX.

1. *Théorie Expérimentale de la Formation des Os.* Par P. FLOURENS, Secrétaire perpétuel de l'Académie Royale des Sciences, Professeur de Physiologie comparée au Muséum d'Histoire Naturelle de Paris, &c. &c. Avec 7 Planches gravées.—*Paris*, 1847.

Experimental Theory of the Formation of Bone. By P. FLOURENS, Perpetual Secretary of the Royal Academy of Sciences, Professor of Comparative Physiology in the Museum of Natural History of Paris, &c. &c. With Seven Engravings.—*Paris*, 1847. 8vo, pp. 164.

2. *Elements of Anatomy.* By JONES QUAIN, M.D. Fifth Edition. Edited by RICHARD QUAIN and WILLIAM SHARPEY, M.D., Professors of Anatomy and Physiology, University College, London. Illustrated by numerous Engravings on Wood. Parts I and II.—*London*, 1843-6. 8vo, pp. 1063.

3. *Cyclopædia of Anatomy and Physiology.* Edited by ROBERT B. TODD, M.D., F.R.S., Professor of Physiology and of General and Morbid Anatomy in King's College, London, &c. &c. Article, *Osseous Tissue;* by JOHN TOMES, Surgeon-Dentist to the Middlesex Hospital.—*London*, 1846. Illustrated with numerous Engravings.

THE history of the formation, growth, and regeneration of bone has long been favourite subjects of inquiry, not only among anatomists and physiologists, but also among such practical surgeons as usually care little for such investigations. There are many reasons why this should be the case, sufficiently obvious to prevent the necessity of our dwelling on them. A vast number of observations and experimental results have been laid before the world at different times, by men of very different qualifications and habits of mind; and there has been accordingly much discrepancy of

opinion on many of the questions which the inquiry involves, and no inconsiderable amount of dogmatism has been manifested in support of the various positions advanced. In this instance, as in many others, however, the progress of knowledge and the prevalence of more philosophical habits of thought have tended to reconcile views which formerly seemed altogether opposed to each other; showing that the facts on which they were respectively based were in reality perfectly consistent, and that it was only in the exclusiveness of the deductions from them that those deductions were erroneous.

Of the attention which has recently been bestowed upon microscopic research, the osseous texture has come in for its full share. Here, too, many errors of observation were at first committed, which have since undergone correction; and many opinions were founded upon them, which have since been modified or abandoned. There has been obviously increasing tendency to agreement on many points which were at first matters of dispute; whilst new questions have been opened up in the more recondite parts of the inquiry; and various opinions which were the result of too limited an induction have been found to be no longer tenable, although the authority with which they were at first advanced, has caused them to be very generally admitted.

We propose, on the present occasion, to put our readers in possession of the results of those later inquiries on this subject, which have most tended to modify the views originally advanced regarding the structure and development of bone; and we shall then examine the results of the experimental inquiries of M. Flourens, with the view of contrasting their results with those at which we arrive by a much simpler method of inquiry, and of pointing out what we believe to be a gross exaggeration of his claims as an original discoverer in this department of physiology.

The elementary substance of bone, considered in its simplest aspect, appears to be homogeneous, even when examined with high powers of the microscope. Mr. Tomes, however, has directed attention to the fact that it possesses a minute granular structure.

"For the purpose of examination, it is best to take a very small portion of a thin plate of bone; such may be found in the ethmoid bone of small animals, as of the rat. If the piece be well selected, it will be found to contain no Haversian canals or corpuscles, but to be extremely thin and transparent. Such a portion, when viewed with the 1-8th of an inch object glass, will present a delicate granular aspect with the surface nodulated. This granular appearance arises from the substance of the bone being composed of minute irregularly spherical granules. It is not difficult to trace this structure in any specimen of bone, though in some it is much more distinct than in others..... ... The granules may be obtained separated from each other, so that each individual may be examined apart from its fellows. When so exposed to view, they exhibit a tolerably regular character, being mostly spherical, some few having an oval form. In some specimens, the oval predominates over the spherical conformation. Often a few will be found which are egg-shaped, with the smaller end elongated, though to no great extent. The osseous granules may be gained by subjecting bone to high-pressure steam, or to a red heat, till all the animal matter is removed. In either instance, the granules may be obtained by taking a small portion of the so-treated bone, saturating it with water, and then gently reducing it to a powder between the slips of glass. By this manipulation the granules individually will be rendered evident when the specimen is examined under a high power.....The granules themselves are subject to some variety in size,

commonly varying from one sixth to one third the size of a human blood-globule." (Cyclop. of Anat. and Phys., vol. iii, p. 848.)

The following observations by Mr. Tomes, on the ossification of permanent cartilage, are of much interest in connexion with the foregoing statements. He has particularly observed the process as it occurs in the cartilages of the larynx, where it takes place slowly, and with no preliminary change in the cartilage. Osseous granules are developed in the intercellular substance, only a few of them being at first seen, and these being spherical and isolated; but a much larger number gradually present themselves, which unite to form an osseous mass.

"The formation of the individual granules is more readily observed in these cartilages than in any other situation. This form of ossification establishes an interesting and explanatory link of connexion between bone and the various osseous plates we find in abnormal situations. For in the latter the spherical granules appear; and these, at first few and isolated, and lying among the fibres of the tissue, rapidly increase in number, unite and form an osseous mass. Osseous plates occur in various soft tissues as the result of deranged action, where in the healthy condition of the part they are not found. Thus we have osseous plates formed in the coats of arteries, in the pleura, in the diaphragm; also osseous masses in the uterus, and sometimes in the muscular tissue and in the placenta. These plates are all formed in the same manner, namely by the development of minute spherical osseous granules, which form into a mass, the shape of which is modified by the form of the tissue in which the development occurs." (Cyclop. of Anat. and Physiol., vol. iii, p. 857.)

This granular arrangement, however, appears to be characteristic rather of the mineral than of the organic constituent of bone; for it cannot be traced in the gelatinous basis, which is left after the earthy matter has been removed by the agency of an acid; on the contrary, this appears from the observations of Dr. Sharpey, which we have ourselves confirmed, to have a *fibrous* structure. If a piece of a long bone be steeped in dilute acid until its calcareous matter is removed, thin films may be peeled off from it with a little care in a longitudinal direction. Most of these fibres however, consist of several superimposed laminæ; and it is only by looking towards the edge, where the different layers are usually torn unequally, so that some extend beyond others, that we can find parts thin enough to show the real structure.

"It will then appear plainly that they are made up of transparent fibres, decussating each other in the form of an exceedingly minute network. The fibres intersect obliquely, and they seem to coalesce at the points of intersection, for they cannot be traced out from one another; but at the torn edge of the lamella they may often be seen separate for a little way, standing out like the threads of a fringe. Most generally they are straight; but they are not always so, for in some parts they assume a curvilinear direction. Acetic acid causes these fibres to swell up and become indistinct, like the white fibres of cellular (areolar) and fibrous tissue." (Quain's Anatomy, p. 143.)

Hence it would seem inappropriate to designate the organic basis of bone by the term cartilage; for it has nothing of the structure of true cartilage; and it differs also in chemical composition, being composed like the ordinary fibrous tissues of gelatin, whilst cartilage, whether temporary or permanent, is made up of chondrin. The prevalent notion of ossification has been, that the calcareous salts are deposited in the substance of the previously existing cartilage; so that the latter, consolidated by the

ossifying process, continues to exist in bone. We shall presently see, however, that there is much reason to believe the osseous tissue to be entirely a new formation; the previous cartilage (where it exists) being merely a temporary substitute for bone, serving at the same time as a sort of mould for giving shape to the bony deposit, and as a support to the still softer parts of the fabric.

In the midst of the dense substance of all but the thinnest laminæ of bone, we find certain dark spots with thread-like lines radiating from them: these, when first discovered, were named the osseous corpuscles, and were supposed to be solid collections of calcareous matter, to the opacity of which their dark appearance, under transmitted light, was supposed to be due. We believe, however, that all microscopists (in this country at least) are now agreed that these spots are *excavations* in the solid osseous substance, and that the thread-like lines are minute tubes or *canaliculi* proceeding from them. This is most conclusively proved by the passage of fluids through them, which may be distinctly seen to take place under the microscope when a dry section of bone is moistened with oil of turpentine. These *lacunæ* (to use the term first introduced by Messrs. Todd and Bowman) seem, in the recent bone, to contain a soft granular matter, which may possibly be a persistent cell-nucleus, and may have an important function in selecting the material which the canaliculi are destined to convey. The manner in which the canaliculi present themselves to the eye, when they are looked for in thin lamellæ torn from the decalcified animal basis, is thus described by Dr. Sharpey:

"The lamellæ are seen to be perforated with fine apertures placed at very short distances apart. These apertures were described by Deutsch, but they have not much attracted the notice of succeeding observers; they appear to me to be the transverse sections of the canaliculi already described, and their relative distance and position accord sufficiently with this explanation. According to this view, therefore, the canaliculi might (in a certain sense) be conceived to result from the apposition of a series of perforated plates, the apertures of each plate corresponding to those of the plates contiguous with it; in short, they might be compared to holes bored to some depth in a straight or crooked direction through the leaves of a book, in which case it is plain that the perforations of the adjoining leaves would correspond." (Quain's Anatomy, p. 144.)

When the lamella is thin enough for its fibrous structure to be distinguished, it may be seen that the perforations correspond to the intervals or openings between the reticulated fibres; and on this view the canaliculi are to be regarded as little vacuities left in the tissue during the deposition of the fibres, resembling the open figures left out in the weaving of some artificial fabrics.

Much attention has lately been given, especially by Mr. John Quekett, to the form and dimensions of the lacunæ, and to the mode in which the canaliculi radiate from them; as indicative of the class and order of vertebrated animals to which any particular bone may belong. It must be obvious that, if any distinction of this kind can be established, it will be most valuable in aiding in the determination of fossil remains, whose fragmentary condition prevents their character from being recognised by their external configuration; whilst it will afford an additional means of deciding upon the zoological position of a doubtful animal, such as the lepidosiren, whose structure is intermediate between that of two classes

of vertebrata. But we cannot attach any great importance to this test, until a much more complete examination shall have been made of the variations which may present themselves in the form, size, and arrangement of the lacunæ in any one animal; since it is quite possible that different portions of the same skeleton may present differences in these characters which we should at first be inclined to regard as diagnostic of difference of class.

We shall now direct the attention of our readers to the recent progress of knowledge in regard to the process of ossification, which has been the fruit of carefully conducted microscopic research. It was very natural that in studying this process, attention should be at first almost exclusively fixed upon that phase of it which presents itself in the consolidation of the long bones of young animals, which are at first laid down or moulded (so to speak) in cartilage; and that hence the *intra-cartilaginous* mode of ossification should be for a time the only one minutely described. Even this, however, is not yet fully understood; and for the due comprehension of it, we deem it requisite that the *intra-membranous* form of ossification, to which attention has lately been recalled by Dr. Sharpey, should be first elucidated. We shall therefore bring his observations under the notice of our readers in the first instance; and may state *in limine* that we have ourselves repeated and confirmed them in almost every particular.

That osseous tissue may be developed in the substance of fibrous membranes, is a fact well known to every pathologist; but the occurrence of such a method of development, as a part of the regular process of osteogeny in man and other vertebrata, has been commonly lost sight of; although it was clearly distinguished from the intra-cartilaginous method of ossification, more than a hundred years since, by Dr. Nesbitt, and has been since alluded to more or less distinctly by other writers on the subject. Dr. Sharpey is the first microscopist, however, so far as we are aware, who has applied himself to the determination of the minute stages of this process; which may be best studied in the tabular bones of the fœtal cranium. The base of the skull in the human embryo is cartilaginous; but in the roof we find (except where there happen to be commencing muscular fibres) only the integuments, the dura mater, and an intermediate layer of fibrous membrane in which the ossification proceeds. In the embryo of the sheep, the roof of the cranium is strengthened by cartilaginous plates which rise up from the base of the skull; but these are evidently destined only to afford a temporary support, and are not in any way subservient to the ossifying process, which takes place in the membrane on its exterior.

"The commencing ossification of the parietal bone, which may be selected as an example, appears to the naked eye in the form of a network, the little bars or spicula of bone running in various directions, and meeting each other at short distances. By and by, the ossified part becoming extended, gets thicker and closer in texture, especially towards the centre, and the larger bony spicula which now appear, run out in radiating lines to the circumference; the ossification continuing thus to spread and consolidate until the parietal meets the neighbouring bones, with which it is at length united by suture.

"When further examined with a higher magnifying power, the tissue or membrane in which the ossification is proceeding appears to be made up of fibres and granular corpuscles, with a soft, amorphous, or faintly granular uniting matter

The fibres have the characters of the white fibres or rather fasciculi of the cellular (areolar) and the fibrous tissue, and are similarly affected by acetic acid. The corpuscles are, for the most part, true cells, with an envelope and granular contents; some about the size of blood-particles; but many of them two or three times larger. In certain parts the fibres, but in most the corpuscles, predominate; and on the whole the structure might be said to be not unlike that of a fibrous membrane in an early stage of development. The bone, seen by transmitted light, is dark and opaque; and near the growing edge it is decidedly granular.

"On now observing more closely the bony processes or spicula at the circumference, where they shoot into the membrane, it will be seen, as you trace them into the soft tissue, that they gradually lose their opaque and granular character, indicative of earthy impregnation, and are prolonged a little way into the membrane, in form of bundles of transparent fibres, having all the characters of those of fibrous tissue. Those fibres are in some parts closely gathered into thick bundles; but more generally the fasciculi are smaller, and irregularly interlaced or reticulated, with corpuscles lying between them; and we may often observe that where the earthy deposit is advancing to invade the fibres, the recently and as yet imperfectly calcified bone with which they are continuous, presents a similar open and coarsely reticular structure; though the older, harder, and more opaque part is comparatively solid and compact. The appearance referred to is especially well seen at those places where a cross bridge of bone is being formed between two long spicula; we may there distinguish the clear soft fibres which have already stretched across the interval; and the dark granular opacity indicating the earthy deposit may be perceived advancing into them, and shading off gradually into their pellucid substance, without a precise limit.

"It thus appears that in the intra-membranous ossification, the growing bone shoots into the soft tissue, in form of transparent fibres, resembling those of fibrous texture, more or less intermixed with granular corpuscles; and that these fibres become charged with earthy salts. As to the cells or corpuscles, they certainly seem in some way involved in the ossification along with the fibres; but I am not able to say what precise share they have in the process. It has been supposed that they eventually form the lacunæ of the bone; but we shall enter upon this question afterwards.

"As the bone extends in circumference, it also increases in thickness; the vacuities between the bony spicula become narrowed or disappear; and at a more advanced period the tabular bones of the cranium are tolerably compact towards the centre, although their edges are still formed of slender radiating processes. At this time, also, numerous furrows are grooved on the surface of the bone in a similar radiating manner; and towards the centre these are continued into canals in the older and denser part, which run in the same direction. The canals, as well as the grooves, which become converted into canals, contain blood-vessels supported by processes of the investing membrane, which deposit concentric layers of bone within; and when thus surrounded with concentric laminæ, these tubular cavities are in fact the Haversian canals." (Dr. Quain's Anatomy, p. 151.)

This account of the first formation of osseous tissue harmonizes extremely well with the description already given of the fibrous texture of the animal basis left after the removal of the calcareous salts by acid; and it would seem, therefore, correct to say, of *this* form of osseous substance at any rate, that it is a fibrous tissue impregnated with calcareous salts, which have entered into chemical combination with the gelatine of which the fibres are composed. Dr. Carpenter has pointed out (Human Physiology, § 192,) the remarkable confirmation which this view derives from an examination of the skeletons of the Echinodermata; these being unquestionably composed of a *calcified areolar tissue*, which presents a striking resemblance to the first osseous network visible in the intra-

membranous process of ossification, and which differs from bone chiefly in having its development arrested at that point, so that it remains incapable of modification, and never passes on to any higher form of organization.

With respect to the early stages of the intra-cartilaginous form of ossification, we believe that there is no considerable difference of opinion amongst microscopists. A *vertical* section of the cartilage carried down to the plane of ossification, shows the cartilage-cells arranged in rows or oblong groups, between which the transparent matrix or intercellular substance appears in the form of clear longitudinal lines. The peculiar arrangement of the cartilage-cells is probably due to their rapid multiplication in the neighbourhood of the seat of ossification, which is copiously supplied with blood-vessels; each oblong group consisting of a row of cells, which seems to have been formed by the usual process of cell-growth, from a single individual. When a *transverse* section of the ossifying cartilage is made, parallel to the plane of ossification and at a short distance from it, we find that the matrix forms a network with large circular areolæ, which are occupied by the cartilage-cells, here in much nearer approximation than in ordinary cartilage. Now it is in the matrix or intercellular substance of the cartilage, that the first calcareous deposit takes place; and it is seen in a vertical section shooting upwards in thin cylindrical plates in the clear spaces of the cartilage, between the oblong groups of cells whose lower ends it envelopes; whilst in a transverse section, carried partly through the bone and partly through the cartilage, we can as distinctly trace the transition from the calcified to the simply chondrinous network of intercellular substance, as we can in intra-membranous ossification from the calcified to the purely gelatinous fibre. The first osseous tissue formed in cartilage, therefore, has, like that formed in membrane, an open areolar texture; but the areolæ at the junction of the bone and cartilage are deep pits or alveoli, into which the ends of the groups of cartilage-cells are received. At a short distance below the ossifying surface, however, where the process has advanced further towards the formation of the permanent bone, the state of things is very different. The first detailed account of the changes which the microscope enables us to trace, as we pass from the ossifying surface towards the perfect bone, was given in the valuable 'Physiological Anatomy' of Messrs. Todd and Bowman, whose statements were chiefly founded on the authority of Mr. Tomes; and the same account is repeated, with little variation in the article "Osseous Tissue," contributed by the latter gentleman to the 'Cyclopædia of Anatomy and Physiology.' Some important modifications have been introduced, however, by Dr. Sharpey; and as we here, also, find our own observations in accordance with his on almost every point, we shall regard his statements as conveying our own conclusions, except where the contrary is expressed.

"According to my own observations, the primary areolæ of bone open into one another by absorption of their intermediate walls, both laterally and longitudinally; they possibly expand to a certain extent, but it is mainly to their lateral confluence that the formation of the larger or what might be termed secondary cavities, which succeed them lower in the bone, is due. In the mean time the cartilage-cells have disappeared; and the bony cavities, as Mr. Tomes has pointed out, are filled with blastema, in which there are a few fibres and numerous granular corpuscles or cells resembling those seen in the intramembranous ossification; there are also many blood-vessels. In the end, some of the enlarged cavities remain to form the cancellated structure; while others, getting

more and more filled up with concentric lamellæ, become Haversian canals; although the Haversian canals of the compact sides of the bone, it may be remarked, chiefly arise in another way, as will be afterwards described. In many of these cavities, the walls of the coalesced primary areolæ may long be distinguished, like little arches, forming by their union a sort of festooned outline, within which the new bony laminæ are situated." (Quain's Anatomy, p. clvii.)

The primary osseous deposit, as Mr. Tomes correctly remarked, is distinctly granular in its character, and is entirely destitute of lacunæ or canaliculi. The larger part of this, as we have just seen, is of temporary duration only, being removed before any subsequent deposit of osseous matter takes place; and it is difficult to recognise any of it in the fully-formed bone. The secondary deposit is quite transparent, and of a more uniform homogeneous aspect; and in this the lacunæ and canaliculi are first distinguishable. As to the mode in which these peculiar excavations originate, the observations of Messrs. Todd and Bowman, Mr. Tomes, and Dr. Sharpey, do not agree. The first-named observers speak of the lacunæ of the earliest secondary layers of bone as being evidently the nuclei of the cartilage-cells; the layers themselves being composed of cells which have been consolidated by ossific impregnation. The cancelli and canals then become filled with a granular blastema, like that out of which all the tissues are evolved; and from this new cells are generated, which undergo a similar change with the original cartilage-cells, their cavity being filled with osseous matter, whilst their granular nuclei remain as the lacunæ. All subsequent production of bone is supposed by these authors (Physiological Anatomy, p. 120,) to take place through the intermediation of "cartilage-cells," developed on the vascular surface, which become ossified in successive layers, their nuclei remaining to form the lacunæ, from the margin of which the canaliculi are elongated. We have been unable to find any ground for the opinion that the original cartilage-cells have any participation whatever in the production of the secondary osseous deposit with its lacunæ and canaliculi; as they seem to us to disappear altogether from the time when the primary osseous areolar structure begins to break down into the larger cavities, previously to its formation. Still less can we admit that any cells which may present themselves at a later period in the osteogenic process, have a title to be called cartilage-cells. Our authors here seem to have been warped by the prevailing idea that all ossification takes place in the substance of cartilage; so that wherever true bony structure exists, there must have been a preceding development of cartilage. Mr. Tomes now regards the lacunæ of bone as themselves being cells; and he considers that the bony matter which intervenes between them is produced by the simple consolidation of a granular deposit contained in the medullary cavities, which includes and unites them. (Cyclop. of Anat. and Physiol., vol. iii, p. 856.) The following is Dr. Sharpey's account of the process:

"Although certain appearances render it not improbable that there may be a layer of flattened and calcified cells next to the surface of the granular bone, I am nevertheless disposed to think that the subsequent and chief part of the deposit results from the calcification of successive layers of fibres, generated in the blastema and possibly derived from the granular cells, some cells being perhaps also involved along with the fibres, as in the ossification of the flat bones of the cranium; in short, it appears to me that the deposit in question is formed after the manner of the intramembranous ossification already described. I infer that such

is the process, from the structure of the layers; for they are made up of fine reticulated fibres, like the lamellæ of perfect bone. On a careful inspection, and with a certain adjustment of the light, the little apertures of the canaliculi may be seen, and in many parts also fine striæ indicating the obliquely decussating fibres of the new-formed laminæ. The structure reminds us of the secondary deposit inside the oblong cells in the wood of coniferous trees, in which the ligneous matter is arranged in fibres, or rather in fine lines, running obliquely round the wall of the cell and crossing one another in alternate layers.

"The mode of production of the lacunæ, or so-called corpuscles of bone, is still an enigma in osteogeny, and I do not pretend to solve it. They are generally supposed to be derived from the cells of the soft tissue involved in the ossification by some sort of metamorphosis which has been variously conceived. Some suppose that the cell becomes the lacuna and sends out branches, like the pigment-cells, to form the canaliculi (Schwann). Others think that it is not the cell, but its nucleus, that undergoes this change, and that the substance of the nucleus is afterwards absorbed, leaving the lacuna (Todd and Bowman). Henle thinks that the lacuna is a cavity left in the centre of a cell which has been partially filled up by calcification, and that the canaliculi are branched passages, also left in consequence of the unequal deposition of the hard matter, as in the instance of the pore-cells of plants. As to this last opinion, it does not seem reconcileable with the structure of ordinary sound bone, and I am also led greatly to doubt whether the lacunæ and canaliculi are derived from cells or their nuclei in either of the two ways supposed. It has rather appeared to me as if they were little vacuities left in the tissue during the deposition of the reticular fibres, as open figures are left out in the weaving of some artificial fabrics (but not within a cell, as Henle imagined), and that thus the apposition of the minute apertures existing between the reticulations of the lamellæ gives rise to the canaliculi, in conformity with what has been already stated respecting their structure. At the same time it seems not unlikely that a cell or cell-nucleus may originally lie in the lacuna or central cavity, and may perhaps determine the place of its formation. Such is the view I feel disposed to take of the production of the lacunæ and canaliculi of ordinary bone, although I can by no means speak confidently on the point. In instances of what might be considered a more crude form of ossification, the mode is perhaps somewhat different. In the slow growth of bone, which encroaches on the attached surfaces of articular cartilages, the ossification would almost seem to be produced merely by the impregnation of the cartilaginous matrix with earthy matter (corresponding with the first step of the ordinary process,) and in this case the cells and clusters of cells being surrounded by the calcified matrix, may remain as little vacuities or lacunæ in the bone; but this, as well as the formation of lacunæ in the crusta petrosa of the teeth and the production of adventitious bony deposits in different textures, requires further investigation." (Quain's Anatomy, p. clix.)

With regard to the chief proposition here enunciated by Dr. Sharpey—that the osseous laminæ concentrically deposited within the Haversian canals have a fibrous tissue for their basis, and that this form of ossification is therefore essentially the same with that in which no cartilage has previously existed,—we are in full accordance with him. Whether the lacunæ are actual *cells* enveloped by the fibrous texture, or whether they are the spaces occupied by granular nuclei which do not undergo development into cells, is not perhaps a matter of much consequence; our own opinion leans to the latter view. But we cannot, with Dr. Sharpey, regard the canaliculi in the light of passages which *happen* to be left in the superposed laminæ of ossified fibres. Their whole arrangement is so regular, their inosculation with those of adjacent lamellæ is so remarkable, and their general distribution so characteristic, that we feel sure that their

presence must be in some way determined by that of the lacuna; and that, whether this last be cell or nucleus, it sends out radiating prolongations whilst the surrounding substance is yet soft, which prolongations remain as tubuli when that substance has undergone calcification. This view is perfectly consistent with Dr. Sharpey's observations already quoted, with respect to the relation of the perforations to the fibres of the animal basis; for if the fibrillation of these take place *after* the bone-cells or nuclei have sent out their prolongations, a thin lamella peeled off in a direction transverse to the passage of the canaliculi will naturally show these as minute apertures in the meshes of the fibrous network, the fibres passing around them and inclosing them in their reticulations as described by Dr. Sharpey.

The most important of all the doctrines newly advanced by Dr. Sharpey, and one as to which we can most completely confirm the observations on which it rests, is this,—that the increase in girth of a long bone, like the increase in thickness in a flat bone, takes place by the progressive calcification of the inner portion of the periosteum, and that no generation of cartilage precedes the development of new osseous tissue in this situation. It is obvious that if this be true, as our own observations assure us that it is, independently of the authority of so cautious and trustworthy a physiologist as Dr. Sharpey, the whole controversy regarding the power of the periosteum to produce bone is at an end. The shaft of a long bone, the femur for example, is increased many times in diameter, from the period when it is at first moulded in cartilage, to the full development of the adult bone; and during its growth, the whole of the first-formed bone undergoes removal, as we shall hereafter see, to make way for the central medullary cavity of the enlarged shaft. Consequently the whole of the hollow cylinder or dense shaft of the adult bone must have been at first formed in the periosteum, though it no doubt undergoes subsequent changes by the progressive filling-up of the Haversian canals. It cannot be supposed, then, that a vascular membrane which is the immediate instrument of the regular production of osseous texture, should be anything else than the most important agent in its regeneration. The following is Dr. Sharpey's description of the process:

"It has been sometimes supposed that a formation of cartilage precedes the bone also in this situation; but such is not the case, for the vascular soft tissue in immediate contact with the surface of the growing bone is not cartilage but consists of fibres and granular corpuscles; in fact, the increase takes place by intra-membranous ossification, and accordingly the Haversian canals of the shaft are formed in the same way as those of the tabular bones of the skull,—that is, the osseous matter is not only laid on in strata parallel to the surface, but is deposited around processes of the vascular membranous tissue which extend from the surface obliquely into the substance of the shaft; and the canals in which these vascular processes lie, becoming narrowed by the deposition of concentric osseous laminæ, eventually remain as the Haversian canals.

"That the ossification at the periosteal surface of the bone does not take place in cartilage, may also be made apparent in the following manner. Strip off the periosteum from the bone at the end of the shaft and from the adjoining cartilage also, taking care not to pull the latter away from the bone. A thin membranous layer will still remain, passing from the bone to the surface of the cartilage; now, take a thin slice from the surface, including this membrane with a very thin portion of the bone and of the cartilage, and examine it with the microscope, scraping off the cartilage from the inside if it be too thick. You will then see that the superficial part or shell of the bone, if it may be so called, is prolonged a little way

over the surface of the cartilage by means of pellucid coarsely-reticulated fibres of soft tissue into which the earthy deposit is advancing. These fibres are intermixed with granular corpuscles or cells, but form no part of the cartilage, and they are no doubt of the same nature as those seen in the intra-membranous ossification of the skull. Their reticulations are in most cases directed transversely, and sometimes they are little, if at all, in advance of the limit between the bone and cartilage. I have observed the structure here described in several bones of the (well advanced) fœtal sheep, also in the human scapula, humerus, femur, tibia and fibula, metacarpus and metatarsus, and it probably occurs in all the long bones." (Quain's Anatomy, p. 160.)

We have ourselves, in prosecuting our observations on the plan here suggested by Dr. Sharpey, been able in more than one instance to trace distinctly the continuity between the soft fibres of the internal periosteum, and the calcified fibres of the newly-forming bone; and we can now fully understand the rationale of the statement of Professor Goodsir, that it is impossible to separate the periosteum without removing along with it minute longitudinal, filamentary, or riband-shaped portions of the surface of the bone.

We thus perceive that microscopic research leads unequivocally to the following conclusions:—1. That true osseous tissue, with its characteristic lacunæ and canaliculi, may be formed from a membranous basis consisting of gelatinous fibres and interspersed cells. 2. That the osseous substance formed in cartilage is of an inferior character, being composed of an aggregation of granules without regular lacunæ or canaliculi; and that this is essentially the same, whether generated in the intercellular matrix of the temporary cartilage of the fœtus, where it constitutes the primary layers of bone, or in the substance of permanent cartilage during that process of abnormal consolidation which often takes place at a later period of life. 3. That the more perfect form of osseous tissue is always developed from a basis of fibres and interspersed cells, whether it is generated as a concentric deposit within the Haversian canals, or whether it is formed on the external surface of the bone. The development of new osseous tissue would seem to be ordinarily confined to these two situations.—There is no adequate reason to believe that bone enlarges by a process that can be properly called *interstitial* growth; or in other words, that the entire shaft swells by the deposit of new matter in every part of its substance. All our knowledge of its manner of increase would lead to the belief that its diameter is augmented by superficial deposit alone; its solidity being increased at a subsequent time by the narrowing of the Haversian canals, which is the result of the new osseous deposits concentrically made in their interior. The membrane lining the central medullary canal and cancelli, obviously belongs to the same category with the membranous lining of the Haversian canals and the periosteum, and is in fact continuous with these at many points; but it appears to be ordinarily concerned, rather in the absorption of bone than in its deposition, since we commonly find no new bone generated in the interior of the shaft or of the cancellated structure, but in its stead a positive removal of bony matter, in order that the enlargement of the cavities may keep pace with the general enlargement of the bone. We cannot help seeing, however, that the inner layer of periosteum, the lining membrane of the Haversian canals and cancelli, and the medullary membrane, all form one system, on which the whole development of the osseous substance depends, from the time when it

was first marked out by the lamellæ deposited in the cartilaginous matrix. When we speak of membrane, we would be understood to include the peculiar cells, which are found between the membrane and the bony substance itself in all these situations, so long as the growth of the bone continues. It is only in bones which have nearly or quite arrived at their full growth, that we find the medullary cavity, the cancelli, and the Haversian canals, filled with fat-cells instead of with formative blastema; and it is then, too, that a layer of fat is commonly deposited on the external surface of the bone beneath the periosteum. What special purpose is served by this deposit, does not appear evident. There is no reason whatever to believe that it has any relation with the nutrition of the bone, since we find the bones of most birds destitute of it. We can only attribute its occurrence to the general tendency which exists in the adult to the deposition of fat in situations where it may be stored up out of the way, and especially in receptacles which have been employed during the earlier age of the animal for some purpose more immediately connected with its nutrition and development.

We shall now pass from this investigation to a review of the 'Théorie Expérimentale' which has recently been put forwards as a great novelty by M. Flourens. We need not say that anything which comes from a physiologist so distinguished is worthy of respectful attention; and we have accordingly perused his treatise (chiefly consisting of Memoirs presented at various times to the Academy of Sciences, of which M. Flourens is perpetual secretary) with every desire to profit by it. The experiments whose results it contains, have been very numerous and ingeniously varied; most, if not all of them, must have been attended with a great amount of suffering to the unfortunate subjects of them; and yet we cannot find that they contribute to establish a single general fact or doctrine which had not been distinctly enunciated by his predecessors. And yet with the self-complacent egotism so common among his countrymen, but with which we should have hoped to find so distinguished a *savant* somewhat less imbued, M. Flourens refers in almost every page to "ma théorie" as if it were distinct from every one that had been previously broached, and were destined to supersede them all,—instead of being, in fact, the doctrine of John Hunter, without those extensions and modifications which a more intimate knowledge of the processes concerned in the development and growth of bone have enabled the physiologists of this country to introduce into it. M. Flourens seems to have made up his mind to pursue the inquiry in a single method only,—the experimental one. For him microscopists have laboured in vain. He scorns to avail himself of their results. Instead of following out the natural processes, whose course, when attentively studied, reveals to us almost everything we can reasonably expect to know, he puts Nature (literally as well as metaphorically) to the torture, and seeks to learn from her efforts and struggles that which it only needed care and patience to ascertain from her calm and regular operations. French physiologists have always appeared to us to have an essentially wrong idea of the proper purpose of experiment. They forget the difference between the laboratory of the chemical or physical philosopher, and the living organized body. In the former nothing takes place of itself; whatever we desire to see must be arranged for; every process must be *set* in operation, whether it be a simple act of precipitation or

evaporation, or whether it be a delicate and complex analysis, the results of which may be of the highest importance. But in the living body we have no need of this expenditure of labour in devising experiments, for every change that takes place under our scrutiny is in fact an experiment which Nature *has prepared* for us; and it is only after we have exhausted all the sources of information with which we are thus furnished, and find some points left in doubt on which a certain variation in the conditions of the phenomenon will probably enable us to satisfy ourselves, that we are justified (as it seems to us) either philosophically or morally in having recourse to this method of investigation. Our Gallic neighbours seem to have a natural turn for much that is repulsive to the feelings of an Englishman. They can starve multitudes of unfortunate animals to death with the same coolness that we should exercise in feeding them. They can lacerate the flesh, break or bore the bones, and dissect out the nerves, of a living animal, with as much nonchalance as we should feel towards a dead one. And they feel no compunction in repeating on any number of occasions, for the mere sake of display, experiments of such cruelty that we should have recourse to them only for the sake of acquiring information on some point of great importance which could not be otherwise settled, and should shrink from repeating them when no further instruction seemed likely to be derived from them. Perhaps this peculiar taste has had its advantages, in bringing to light a few facts which might not have been otherwise sought; but the discoveries to which it has contributed have been marvellously few, when compared with those which have resulted from what we deem a more philosophical method of inquiry into the secrets of Nature. The former researches of M. Flourens on the Functions of the Nervous Centres, are perhaps among the most valuable of all the contributions which the French experimental school has made to physiology; and yet we are much mistaken if the value of these be not hereafter estimated rather by their coincidence with the inductions founded upon extended anatomical inquiry, than by the apparent positiveness of their own indications.

In order that the position of the questions which M. Flourens has set himself to determine may be clearly understood, it will be necessary to revert for a brief space to the history of opinion regarding the growth and development of bone. The most ancient idea seems to have been, that the bones increased by *extension* in every direction, through the assimilation of nutritious fluid derived from the blood. It is not surprising that Clopton Havers should have sanctioned this view, when he traced the blood-vessels into the canals which bear his name, and thus showed how nourishment is supplied to every part of the bone. Duhamel was apparently led to investigate the subject, from an expectation that he should find something in the growth of bones analogous to the growth of trees by the addition of successive layers to their surface; and he was the first to propound the idea that the laminated structure of bone results from this mode of increase by *superposition*, the new layers being formed by the periosteum. Yet he does not seem to have entirely given up the notion that bone grows also by extension; this extension being produced by the *interposition* of new particles. At that time the idea of the possibility of the removal of bone already formed by the process of absorption, had not been entertained by physiologists; and there seemed no other way of

accounting for the enlargement of the medullary canal, but by the notion that the entire cylinder expands by interstitial growth. Duhamel strongly upheld the instrumentality of the periosteum, as the agent concerned in forming the laminæ successively added to the exterior of the bone; and on this and other points he was opposed by no less an authority than Haller, who maintained that the first formation of bone, whether in fœtal growth or in the production of callus, resulted from the consolidation of a gelatinous fluid, which first assumed the characters of cartilage, and then acquired those of bone by the ossifying process. Matters remained in this state when John Hunter took up the inquiry; and although the experiments which he seems to have made were few, and these imperfectly recorded, yet he introduced a new *idea* into the explanation of the process, which seems at once to have led to a considerable modification in the views then current on the subject, and which has left its impress on all subsequent opinion, in this country at least. This idea was the *removal by absorption* of osseous matter already deposited; so that whilst new matter was being added on the exterior, the form and proportions of the bone should remain the same. We unfortunately do not possess Hunter's own record of the experiments in question, or of his deductions from them. All our information on the subject is derived from a short paper drawn up by Sir Everard Home, which professes to give a summary of them. We need not say how much more satisfactory it would have been to have had Hunter's views expressed in his own terse and vigorous language. They are thus summed up by his successor:

"From these experiments he ascertained the changes which take place in bones during their growth and the readiness with which the materials of bone are absorbed; and from these facts laid it down as an established principle, that the absorbents are the agents by means of which the bones, during their growth, are modelled as it were and kept of the same shape. Bones, according to Mr. Hunter's doctrine, grow by two processes going on at the same time, and assisting each other; the arteries bring the supplies to the bone for its increase; the absorbents at the same time are employed in removing portions of the old bone, so as to give to the new the proper form. By these means the bone becomes larger, without having any material change produced in its external shape." (Hunter's Works, vol. iii, p. 318.)

Now although it is not here stated, in so many words, that the shaft of a bone grows in diameter by superposed layers, and that the medullary canal is enlarged by absorption, without any extension of the substance of the bone already formed, yet it is perfectly obvious that this would be Hunter's notion of the process in the particular case in question; it has been taught as Hunter's by British surgeons and physiologists from that time to the present; and it is clearly expressed by Professor Owen, who, in a note to the preceding passage, contrasts the Hunterian doctrine with that of Duhamel with express reference to the celebrated experiment of the latter, to which we shall presently make more particular reference.

"The difference between the Hunterian theory of the growth of bone, and that of Duhamel, will be readily appreciated by attending to the explanation given by Duhamel of the phenomena which he observed during his investigations on this subject. Let us, for example, select from the number of his ingenious and instructive experiments that in which he placed a ring of silver wire round the middle of the shaft of the thigh-bone of a young pigeon; and found at a subsequent period the ring in the medullary cavity of the bone, instead of embracing

the exterior of the shaft where he had placed it. It need scarcely be observed that the Hunterian physiologist would explain these facts by stating that the arteries of the periosteum had deposited new bone on the external surface of the ring, while the absorbents had removed the old bone in contact with the internal surface of the ring, by which its relations to the osseous parietes of the femur became reversed. But this physiological view of the phenomena, arising out of a knowledge of the powers and actions of great and important vascular systems in the frame, was wholly unsuspected by and unknown to the predecessors of Hunter. Duhamel explains the facts on mechanical principles; assuming that the bony layers of the shaft of the thigh-bone were expanded by the interposition of additional osseous matter, and that the layers were cut through in this process of expansion by the unyielding wire which he had placed around them." (Hunter's Works, vol. iv, p. 318.)

These remarks, published as much as ten years since, sufficiently indicate the sense in which the imperfectly-expressed views of Hunter have been understood by British surgeons and physiologists; and it would be easy, if it were necessary, to adduce a large amount of testimony to the same effect from other quarters. Whether the lymphatic division of the absorbent system is as largely concerned in the removal as Hunter and his disciples have supposed, or whether it is not rather by the veins that the osseous substance of the interior of the bone is carried off, is a question of secondary importance. The main point is, that Hunter distinctly enunciated the doctrine of the growth of bone by *superposition only*, in opposition to Duhamel, who conceived that, besides the superposition of new particles, there is an interstitial deposition by which the entire substance of the bone undergoes expansion.

We are utterly at a loss to understand, therefore, on what pretext M. Flourens claims the slightest trace of originality for this doctrine, and trumpets it forth thoughout his treatise as "ma théorie." We can only account for the circumstance by that extraordinary blindness to the doings of scientific men of other countries, which has gained for French *savans* so unenviable a notoriety. We have a "glaring instance" of this tendency in the fact that in a recent number of the 'Comptes Rendus' of the French Academy, the photographic process, which was published some years since by Mr. Fox Talbot under the name of the calotype, which has been subsequently most extensively used in this country, and of which specimens were submitted to the Academy two years since by the author through M. Biot, has been *republished as original;* the only variations from Mr. Talbot's account being in the most trifling improvements of detail, which had in fact been in common use in this country at least twelve months previously. That we do M. Flourens no injustice in imputing to him a most unfair neglect of Hunter, to say nothing of Duhamel, will we think be sufficiently obvious from the following passages, which form the commencement of his treatise.

"This work is composed of four parts.

"The first is the exposition, and, I venture to say, the demonstration, of my theory on the formation of bone.

"The second is the reproduction, much abridged, of my experiments already published on the coloration of bones by madder.

"The third is the discussion of the theories which have preceded mine.

"The fourth is the examination of the important and novel problem of the relation of force with matter in living bodies.

"This problem, now for the first time truly set forth in science, will doubtless call the attention of physiologists to my experiments and my views." (pp. 17-8.)

The preceding sentences constitute the advertisement; the first chapter of the work itself, entitled "My Theory and the Experiments which demonstrate it," commences as follows:

"My theory of the formation of bone rests on the six following propositions:
"1st. That bone is formed in the periosteum.
"2d. That a bone increases in thickness by superposed layers.
"3d. That it increases in length by juxtaposed layers.
"4th. That the medullary canal is enlarged by the absorption of the internal layers of the bone.
"5th. That the heads of bones are successively formed and removed by absorption, to be again re-formed in accordance with the growth of the bone.
"6th. That the *continual mutation* of matter is the great and wonderful spring of the development of bone." (p. 2.)

In regard to the first proposition, M. Flourens generously admits that he was anticipated by Duhamel; for that sagacious physiologist expresses himself on the subject in a way that cannot be misunderstood, even going so far as to term cartilage a thickened periosteum. M. Flourens further accords to Duhamel the merit of exactness in his experiments: "I have," he says, "repeated them all; and everything which he says that he has seen, I also have seen." But, he asks, why is it that Duhamel's opinion has not been generally admitted; or rather, to speak more correctly, why is it that, commencing with Haller, it has been combated by almost every physiologist? The reason, according to M. Flourens, is to be found in the fact, that in judging of the opinions of Duhamel, no care was taken to repeat his experiments; and that these experiments, although exact as far as they went, were not sufficient to establish his views. Now upon this we have to remark that, although Haller and some subsequent physiologists may have found it difficult to reconcile Duhamel's views with their own preformed opinions, yet all who have themselves appealed to Nature have arrived at the same conclusion with Duhamel: so that, as physiologists have shown an increasing tendency to give up hypotheses in favour of facts, and to adopt actualities instead of abstract notions, the truth of Duhamel's doctrine on this point has been more and more fully recognised, in this country at least; and there has been of late so general a conformity in the doctrine that bone can be formed through the instrumentality of the periosteum, that the result of Dr. Sharpey's microscopic inquiries has been felt to be rather an anticipated consequence of Duhamel's doctrine, than a supplemental proof required for the corroboration of the latter. Still we are far from asserting that M. Flourens has not obtained by his experiments additional proof of a position that was still disputable. Our complaint is of the degree of credit he has taken to himself for having done so. And as any testimony derived from experiments on this subject must necessarily be very much of one kind, we do not think *his* confirmation of Duhamel's doctrine nearly so well adapted to convince those who may remain sceptical, as the observation of the actual process of the conversion of the periosteum into bone, carried on after the method of Dr. Sharpey.

In order to put in the clearest light the "great fact" of the formation of bone in the periosteum, M. Flourens did not confine himself, like Duhamel, to the simple experiment of fracturing the first bone he came to—the tibia for example—and then watching the results. In all such

cases, the two ends of the bone are drawn together, or are even caused to override each other, by muscular contraction; and an effusion ensues, in the midst of which it is impossible to see clearly what subsequently takes place. "I say," observes our author, "to see clearly; for what is not seen clearly is not seen at all; and a great part of the art of experimenting consists in rendering the facts clear and precise." For the sake of avoiding the source of fallacy just alluded to, M. Flourens chose the ribs as the chief subjects of his experiments; since, their extremities being fixed, the divided ends are prevented from approximating when a portion of the bone has been removed. In these cases the periosteum that surrounded the portion of bone withdrawn, was left undisturbed; and the formation of new bone took place distinctly through its medium. It was found that the membrane first became greatly tumefied; that it then presented a cartilaginous structure; and that ossific deposits then began from centres or nuclei at points intermediate between the ends of the bone, gradually extending themselves so as to fill up the vacuity, and thus at last coming into contact with the original bone, at the cut extremities of which, according to M. Flourens, no new deposit is formed, so that it does not itself in any way participate in the reparative process. On this last point, however, other observers are at issue with him. In another series of experiments, M. Flourens aimed to give origin to a new bone, after the following extraordinary fashion: he drilled a hole in a bone, the tibia for example, and introduced into this hole a silver canula; he then partially detached a portion of periosteum from the surface, and carried it down this canula, so that it might come into contact with the medullary membrane of the interior of the bone; and the result was that this periosteum became thickened and cartilaginous, and then produced a new bone within the tube of the canula. Thus he considers that he has distinctly proved the fact of the power of the periosteum to produce bone. But we wonder that he should have been satisfied with this evidence, when the microscope, as Mr. Goodsir has shown, clearly reveals the impossibility of detaching the periosteum from a bone without carrying shreds of osseous tissue along with it; so that those who maintain that the periosteum alone cannot produce bone have a right to challenge M. Flourens to the proof that the periosteum alone was the agent in these cases.

The *second* proposition, that bone increases in thickness by superposed layers, is now so universally admitted that there appears to us still less ground for M. Flourens' enunciation of it as part of *his* theory; nor can we find that he has adduced a single new fact in its support. The experiment of placing a ring upon a growing bone, and watching its gradual inclosure by the newly-formed layers, until it is at last found within the medullary cavity, was first devised by Duhamel; and it has been so often repeated with the same result, and that result corresponds so precisely with other indications of the same truth, that M. Flourens might very well, we think, have spared himself the trouble, and the poor animals the pain, of its repetition.

The third proposition, that a bone increases in length, not by the growth of its entire substance, but by the formation of new osseous tissue near its extremities, is also, as our readers must be well aware, very far from possessing any claim to novelty; and the experiments upon which it is sustained by M. Flourens, are little more than a repetition of those of

Hales, Duhamel, and John Hunter. He seems, however, to have adduced additional evidence in support of the belief which had been formed upon other grounds, that the increase is chiefly effected by the progressive growth and ossification of the cartilage which continues to intervene between the shaft and the epiphyses of the bone, up to the period of its attaining its full development. This increase took place in M. Flourens' experiments, at both ends of the bone; but was rather greater at the upper than at the lower.

The enlargement of the medullary canal by the absorption of the inner layers of the bone, constitutes the *fourth* proposition. That the medullary canal increases in diameter with the shaft, so that in time it occupies a larger space than the entire solid bone originally did, is a matter of familiar observation; and it is proved, if proof were necessary, by the experiment of the ring, which, placed on the exterior of the very young bone, is found within the medullary cavity of the one more advanced in age. Duhamel seems to have thought that the hollow cylinder which forms the shaft, might expand by interstitial growth, and that the ring might cut through it (so to speak) the layers being successively divided by the ring from without inwards, and the section being then repaired; until all the layers had passed to the outside of it. But John Hunter in effect gave (as we have seen) the more philosophical explanation which is adopted by M. Flourens; namely, that the ring becomes imbedded by the formation of new layers upon the surface of the shaft, whilst the inner layers are removed by absorption; and M. Flourens has confirmed this view by a variation of the experiment, which was calculated to bring the question to an issue. Instead of employing a *ring* which presses and resists, and which might be imagined to cut through the dilating shaft, he introduced a thin lamina of platina beneath the periosteum, and found it (like the ring) covered by successive layers on its exterior, but brought nearer and nearer to the medullary cavity by the enlargement of the latter, until at last, from being applied to the outside of the shaft, it found its way to the interior of the cavity.

The *fifth* proposition enunciates that the heads of bones are successively formed and absorbed, to be again reformed with the progressive growth of the bone. M. Flourens does not tell us what he means by the term "têtes des os;" and if his statements apply to the epiphyses which are usually included under that designation, we apprehend that it will require modification. These epiphyses grow like the shaft of the bone; and as the great increase in length takes place between the epiphyses and the diaphyses, we apprehend that no greater amount of change need take place in them, than in the shaft itself. But from the manner in which M. Flourens states the case, it would almost seem as if he supposed that the heads of the bones had to undergo a continual absorption and renewal with the increase of the entire bone,—that which was once the head of the femur, for example, being changed in form so as to be incorporated with the shaft, and a new head being generated at its extremity. This must of course be the case with regard to processes which have no separate centres of ossification, such as the spine of the tibia, on which M. Flourens' experiments were made; but we apprehend that the epiphyses follow altogether a different rule, since they are for the time independent bones, and the increase of the shaft in length does *not* draw them into itself, but takes place at the expense of the intervening cartilage.

The *sixth* proposition is couched in the following grandiloquent terms. "The continual mutation of matter is the great and marvellous spring of the development of bones;" and this spring or source, we are presently told, was previously unknown. Possibly, it was a novel idea to M. Flourens; but we cannot see in what other light the experiments of Hales, Duhamel, Hunter, and their successors could be viewed, when interpreted by the light of modern physiology, which sees in this continual interchange only one example of what is constantly taking place throughout the body.

The second chapter is devoted to the truth of the "identity of the medullary membrane with the periosteum." M. Flourens first adverts to the experiments of Troja, who, having amputated a limb, destroyed the medullary membrane of the remaining bone, and then found that the old bone died and that a new bone was formed around it. An objection may be raised to the validity of any inferences drawn from such experiments, as to the functions of the medullary membrane; since the transverse division of the bone, and the removal of the lower part of the limb so much alters the condition of the remainder. Our author, therefore, adopted a different plan of operation; making a simple perforation in the bone, and destroying the medullary membrane of the entire cavity by means of a stylet introduced through the aperture. He found that an entirely new bone was then produced around the old one, which remained quite detached within it, undergoing gradual absorption from its external surface. He ascertained, further, that where the new bone had not begun to be developed, that which would afterwards become its medullary membrane was, in fact, the inner layer of the thickened periosteum. And he found strong reason to believe that the absorption of the old bone inclosed within this, was effected by the inner surface of the new medullary membrane; for this membrane presented a series of mamillary elevations and depressions, which correspond precisely with depressions and elevations on the surface of the bone, giving it an *eroded* aspect which seemed to be the result of the absorption of its material by the surface-action of the membrane. Not content with this method of proof, M. Flourens bored a hole in the tibia of a dog, and introduced into the medullary cavity of this bone one of the smaller ribs of a rabbit. He found that the medullary membrane became tumefied; and that the rib (as ascertained by numerous experiments of different duration) was gradually removed by absorption, until at last it disappeared almost entirely. Hence he concludes that the medullary membrane has a special power of *absorbing* bone. But he shows that this membrane has also the power of *forming* bone, by destroying the periosteum; in which case the shaft (or at least its outer layers) die, and new bone is formed in the medullary canal. We cannot refrain, however, from again expressing our astonishment that M. Flourens should have regarded either the experiment or the conclusion drawn from it as possessed of sufficient novelty to form part of *his* theory. He goes on to show that the removal of the old inclosing shaft, in this instance, is effected by a new periosteum, which grows over it without attaching itself; but, on the contrary, has the same kind of eroding action as the medullary membrane in the former case; and that, having eaten through the dead bone (so to speak) in various parts, it goes to attach itself to the surface of the new bone which this incloses, and, finally, becomes its proper periosteum. He further demonstrates the absorbing power of the

periosteum, by introducing laminæ of an extraneous bone beneath the periosteum of a living bone; these gradually disappeared by the surface-action of the periosteum, just as in the former instance by that of the medullary membrane. From these and other considerations, it is concluded by M. Flourens, that the periosteum and medullary membrane are one and the same, the medullary membrane being in fact an internal periosteum;—a conclusion, be it observed, at which those who have been engaged in studying the minute structure of bone, and the mode of its development and continued nutrition, had arrived long since.

The Third Chapter relates to the formation of callus, which, according to M. Flourens, takes place in the periosteum, and nowhere but in the periosteum. The first of these propositions was sustained by Duhamel; and we do not find any new evidence in its favour in the present work. The second appears to us too exclusive; for although we are quite ready to admit the fallacy of the old notions that the callus is formed by the consolidation of effused fluid, or by a deposit from the extremities of the fractured bone, yet we cannot see any adequate reason for excluding the other membranous structures, which (on M. Flourens' own showing) form one and the same system with the periosteum, from a share in the process in cases in which they do not undergo the usual destruction or laceration. If the entire treatise had not shown a marvellous ignorance of the present state of physiological knowledge, we should have found a good subject for special animadversion in the following passage, wherein M. Flourens enunciates what he seems to regard as a great discovery:

"Thus at last seen, for the first time perhaps, in its true light, the reunion of fractures by the formation of callus is no longer anything peculiar, exceptional, and mysterious in physiology.

"Callus is bone and nothing but bone; and is bone which is formed where all bone is formed,—that is, in the periosteum.

"The formation of callus is then but a particular instance of the general and ordinary process of the formation of bone." (p. 65.)

Although this statement is perhaps somewhat more formal and dogmatic than those to which we have been accustomed, yet it seems to us in no essential degree an advance upon the previous state of our knowledge; since all inquiry has tended to show that the reparative processes (wherever they are complete) take place on the same plan as the first development of the tissues which exhibit them: and the evidence of this has been peculiarly satisfactory in regard to bone, and does not seem to us in any way strengthened by the coarse experiments and observations of M. Flourens.

The Fourth Chapter consists of nothing else than a repetition of the statement, in a different form, of the fact that the periosteum has the power of reproducing bone, that it may regenerate any part or even the whole of a bone, and that if it be destroyed it is first regenerated and then reproduces bone. Our author shows in this and several other parts of his work a marvellous gift at *reproducing himself*. The following is his concluding summary:

"The periosteum is then the material, the organ, the *stuff*, which serves for all these wonderful reproductions.

"The periosteum is the organ which produces bones and reproduces them;

and there is no other part of the animal economy which enjoys in so high a degree the faculty of reproducing itself.

"Some days are sufficient for its regeneration; and this regeneration is indispensable.

"A portion of the periosteum may be removed, and it reproduces itself; still more may be removed, and still more is regenerated; and so on.

"And now, after having brought into full view, after having demonstrated by so many different experiments, the surprising faculty, previously so little known (!) which bones possess of reproducing themselves, may I not be allowed to hope that this marvellous power will soon be a new agent in the hands of surgery? Without doubt. For I address myself to surgeons who observe, who think, and who do not see in surgery a mere routine method, but a science, a noble science; and who, even above that science, see humanity." (p. 72.)

We cannot depart from the strict verbal rendering of this grandiloquent passage without paraphrasing it altogether. We are totally at a loss to discover what are the new facts by which M. Flourens considers that he has given new powers to the surgeon. The ordinary course of disease has long since furnished examples in plenty of every principle which he advocates; and we do not see what can go beyond the reparative power which regenerates an entire bone, when the original one has been destroyed by necrosis,—a fact with which every surgeon is familiar, if not within his own experience, at any rate as an occurrence sufficiently common to find a place in every system of surgery.

The second part of the work contains the results of the madder-experiments performed by M. Flourens; and here, as elsewhere, we find marvellously little notice taken of the labours of his predecessors in the same line of inquiry; Belchier, the first discoverer of the peculiar affinity of this colouring-matter for the osseous texture, and Duhamel being the only authors referred to. Although Duhamel at first misinterpreted the results of these experiments, believing that the colourless rings of bone found after alternations of the madder-regimen with ordinary diet, were produced by the absorption of the colouring matter, he afterwards saw them in their true light, and perceived that the colourless rings were those which were formed during the intermission of the madder-regimen. We cannot see, therefore, on what ground M. Flourens thinks that by adopting this view (which was confirmed by Hunter and others) he has in any way contributed to the advance of science. Even the power of distinguishing by the use of madder not merely the additions made during a given time to the shaft of a bone, but also those made to its extremities, which is claimed by M. Flourens as a new observation, was distinctly indicated by John Hunter. He seems rather perplexed by the fact that the layer in immediate contact with the medullary cavity is often reddened by the same regimen as that which colours a layer that is being formed beneath the periosteum; and he also notices, without attempting to explain the circumstance, that the so-called colourless layers are seen with the microscope to exhibit traces of the reddening influence. These facts are easily understood, if the mode in which the osseous texture receives its nourishment be properly kept in mind. The vessels of bone are distributed on its external surface, on its internal surface, and through its small medullary canals and cancelli. In a very young bone, owing to the smallness of the shaft, and the openness of the Haversian canals, none of the osseous texture is far removed from the vascular surface; and the entire substance receives the madder-

tinge, within a day or even a few hours of the ingestion of that substance with the food. In the adult bone, on the contrary, it is difficult to produce any decided effect with madder. The thickness of the shaft withdraws the chief part of it from direct communication either with the periosteum or the medullary membrane; and the Haversian canals are so blocked up by the formation of concentric layers of bone, that the amount of vascular surface they afford is very small. The coloration of adult bones by madder is therefore confined to the portions in immediate contiguity with the periosteum and medullary membrane, and to the innermost of the concentric rings surrounding the Haversian canals; and in this last position it cannot be detected without the aid of a microscope. Now when a growing but not very young animal is fed with madder for a time, the layer of previously-formed bone in immediate contact with the medullary membrane will receive a tinge from it, and this may be so deep as to form a definite circle round the medullary cavity. At the same time a series of isolated deposits of the same kind will take place round the Haversian canals of the bone already formed; but these, although discernible with the microscope, will not give to the transverse section a tint visible to the naked eye. It will be in the new bone alone, formed beneath the periosteum during the continuance of the madder-regimen, that the entire osseous texture will be permeated by the colouring-matter.

The historical summary of opinions, which M. Flourens professes to give in his third part is meagre in the extreme. It takes cognisance of little else than the opinions of Duhamel and the objections of Haller; and although it includes a notice of the *experiments* of John Hunter, it says not a word of the *idea*, first applied by him to the interpretation of those and similar experiments, that the enlargement of the medullary canal is due to the absorption of the first-deposited bone.

We do not find it necessary to dwell on the "Examination of the relations between force and matter in living bodies," with which the treatise concludes; since nothing can possibly be more paltry than the manner in which the Perpetual Secretary of the French Academy here endeavours to gain the credit of originality by a mere piece of clap-trap. Our readers will scarcely believe us when we tell them that the only *idea* in the chapter thus grandiloquently headed is the following;—that as there is a continual mutation of matter in the living body, whilst the vital forces are comparitively unchanged, the *law* which fixes the relation of forces and matter in the living body is on the one part the *permanence of forces* and on the other the *mutation of matter*. We must own ourselves unable to perceive the novelty of either of the assertions embraced in this statement; as well as to understand how their juxtaposition can constitute a *law*. But upon the logic of science, our neighbours have always seemed to us to have very imperfect notions. Will it be credited, however, that M. Flourens assumes to himself the merit of *proving* the mutation of the materials of the living body?—a doctrine, he says, which with Buffon and Cuvier was the fruit of abstract meditation only. Truly he must be a man capable of entertaining but one class of ideas at a time; and that idea for the present is—*bone*. "There is nothing like it for proving everything; and everybody who did anything in the matter before me, did it so imperfectly that I must do it all over again and make it my own." This is a notion

which seems to have possessed the mind of M. Flourens for some years past; and unless he redeems his character by a series of researches prosecuted in a more philosophical and less egotistical spirit, we shall be obliged to believe that the Perpetual Secretaryship of the French Academy has put an extinguisher upon all that was valuable in one of the most eminent of the French school of physiologists.

Art. X.

Der Winterschlaf nach seinen Erscheinungen im Thierreich dargestellt von Dr. H. C. L. Barkow, ordentlichen Professor der Medicin und Director des Anatomie-Instituts an der königlichen Universität zu Breslau. Mit 4 Steintafeln.—*Berlin*, 1846.

Hybernation considered in its Phenomena throughout the Animal Kingdom. By Dr. H. C. L. Barkow, Ordinary Professor of Medicine and Director of the Anatomical Establishment in the Royal University of Breslau. With Four Lithographic Plates.—*Berlin*, 1846. 8vo, pp. 525.

This work contains a copious account of the phenomena of hybernation as it occurs throughout the animal kingdom, derived partly from the researches of preceding authors on the subject, but principally from the author's own observations, which have been very numerous, especially on the hybernation of snails, toads, hedgehogs, and marmots. Indeed, it is the number of his observations and the details he enters into in recording them, that has lengthened out the book to so many pages.

We cannot afford room for any elaborate analysis of the work, but must content ourselves with laying before the reader the following notices regarding the circumstances in the history of hybernation, most interesting in a physiological point of view.

There is no example of hybernation among birds, but among all the other classes of vertebrata and among the invertebrata there are examples. The duration of winter-sleep does not appear to be in any way connected with the class to which the animal belongs, for in animals of the same class it may be of long as well as of short duration, continued or interrupted. And, though in animals of the same species it is the same, there is probably some difference, according to age; seeing, for example, that Barkow found the winter sleep of younger individuals among hedgehogs in general shorter than that of older individuals.

The temperature of hybernating mammiferous animals, whilst in the waking state, is known not to be less than that of other mammifera, but, in common everyday sleep, Dr. Barkow says, that their temperature has a tendency to fall. During their winter-sleep the temperature exceeds little, or not at all, that of the atmosphere. The temperature is in the inverse ratio of the deepness of the sleep, being higher in the less degrees, and lower in the higher degrees. When cold weather is protracted the animals awake, and, if the cold be not severe, remain so; but, if the cold is severe, Dr. Barkow found that they became lethargic, stiff, and cold, and that they at last died.

Respiration during winter sleep is very slow, or may be wholly sus-

pended. In the latter case *circulation*, however, still goes on, the action of the heart, though much diminished, not entirely stopping.

The following is the result of Dr. Barkow's observations on the blood of the hedgehog during winter sleep: Coagulation takes place slowly, as does also the separation of the clot and serum, and putrefaction is longer of taking place. In these respects the blood of the arteries (which is of a dark colour), is more remarkable than that of the veins; and the blood of old animals is more remarkable than that of young, arterial being compared with arterial blood and venous with venous; but the opposite is the case, if we compare the arterial blood of young animals with the venous blood of old.

Our author's observations agree with those of Dr. M. Hall, as to irritability being retained longer by the muscles in animals killed during winter sleep than when in the waking state; and as to the excitability of the parts of the nervous system engaged in the production of the reflex movements remaining.

Some animals, whose winter sleep is incomplete, such as the badger, the hamster perhaps, &c., do not entirely cease taking food, but by far the greater number of winter sleepers take no nourishment. When awakened by exposure to a warm temperature, Dr. Barkow found that they have in general a disinclination to eat, and if they do eat, the quantity taken is small, and in some it is imperfectly digested. Dr. Barkow also found that when the awakened state was kept up the animal died.

Causes and nature of winter sleep. Winter sleep occurs in animals with the most different forms and degrees of development of the nervous system, and with the most different forms of the vascular system. Nor in the blood is there any peculiarity which can be considered as a cause of winter sleep. Barkow thinks that imperfect respiration is the principal condition of winter sleep. But it may be asked, what is the cause of this imperfect respiration but a diminution or suspension of the respiratory movements, occasioned by a diminution or suspension of the nervous influence to the respiratory muscles; this diminution or suspension of nervous influence to the respiratory muscles being the result of some change of state of the nervous centre induced or promoted by diminution of external temperature?

Dr. Barkow found that, during winter sleep, young toads and young hedgehogs do not grow, and that regeneration of lost parts in snails is arrested. Retardation, he says, is the general character of the organic as well as of the animal functions. The fully developed animal passes slowly into winter sleep, and comes forth from it in a state of rejuvenescence. The tame marmot is found to have forgot its education and to have become wild again.

Winter sleep, Dr. Barkow affirms, is not of the nature of asphyxia. Hedgehogs in a state of winter sleep, if kept a long time under water, become asphyxiated, but the animal does not pass directly from the state of winter sleep to that of asphyxia, as from a lower to a higher degree of one and the same state, but first awakes, though imperfectly, and endeavours to escape. Again, the asphyxiated animal does not, on the return of life, pass through the state of winter sleep into the waking state, but directly into the latter, and only again falls into the winter sleep from the waking state. Nor has winter sleep any analogy, Barkow says, to the

embryonic state, seeing that in this, development is most active, whilst in that, the activity of development is arrested.

Winter sleep differs essentially from common sleep. The most important differences between the two refer to digestion, growth, nutrition, reproduction, and formation of fat, almost all secretions, circulation, respiration, and evolution of heat. In common sleep, chymification goes on unchanged. Only at the beginning of incomplete winter sleep, or on interruption of it, does digestion go on. In perfect winter sleep the stomach is empty. If animals, with their stomachs full, are surprised by the sudden occurrence of cold, and put into winter sleep too soon, chymification stops. The common sleep favours nourishment, growth, and reproduction; winter sleep stops these processes. The common sleep of winter-sleepers powerfully promotes the formation of fat; during their winter sleep the fat is exhausted.

Art. XI.

A Treatise on the Structure, Diseases, and Injuries of the Blood-Vessels. By Edwards Crisp.—*London*, 1847. 8vo, pp. 354. With Plates.

The subject selected by the Council of the Royal College of Surgeons for the Jacksonian Prize for 1844, was "the Structure and Diseases of the larger Blood-vessels." Mr. Crisp was the successful competitor, and the present treatise contains the substance of his Prize Essay. More than two years, however, having elapsed since this was written, Mr. Crisp states that he has been enabled by personal observation and the recorded experience of others, to obtain much additional information. He has likewise taken a more comprehensive view of the subject, and treated of injuries as well as diseases of the blood-vessels; in this way the work has been so much enlarged, that he says it is due to the adjudicators to state that they are not in any way responsible for any errors that may have occurred by the introduction of new matter. In treating of the structural and pathological conditions of the blood-vessels, Mr. Crisp states that he has given the results of his own observations, and has not drawn any inferences without careful investigation; and that, being of opinion that the statistical is the only sure method of arriving at accurate deductions in medical inquiries, he has constructed a table of all the cases of spontaneous aneurism that have been reported in the British Journals from the year 1785 up to the present time. The analysis of this table, comprising 551 cases, furnishes the basis of the greater part of our author's remarks on aneurism. The work likewise contains the histories of cases bearing on the subject of which it treats, that have either occurred in the author's practice, or that of his friends, or that have been seen by him at the Borough Hospitals. We shall now proceed to lay before our readers an analysis of the more novel or interesting matters contained in the work itself.

Structure of Arteries and Veins. Mr. Crisp, in his views of the structure of arteries, differs from Henle in only recognising four instead of six coats; viz. 1, the internal or serous; 2, the sub-serous, or tunica cellulosa of Haller, or the sclerous of Malgaigne; 3, the fibrous; and lastly, the external cellular. Mr. Crisp has not been able to determine the existence of the epithelial lining, and the broad longitudinal fibres as described by

Henle, from whom he likewise differs in not admitting the muscularity of arteries, or the existence in them of fibres analogous to the unstriped muscular fibre of organic life. He confesses, however, very candidly that his experience in the use of the microscope is not such as to entitle his opinion to much weight. And certainly, putting all microscopical evidence aside, physiological facts are strongly in favour of the correctness of Henle's views, which, in a remarkable manner, bear out John Hunter's opinions with regard to the irritability of the smaller arteries. Mr. Crisp is in error where he says that most anatomists now believe, in opposition to the views of John Hunter, Sömmering, John Thomson, and others, in the non-muscularity of arteries. This may be the case with the larger vessels, to which Mr. Crisp appears principally to have directed his attention, but is certainly not the prevailing opinion with regard to the smaller arterial ramifications. Henle has shown that the arterial tunic composed of flat, clear, granular fibres, resembling the muscular fibres of organic life, is peculiarly developed in the smaller arteries, which possess a distributive power over their contents, and which Hunter proved to be endowed with irritability, whereas the genuine yellow elastic tissue is principally met with in the larger vessels, which are distinguished by their elasticity, which is necessary to sustain the force of the heart's action where it is strongest, and which gradually decreases with the size of the artery.

Mr. Crisp relates some experiments that he has performed upon the arteries of man and the lower animals, with the view of ascertaining the amount of physical elasticity which they possess, the following are the conclusions to which he arrives:

"1. That the fibrous coat of the arteries and veins is highly elastic; that it possesses no properties analogous to muscular fibre; that this tissue differs from all other structures of the body, except the ligamentum nuchæ and ligamentum subflavum, as regards its amount of elasticity.

"2. That the elastic property is the same in every part of the fibrous coat of the larger arteries.

"3. That the arteries of women are rather more elastic than those of men; and that the elastic property generally diminishes after the age of forty-five or fifty.

"4. That the elasticity of an artery decreases with its size.

"5. That the veins are in a slight degree less elastic than the arteries.

"Lastly. That the cellular coat of an artery possesses but little, if any, elasticity." (p. 7.)

The next chapter is devoted to various *Morbid conditions of the Arteries.* The principal question discussed, is "How are the different forms of redness to be distinguished?" On the solution of this question, our author throws no new light whatever. He adopts the usual division of these discolorations into the inflammatory, the congestive, and redness from imbibition, but does not appear to draw any distinction between the two last-named kinds of discoloration, which are no doubt, as Gendrin has proved, merely the result of post-mortem changes in patients whose energies have, during life, been depressed, and in whom the blood is in a morbid condition.

Arteritis. Our knowledge of this disease is as yet imperfect. For this there are two reasons: first, the great difficulty of distinguishing inflammatory redness of the serous and sub-serous coats from simple imbibition; and, secondly, the obscurity that, in the majority of cases, attends its symptoms during life. After giving, in a cursory and not very complete

manner, the general cause and varieties of this disease, the author proceeds to consider it as it affects the aorta and as it attacks the arteries of the extremities.

If some degree of obscurity shrouds the subject of inflammation of the arteries in general, it is increased when we examine this disease as affecting *the aorta;*—the products of inflammation—lymph, pus, and coagula—being but seldom found in that vessel, and thickening of its coats not being readily appreciable except when extending to the valves. This obscurity, presented by the symptoms of aortitis, has been well pointed out by Dr. Hope, who has shown that it is almost impossible to recognise it during life as an abstract disease, on account of the invariable complication of endocarditis, by which its symptoms are, to use his expression, absorbed. Indeed many of the so-called cases of aortitis on record will prove, on a closer examination, merely to be instances of endocarditis—of this description, for example, are the cases mentioned by Bertin. Mr. Crisp mentions having seen three cases of what he believes to be aortitis, but as only one of these patients died, and in him no post-mortem examination was made, but little value can be placed in them. A table is given of seventeen cases in which the aorta was obliterated during life; on looking over the details of these, it is curious to observe how little inconvenience most of the individuals suffered from this abnormal condition of the aorta. One man lived to the age of 93, and four others died above the age of fifty. Two patients died from rupture of the ascending aorta, another from rupture of the left ventricle, and a fourth from rupture of the right auricle. In the greater number of the cases, the aorta was obliterated or contracted near the point at which the ductus arteriosus opened into it. This liability to obliteration or contraction of the vessel at this part, was first pointed out by Dr. Craigie, and is well illustrated by the cases Mr. Crisp has collected.

Inflammation of the arteries of the extremities. In these cases the diagnosis, although obscure, is not so difficult as when the vessel is situated so deeply as the aorta is. There are pains of a burning, pricking, or shooting character, with great tenderness on pressure along the course of the affected vessel. These symptoms are often followed by a cessation of pulsation in the inflamed artery, and a tendency to gangrene, especially when occurring in the lower limbs. In some cases the vessel is hard and cord-like, and there is always more or less febrile excitement. Mr. Crisp has never met with inordinate pulsation as described by some authors. In old people the inflammation is most generally subacute, the pain less severe, the pyrexia not so great, but the tendency to gangrene more manifest. Mr. Crisp is a firm advocate of the doctrine that the gangrene of the legs of old people is the result of arteritis, and gives numerous cases in support of this opinion.

"Some high authorities believe that arteritis is not present in the dry gangrene of old people. I have paid much attention to this complaint for many years, and have had an opportunity of inspecting the bodies of several who have died of it. I have invariably found the lining membrane of the artery of a red appearance, the vessel often obstructed by fibrinous coagulum, some distance above the gangrenous part, as well as in the arteries near the seat of the disease. The inflammatory redness is not present in the preparations alluded to, there is, however, I think, abundant evidence to show that it existed.....

"How often the aged are affected with cutaneous inflammation of the legs,

and, reasoning from analogy, why, let me ask, may not the internal coats of the arteries be subject to the same engorgements? We have, I think, abundant evidence to show that ossification alone is not sufficient to produce the disease, as this deposit exists to a great extent in the majority of individuals who have attained an advanced age. The most common of the exciting causes of acute arteritis in the aged is, I believe, the detachment of a portion of the bony plate of the artery. The great toe, a part the most exposed to mechanical injury, is often the first affected. This accident I conceive, may occur at any part of the vessel, and its effect will probably be the production of inflammation, plugging of the collateral branches, and the consequent death of the parts below the seat of obstruction." (pp. 33-4.)

It may, however, be questioned whether this deposition of fibrine, and evidence of inflammatory action within the vessels, is a proof that the primary disease begins in them. May it not, in many cases, be the consequence of an obstruction to the passage of the blood through the capillaries. The death in the extreme parts being necessarily followed by deposition of fibrine in the larger vessels leading to those parts. In all cases of mortification, from whatever cause, those vessels that lead to the sphacelated part are blocked up by fibrinous exudation, as a safeguard against the occurrence of hemorrhage in the separation of the sloughs, and are usually found to present more or less of that inflammatory action, which has given rise to the deposition of lymph, and the obliteration of the vessel.

Mr. Crisp's account of *the treatment* of arteritis is somewhat unsatisfactory; but we know not that it could be otherwise. When the disease affects young subjects, he recommends local bloodletting and mercurials. In our opinion, the latter are very badly borne indeed in these cases, unless combined with large doses of opium. In old people our author recommends opium, and a mild unstimulating diet, with attention to the position of the limb. Hot fomentations, linseed meal, yeast, or beer-ground poultices. He makes no mention of a local application that is now very generally employed, and which we have always found much more cleanly and efficient than any moist application, namely, enveloping the limb in carded wool, or cotton wadding, retained by means of a silk handkerchief, and not opened more frequently than is absolutely necessary, in order to dress the sloughy or ulcerated parts.

The following chapters on "*Arterial deposits,*" and on the "*Diseases of the Aortic Valves,*" contain very little that is new or of any interest; we shall accordingly pass them by.

Abdominal pulsation. These cases, the author states, may very conveniently be divided into three classes. The first are those depending upon constitutional causes, such as chlorosis, hysteria, anemia from loss of blood, &c., in short, any condition of the system inducing an impoverished condition of the circulating fluid, or derangement of the nervous functions. In the second variety, mechanical obstruction appears to be the chief cause of the pulsation. The most frequent of the mechanical causes, are tumours of various kinds pressing upon the vessel, such as enlarged pancreas or spleen, scirrhous stomach, diseased mesenteric glands, collections of air and scybalous matter in the bowels. Allan Burns likewise mentions solidification of the lower lobes of the lungs, dilated heart, and enlarged vena cava. In the last, and, according to Mr. Crisp, the most frequent form, the aorta appears to be sympathetically affected. Dr.

Baillie supposed that this depended upon a disordered state of the digestive organs, and that it occurred more frequently in the male sex. Mr. Crisp, however, finds that this opinion is incorrect, as the majority of recorded cases have occurred in females. Mr. Crisp agrees with Drs. Fausset and Stokes, that stomach or intestinal affection is generally the exciting cause of this malady; yet he believes that it can only take place when the parietes of the vessel are in a weakened condition; this probably depending upon structural defect, or the derangement of nervous influence.

"It is difficult to imagine that local pulsation can arise from nervous excitability alone, or that the upper part of the aorta can be morbidly excited without the lower portion being in a similar state; if, however, the coats of the artery, from want of innervation or structural peculiarity lose their tone and possess greater tenuity and expansibility in certain parts, the phenomenon is more readily explained." (p. 97.)

The diagnosis of these cases from aneurism, is sometimes attended with difficulty, especially so in those cases in which the pulsation is limited to a small portion of the arterial tube; on this subject, however, our author adds nothing to the signs that had been pointed out by Dr. Hope some years ago, and omits to mention the very valuable diagnostic symptoms to which Dr. Graves was the first to direct the attention of the profession, namely, that the murmur will sometimes occur in the horizontal position in these cases, when from hydrostatic pressure it ceases in the erect.

Aneurism. The section devoted to this subject is the most important and complete of the work. As has already been stated, the materials of which this portion of the treatise is composed, are derived from the analysis of a table of 551 cases of aneurism that have been reported in the British Journals during the last sixty years. After making a few general remarks on the nomenclature, divisions, and comparative frequency of aneurism in different situations, our author proceeds to a consideration of the causes of this disease.

"It will naturally be asked why aneurism is more frequent in one artery than in another? But if we take into account the form, size, and anatomical bearings of the different arteries, the matter I think admits of a ready explanation. Thus, the ascending and right side of the aorta is the most common seat, and here the impulse of the blood is the strongest; especially during sudden and violent contractions of the ventricle. Next in order, as to frequency, comes the arch of the aorta, a part also subject to the blood's impulse, and probably somewhat weakened by the giving off of the large branches; the disease does not so often occur in the descending thoracic, and when it does, it is generally immediately below the arch where the parietes of the vessel are more acted upon by the current of the blood than in the straight portion. In the abdominal aorta, the most common situation is near the cæliac axis, where several large branches are given off, and below, which part the aorta diminishes in size. The common iliac, which is straight and gives off no branches, is rarely affected; and the same remark applies to the external and internal iliac, the branches from the former are very small, and the latter vessel is short and terminates in numerous divisions. The upper portion of the femoral artery is often affected with aneurism. The branches arising from it, the frequent motion of the thigh, and the greater liability of the vessel in this situation to external violence, are probably the chief causes. The middle third of the femoral is seldom subject to this disease, whilst the popliteal, which forms a curve over the joint, gives off several articular branches, is much exposed to violence, and probably to elongation in sudden extension of the leg, is very frequently aneurismal. The disease appears to occur in the branches of the arch in

nearly similar proportions; but those on the right side are rather more subject to it. It is difficult to assign a reason for the exemption of the brachial artery from this lesion. The most likely is the looseness of its connexions, and the curvature of the axillary artery, which allows of a certain amount of elongation, and thereby prevents the stretching which probably occurs under certain circumstances in the popliteal. In reviewing the foregoing localities of aneurism, this inquiry naturally presents itself, viz. are the arteries in these situations more subject to disease? After a very careful investigation, I have come to the conclusion that the various deposits are less frequent in the arteries most affected with aneurism, for example, the ascending aorta, and popliteal arteries (the most common seats of aneurism) are not oftener diseased than the descending thoracic, the lower part of the abdominal aorta, and the iliacs, where the disease is comparatively rare. I must therefore infer that these deposits are less concerned in the production of aneurism than is generally supposed, and that mechanical causes afford a more probable explanation." (pp. 114-15.)

Mr. Crisp finds that, including all the cases of aneurism reported in his table, the proportion of females is rather less than one eighth. This is no doubt owing to the less laborious nature of their occupations, and their being less exposed to mechanical injury. This proportion, however, is not maintained in particular aneurisms. Thus, for instance, in the 245 aneurisms of the aorta, pulmonary, and cerebral arteries, it amounts to about a fifth. Of the 25 carotid aneurisms, 13 occurred in males, 12 in females. In 308 cases of external aneurism, including the carotid, there were 276 males to 32 females, the latter forming barely a tenth; whereas in the 21 cases of dissecting aneurism there are 14 females to 7 males.

The period of life at which aneurism is most common, is between the ages of 30 and 40. Of the 551 cases reported in the table, 198 occurred between these ages: between 40 and 50 there were 129 cases. Whereas, before the age of 10, there was only one case, between 10 and 20, five cases, and after 70, eleven cases.

Mr. Crisp, in opposition to the opinions of many of the older writers on aneurism, and of some of the moderns, more particularly Mr. Porter, has not been able to satisfy himself that mercury exercises any influence in the production of this disease; he thinks, however, that syphilis, venereal excesses, the use of spirituous drinks, or any causes, mental or physical, producing violent and irregular action of the heart and arteries, must predispose to this affection. Of all the exciting causes of aneurism, whether external or internal, he believes violence and sudden muscular efforts to be the most frequent.

Mr. Crisp believes that aneurism is of more frequent occurrence in England than in any other country. This he attributes to the habits of life of the people, their great industry, the violent and continued exertions they are obliged to make, as well as their greater addiction to the use of ardent spirits. Sailors and soldiers are more liable to aneurism than any other class of the community, being often called upon to make sudden and very violent efforts. Amongst agricultural labourers, on the other hand, the disease is not frequent. Judging from the number of cases of aneurism reported in medical journals and publications, the only means that Mr. Crisp has been able to make use of in instituting a comparison between the frequency of the disease in this and other countries, he believes that next to England the disease is more prevalent in the United States of America, than in France, and, lastly, in Italy. It would appear

from information that he has received from resident surgeons, that aneurism is of extremely rare occurrence both in the East and West Indies, and South America.

Internal aneurism. By internal aneurism, Mr. Crisp means those cases that are not within the reach of surgical operation. Of 243 cases of this description, 175 were in the thoracic aorta, 59 in the abdominal and its branches; 2 in the pulmonary artery, and 7 in the cerebral; 198 were males and 45 females. Three of these cases occurred before the age of 20; twenty-three between the ages of 20 and 30; eighty-two between 30 and 40; after this age the number of cases gradually diminished. In his account of the symptoms and diagnosis of this class of cases, Mr. Crisp adds nothing to what is already well known.

Pathological effects of thoracic aneurism. On this subject the analysis of the table of cases has furnished some very interesting and important results. Aneurism of the thoracic aorta may burst into the left pleura and posterior mediastinum; the former mode of termination is most generally considered of most frequent occurrence in these cases. From this opinion, however, Mr. Crisp dissents, and states that it is his belief that the most frequent mode of termination of aneurism of the descending aorta is by rupture into the pericardium. The tumour rarely bursts externally, although in some cases before death small discharges of blood take place; and death, from external hemorrhage, appears inevitable. Sometimes the internal tumour is considerably reduced in size before death, and in five instances it disappeared entirely. The following is the analyses of the mode of termination in all the cases of thoracic aneurism collected by Mr. Crisp.

"*Ascending aorta.* Of 98 cases of aneurism of the ascending aorta (76 males and 22 females) the terminations were as follow:—30 opened into the pericardium, 6 externally, 4 into the left pleura, 1 into the right, 3 into the pulmonary artery, 3 into the right lung, 3 into the superior cava, 2 into the œsophagus, 2 into the right auricle, 2 into the right ventricle, 1 into the left, 1 into the trachea; 7 died from pressure on the trachea and bronchi, 6 from serous effusion into the pericardium and pleura, 2 from phthisis, 2 from syncope and exhaustion, 2 from coma, 1 from each of the following—regurgitation into the ventricle, compression of vena cava, of pulmonary artery, cerebral congestion, apoplexy of lung, distension of stomach, pericarditis, bronchitis, and hemoptysis; 4 died suddenly, without rupture of the sac, and in 8 the cause of death was doubtful.

"*Aortic arch.* In 48 cases of aneurism of the arch of the aorta (35 males and 13 females), 4 opened into the trachea, 2 into the pericardium, 2 into the œsophagus, 2 into posterior mediastinum, 2 into right pleura, 2 into the left, 2 into the bronchus, 1 into the pulmonary artery, 1 into the superior cava, 1 into the lung, 1 externally; 12 died from supposed pressure on the trachea and bronchi, 3 from suffocation, 2 from hydrothorax, and 1 from each of the following—pressure on the recurrent laryngeal nerve, pressure on the cava, hæmoptysis, dysentery, serous apoplexy, rupture of the descending aorta, and external violence; and in 6 cases the cause of death is not stated.

Descending thoracic. Of the 21 cases of aneurism of the descending aorta (all occurring in males), 4 opened into the right pleura, 4 into the left, 5 into the œsophagus, 2 into the left bronchus, and 1 into the left lung; 2 died from pressure on the trachea and left bronchus, 1 from exhaustion and suffocation, 1 from apoplexy; 8 cases are described as aneurism of the thoracic aorta, and their termination not given.

Treatment. In the treatment of internal aneurism little can be done

towards effecting a cure, but in many cases much may be accomplished towards alleviating the sufferings and prolonging the life of the patient. Our author lays some stress on the importance of small and repeated bleedings in these cases, with the view of diminishing the impulse of the left ventricle, and consequently the force with which the aneurismal sac will be distended. Six or eight ounces of blood taken at regular intervals he has found of much service, both as regards the diminution of pain and the prolongation of life. When much pain is present the preparations of opium will afford most relief, and in some cases large doses of this medicine will be required. Sprinkling a blistered surface with the muriate of morphia has also been productive of some benefit. The diet is a matter of much importance in the treatment of aneurism; it should be as simple as possible, and all nourishment of a stimulating character should be avoided, on the principle that anything that produces increased action of the heart is injurious.

Aneurism of the abdominal aorta. Of fifty-nine cases of aneurism of the abdominal aorta, recorded in the table, fifty-one were males and eight females. Eleven were between the ages of 20 and 30; twenty-two between 30 and 40; fourteen between 40 and 50; and three between 50 and 60. The ages of nine are not given. As the abdominal viscera are not so essential to life as the thoracic, these aneurisms may attain an immense size before proving fatal. In Mr. Langstaff's museum was one, the coagulum in which weighed five pounds and a half. In the Chatham museum is one, the coagulum of which weighed ten pounds; and in the museum of the London Hospital is an abdominal aneurism, the sac of which held just one pailful of water.

Tumours of various kinds in the abdomen, whether solid or fluid, may be mistaken for aneurism of the abdominal aorta; but what is called nervous pulsation of the aorta, is by far the most frequent source of error, and it often requires much nicety to distinguish between the two diseases. The following are the diagnostic signs given by our author.

"In abdominal aneurism, pain of an aching, tearing, or darting character is often felt. In aortic pulsation the patient seldom complains of pain in the immediate neighbourhood of the aorta. The sound, when heard in abdominal aneurism, is generally a single rough bruit. In aortic pulsation abnormal sound is seldom present. The best diagnostic marks are the following: in abdominal aneurism the impulse is mostly heaving and confined; in aortic pulsation free and bounding. The dissection of the sac in abdominal aneurism is generally lateral or posterior, and the tumour is seldom distinctly felt, when seated near the cæliac axis. The state of the abdominal viscera, the sex, age, habits, and constitutional peculiarities of the patient, are circumstances which must not escape the notice of the medical attendant.

"Abdominal aneurism more frequently commences near the cæliac axis, where (as I have before observed) the arterial tunics are probably much weakened by the origin of the collateral branches; the diameter of the vessel is likewise very perceptibly diminished below this part. In several instances recorded in the Table of Aneurisms, rupture of the sac took place some time before death, the blood finding its way behind the peritoneum. At the time of rupture the pain is very acute from the detachment of this membrane from its cellular connexions; there is also great depression of the vital powers. After a few hours or days the peritoneum gives way and the blood escapes into its cavity." (p. 163.)

The mode in which the fifty-nine cases of aneurism of the abdominal aorta and its branches collected by Mr. Crisp, terminated, is as follows:—

Eleven opened behind the peritoneum, ten into the peritoneum, four into the left pleura, three into the vena cava inferior, one into the lung, one into the colon, one into the pelvis of the kidney, one into the posterior mediastinum, one into the chest, two died suddenly without rupture of the sac, two from jaundice, and one from each of the following causes: —heart disease, scarlatina, phthisis, diarrhœa, exhaustion, hydrothorax, anasarca and ascites, senile gangrene, and rupture of sac by vomiting. The terminations of twelve cases are not given.

Cerebral aneurism. The cerebral arteries are, according to Mr. Crisp, more frequently the subject of aneurismal dilatation than others of a similar size. This he attributes to their unsupported state from the absence of a cellular sheath, the frequency of concussions of the skull, and the irregularity of the circulation in the brain from mental or bodily excitement. The eight cases of cerebral aneurism that are reported in the table all occurred in males. Five of the aneurisms were seated in the basilar, three in the cerebral arteries. They varied in size from a pea to a common apple. Three of the patients were under twenty years of age, and the remaining five between thirty-five and sixty. In the three that were seated in the cerebral arteries, death took place by rupture of the sac and extravasation of blood; in the remaining five, those seated in the basilar artery, sanguineous apoplexy, cerebral congestion, encephalitis, hemiplegia, and pressure on the medulla oblongata were the causes of death.

Dissecting aneurism. The following is the condensed account that our author gives of his views on dissecting aneurism.

"This variety of aneurism is much more frequent in women than in men, and most of the patients are advanced in years. It is not difficult, I think, to understand why this form of the disease should occur more frequently in the aged, when we consider the state of the arterial tunics at this period of life, their connecting media, from increased dryness, being more readily destroyed. I have performed several experiments on the coats of arteries of persons of different ages, which have fully confirmed this opinion. Thus, if a small syringe, Anel's, is filled with water, and its nozzle inserted into the layers of the fibrous coat, or between it and the cellular coat of an artery taken from an old subject, the fluid will be rejected a considerable distance between the tunics. If, however, the experiment be made on a young subject, considerable difficulty will be experienced in separating the laminæ, and the water will only divide the coats to a small extent. I am, however, at a loss to account for the greater frequency of this disease in females, and I will not at present attempt an explanation. The lower part of the ascending aorta is the most common seat of these ruptures, and the blood is generally discharged into the pericardium. Thus, 15 cases terminated in this manner, and 4 died of the following—senile gangrene, tuberculated lungs, dropsy, and orthopnœa; in 2 the cause of death is not given. This lesion is generally attended with acute pain, great collapse, sense of suffocation, and constriction of the chest; there are no positive signs, however, which can indicate its existence. In some cases death takes place at the time of the rupture; in others the cellular coat of the aorta and its pericardial covering form a barrier for a few days to the passage of the blood into the pericardium; whilst in a third variety a new channel is formed by the splitting of the coats; and in one instance this channel appeared to be lined with false membrane, the patient living for a considerable time after the rupture. Seven of the patients died suddenly, seven lived from one to twelve days after the accident; another survived a month; another lived several months; and in a third the period is not named. The coats may be separated on one side of the artery only, or the splitting may ex-

tend to the whole circumference of the vessel. In some instances, the blood, after dissecting the coats, again found its way into the artery; in two cases only did the rupture occur in the abdominal aorta, and in three others the blood passed along the coats of the thoracic to the abdominal aorta, being discharged above into the pericardium. From the experiments I have made I am disposed to think that an artery, containing a large quantity of bony and atheromatous matter, is not so likely to be affected with this form of aneurism, as one which is nearly free from deposit, the adventitious substance tending rather to prevent the splitting of the coats." (p. 168.)

External aneurism. We shall now give an analysis of the statistical data that Mr. Crisp has collected on the subject of external aneurism, in reference to the comparative frequency of this disease in different arteries and at different ages, and in the results of this ligature.

The observations made by our author on the general treatment of external aneurisms, on their spontaneous cure, and on the employment of pressure in those cases to which it is applicable need not detain us, as he gives nothing with which the profession is not already familiar. Indeed his observations on the employment of pressure in the cure of aneurisms, a subject of the greatest interest, and one to which the attention of surgeons is peculiarly directed at the present time, are very incomplete, evincing less acquaintance with the principles on which the modern treatment by pressure is conducted than we could have expected. Mr. Crisp's account contrasts very unfavorably with the very excellent and practical Essay lately published on this subject by Dr. Bellingham of Dublin, to which we would refer our readers as containing a most masterly account of the treatment of aneurism by compression. Mr. Crisp's observations on another mode of treatment that is also beginning to attract some attention, both in this country and abroad—we allude to the galvano-puncture—are also far too cursory and superficial. Short as they are, however, they are not free from error, the introduction of this plan of treatment being attributed to M. Petrequin of Lyons, instead of to a countryman of our own, Mr. Benjamin Phillips, who was the first to direct the attention of the profession to this subject, in an essay published as far back as the year 1832.

Secondary hemorrhage. The statements of different observers vary considerably in respect to the frequency of this accident after ligatures. Lisfranc states as the result of his inquiries into the details of 180 operations for aneurism performed according to the Hunterian method, that secondary hemorrhage occurred in 32; being in the ratio of 1 in 6. Porta in his recent work on the Ligature of Arteries, has collected 600 cases of the ligature of the principal arteries, in 75 of these secondary bleeding occurred; of these 31 were cured, 30 died from loss of blood, and 14 from other accidents. The treatment adopted in 31 cases was as follows: the plug was used in 13, torsion in 4, a second or third ligature in 9, bandaging or cold applications in 3, and amputation of the limb in 2. It will be seen by this, that according to Porta, secondary hemorrhage only occurs in about one twentieth of the cases, instead of in one sixth, as stated by Lisfranc. This discrepancy may very probably be reconciled by the fact that Lisfranc has only included in his estimate cases in which the vessel has been ligatured for aneurism, whereas Porta speaks of the results of the ligature for wounds as well as for disease. Mr. Crisp finds that of 256

cases of ligature of the larger arteries for aneurism, 21 patients died of hemorrhage, and in 8 cases the termination was of a mixed character, but bleeding to a certain extent had occurred, and therefore had some influence in producing the fatal result. The deaths lately from hemorrhage amounted to rather more than one twelfth, including the doubtful cases to barely a ninth; whilst the general mortality is 22 per cent., the general mortality of Porta's cases being 27 and a fraction per cent. All these operations were for the cure of spontaneous aneurism, and cases of the distal operation, of ligature of the aorta, the innominata, and of the subclavian, on the anterior side of the scalenus are included. If these cases were omitted, the rate of mortality would be considerably diminished.

The following is the time of separation of the ligatures in 150 cases of operation for the cure of aneurism, nearly all of them spontaneous. In two cases of ligature of the common iliac, the ligatures separated on the 18th and 35th days; of the internal iliac, on the 21st and 42d days. In 33 cases of ligature of the external iliac, on an average on the 22d day. In 54 cases in which the femoral was tied, the ligature remained on an average 18 days. In 30 cases of ligature of the subclavian, 17 days; in 21 cases of ligature of the carotid, 21 days; and in 8 cases of the brachial, 14 days.

Aneurism of the innominata. Of 20 cases of aneurism of the innominata, 15 of the subjects were males, and 5 females. Aneurism of this vessel seldom burst internally,—not one of the 20 cases terminating in this manner;—1 burst externally, 3 proved fatal by pressing on the trachea, 1 on the right bronchus. Serous effusion into the pleura, pericarditis, and exhaustion carried off 3 others; 6 were subjected to surgical operations, and in 5 the cause of death is uncertain. The application of the ligature to this artery has, in every case, been unsuccessful, and few surgeons would, we think, at the present day be found who would venture under any circumstances to have recourse to this operation. There are only three instances of cure of aneurism of the innominata on hand. This occurred to Mr. Porter, who attempted to tie the artery, but on cutting down upon it, he did not think it safe to apply the ligature; strange to say, the patient recovered, pulsation ceasing entirely in the sac. Another case was treated successfully by Mr. Luke, by small bleedings frequently repeated, and digitalis; and the third was a case in which the right carotid was tied by Mr. Evans; the patient was well nine years after the operation.

Aneurism of the subclavian. Twenty-six cases of aneurism of this artery are on record; of these 25 were males, and only one a female. 15 occurred in the right, and 8 in the left subclavian; in 3 the situation is not mentioned. The artery was ligatured in 23 of these 26 cases; of these operations 11 were successful, and 12 terminated fatally. In 4 of these unsuccessful cases, however, the artery was tied internal to the scalenus; in 2 the innominata was ligatured, and in 2 the sac was punctured, so that if these eight cases be included, the mortality from ligature of this artery when the operation is performed at an early period of the disease, and in the third stage of the vessel, is not very great. We may mention that all the cases in which the vessel has been tied internal to the scalenus have terminated fatally.

The results of the ligature of the subclavian artery for axillary aneurism

form a striking contrast with the preceding statement. Of 14 cases in which the ligature was applied to the subclavian for this disease, 11 were successful.

Carotid aneurisms. Of twenty-five cases of this disease, thirteen were males and twelve females. The frequency of the disease amongst females is a remarkable fact which we have not seen noticed by any other writer, and which Mr. Crisp attributes to the exposed state of the neck of females, and its consequent liability to injuries. Of these 25 cases, 21 were operated upon. Ten of these operations were successful, and eleven had an unfavorable issue. Five of the operations were performed on the distal side of the sac; of these three were successful, and in two the life of the patient was prolonged. The principal sources of danger after the ligature of this vessel are, as Dr. Miller and Dr. Norman Chevers have shown, referable to engorgement of the lungs or to cerebral disturbance, and though Mr. Crisp doubts the accuracy of Dr. Chevers's conclusions with regard to the frequency of the cerebral disturbance after ligature of the common carotids, there remains in our minds no doubt of the fact, after perusing the evidence that he has adduced in support of it.

Iliac arteries. Only two cases of aneurism of the common iliac are mentioned in the table; both occurred in males: one was a varicose aneurism, in the other the aorta was tied.

Aneurism of the external iliac occurred in nine patients, all males. In two cases the common iliac was tied: in the first, death occurred on the eighth day, from hemorrhage; in the last, the operation was successful; and in four it was ligatured successfully.

With the exception of the popliteal, the upper third of the femoral artery is the most frequent seat of external aneurism. Mr. Crisp has collected 66 cases of aneurism in this situation, 61 of which occurred in males and but 5 in females. A remarkable fact connected with this species of aneurism is its frequency in sailors: of 39 patients whose occupations are given, no less than 12 being sailors. In these 66 cases the external iliac was ligatured 43 times, in two instances the femoral was tied also; 36 of these cases terminated favorably; of the 10 unsuccessful, 3 died from hemorrhage; 2 from sloughing of the sac; 2 from pressure; and 1 from gangrene; another from tetanus, and a third without apparent cause. The femoral artery alone was tied in 12 cases: of these 9 were successful, and in 5 cases amputation was practised with success.

Popliteal aneurism. The table contains 137 cases of aneurism of the popliteal artery. Of these, 133 were males and 4 females; in 44 cases the left side was affected; in 40 the right, in 42 the situation is not named; and in 11 cases the aneurism was double. Of these 137 cases, 119 were cured and 18 died. In 110 cases the Hunterian operation was performed in 91 with complete success, and 7 others terminated favorably after amputation, the ligature having been previously used. The 11 patients affected with double popliteal aneurisms were all cured, the ligature being used in 10 and pressure in one.

Circoid aneurism, or aneurism by anastomosis. Forty-five cases of this disease are arranged in a tabulated form: 24 of these were males, 16 females, and the sex is not stated in 5 cases. In 38 cases the disease was seated in the head, neck, and upper extremities. The lower part of the rectum, perineum, nates, scrotum, leg, knee, and foot, were affected in

the remaining seven; 22 were cured, 6 benefited, 11 died after the operation, and 4 were not operated on. In 16 cases of this disease, seated about the head and face, the carotid was ligatured; and in 3 patients both carotids were tied. The ligature alone was successful in 5 cases; in 7, benefit was derived; and the disease was afterwards removed by pressure, caustic, or incision. In the 3 fatal cases, the patients did not appear to die from the immediate effects of the operation.

Our author next devotes a page or two to some cursory remarks on the important and interesting subjects of aneurism of bone and fungus hæmatodes, and gives two or three short chapters on ruptured, torn, and wounded arteries, which, as they are not the result of personal observation, need not detain us; these are followed by a mere sketch of the diseases of the veins, with which the work concludes.

On the whole, we regard Mr. Crisp's book as a valuable addition to our surgical literature. Our remarks on various parts of it show that we think it might have been made better than it is; but, with all its imperfections, we consider it as highly creditable to its author. It can have no pretensions to be considered a *complete* treatise on the diseases of the blood-vessels, many of the most important of these affections being treated only in a superficial and incomplete manner. Its principal value consists in the Table of Aneurisms which the author has, with great industry and care, constructed from various sources; and by the analysis of which new light is thrown on several points connected with aneurism, and the result of the ligature of arteries. The value of this collection would, however, have been much increased had the cases been arranged in a systematic manner, and not put down merely in the order in which the author met with them, without any attempt at classification or arrangement. If called on for a new edition, Mr. Crisp will add greatly to the value of his work by enlarging his statistics still further, so as to include the records of foreign as well as of British pathology and practice; and by paying more attention to some of the points on which we have taken the liberty to remark.

Art. XII.

Om Torsk hos Barn. Af Dr. Fr. Th. Berg, Ofverläkare vid Stockholms Allm. Barnhus, &c.

On Thrush in Children. By Dr. Fr. Th. Berg, Physician in Chief to the General Children's Hospital in Stockholm, &c.—*Stockholm*, 1846. 8vo, pp. 157.

We have in former notices of the labours of our Swedish brethren, awarded, we hope, due praise to their steady pursuit of the legitimate objects of medical science; the work now before us, is a further proof, that we have not overvalued their merits. Dr. Berg's modest little volume has afforded us great satisfaction. We have admired the diligence and perseverance requisite for so many and such varied experiments and observations, while the candour and good faith with which these researches are recorded is a guarantee for their perfect accuracy

It is true that the malady of which our author treats is regarded by many, as a disease of no great importance in a practical point of view;

but those who have experienced the difficulty of finding appropriate remedies to check its progress, will be glad to acquire a more intimate knowledge of a disorder too lightly noticed in works on infantile disease. To the scientific investigator this little essay will, we are sure, be valuable from the numerous and well conducted experiments it records, while to the microscopic observer its interest is enhanced by the fresh paths of research which it lays open for the still further cultivation of pathological and physiological subjects.

Definition. The following definition of Thrush by our author differs widely from that of earlier writers.

"Thrush is a deposit upon the mucous membrane, in the form of small points, rings, conical or hemispherical elevations; at other times it covers large surfaces with a continuous membrane. It is at first of a milk-white or pearly hue, but occasionally when undisturbed, or when it affects children at the breast, it assumes a gray or yellowish colour, but it rarely becomes darker without the addition of foreign matters. It is more or less of a soft cheesy consistence, and in thickness varies from that of the thinnest paper to half a line or more. At first it adheres pretty closely to the parts on which it appears, but later, it becomes more loose, and at length is spontaneously detached without any lesion of continuity of the subjacent mucous membrane. Thrush is met with most frequently on the edges of the lips, where the skin merges into the mucous membrane, and on the inside of the cheeks, on the gums, the hard palate, and the upper and under surfaces of the tongue; as likewise in the fauces and œsophagus, down to the cardiac orifice of the stomach."

Microscopical character. The great object of the author is to elucidate the microscopical character of the disease. We make no apology here for translating the writer's own words, as it would not be possible to condense his descriptions without materially impairing their completeness. In microscopical anatomy the omission of one small part of the appearances observed is hardly less injurious, than the leaving out of an acid or an alkali in a chemical experiment.

"The white coating consists of epithelium thickened by the swelling of its constituent cells, and from this epithelium there springs a parasitic fungus in greater or less quantity, so that the chief portion of a patch of aphthæ is composed either of epithelium, or else of the parasitic growth. Now the relative proportions of these two substances seem to depend upon the length of time that has elapsed since the growth of the parasite commenced, which varies in different children, and it is also in relation to the density of the epithelial thickening. More or less of molecular albuminous matter is also to be found in these patches.

"When the parasitic growth and the epithelial condensation is confined merely to the extremities of the smaller papillæ, (as at the point of the tongue) they have the appearance of small isolated specks, but when they appear upon portions of the mucous membrane where the papillæ are less prominent and where the intervals between these are filled by a denser epithelium, (such as is seen on all parts of the mucous membrane of the mouth save on the upper surface of the tongue,) the white coating then assumes those forms of circles, of interlacing bands, or of hemispherical elevations, so frequently observed in parasitic vegetations, where they are permitted to increase freely without any mechanical obstacle to their growth. Lastly, when both the epithelial and the parasitic growths alike proceed vigorously, those spots which at first were isolated, coalesce more and more into a continuous covering, the cohesion of which is maintained not merely by the natural adhesion of the epithelial cells, but also by the interlacing of the parasitic fibres, among themselves, and between the cells of the epithelium." (p. 3.)

Colour. The alterations of colour observed in thrush and aphthæ are

next described at full length, and the cause of these variations is carefully explained.

The usual colour of aphthæ is well known to be white or pearl gray, but when the parasitic growth increases and forms numerous sporuli on the surface, the spots tend to assume a yellow or greenish hue, like other fungoid growths where sporuli are produced in abundance. In this way we explain the fact that aphthæ occurring in children brought up at the breast and in their own homes, are usually thinly scattered, and of the usual white or gray appearance; while in lying-in hospitals, where children rarely get the breast, where their mouths are seldom cleansed during their prolonged slumbers, the aphthous vegetations advance unchecked, and constantly assume the yellow or green colour.

Attachment. Aphthæ are well known to be more firmly adherent in some parts than in others, and to vary in this respect at different periods of their existence. Our author accounts for these differences in the following paragraphs.

"The fine threads or fibres of the parasite insert themselves in every direction amid the superior layer of epithelial cells, and, according to Gruby they pass even into the cells themselves. By this means they acquire a firm adhesion to the neighbouring parts, and involve many epithelial cells, among their meshes. This will explain why aphthæ are so firmly attached when first formed, and will account also for their varying degrees of adhesion in different parts. Certain surfaces present numerous papillæ, and these latter are often cloven at the summit in many directions, thus affording an innumerable quantity of furrows and depressions for receiving the fibres of the parasitic growth. In this way aphthæ become firmly fixed upon the tongue in spite of the perpetual movement of that organ, while on other parts of the buccal mucous membrane, where the papillæ are fewer, smaller, and less elevated, and rarely cloven at the summit, they adhere with much less tenacity.

It will be known however to the reader that these vegetations do not sink deeper than the epithelial layer and that this layer is constantly renewed by the destruction of its most external cells, their place being supplied by new cells from the inferior surface. The spread of the parasitic fibres among the cells of the epithelium will tend still further to loosen the adhesion of the latter. But even these causes combined are scarcely sufficient to account for the whole process of softening and throwing off of the aphthous patches; it seems that the same (*undiscovered*) cause which renders the epithelium the appropriate soil for the growth of these parasites, produces also in its cells an operation similar to that of maceration if we may judge from the white colour assumed under such circumstances by the membrane. In consequence of this, there ensues, sooner or later, the throwing off of the whole aphthous patch with the subjacent epithelium, after which there remains the mucous membrane covered with the thinnest possible layer of recently formed epithelium; and the thinner the latter is, the deeper is the hue of the subjacent mucous membrane.

"Aphthous vegetations therefore require to have a firm root among the cells of the epithelium, before they can vegetate upon those surfaces where there is a constant motion of the parts. Consequently the epithelium on which we find numerous ciliæ in constant movement is eminently unfitted for the localization of aphthæ, nor will the epithelium which is composed of a single layer of cells be better adapted to this purpose, as the membrane will then be undergoing constant and rapid change. We must therefore conclude that the best *soil* for the growth of aphthous vegetations is undoubtedly that portion of the epithelium where the absence of ciliæ gives full scope to the implantation of sporules, and where several layers of cells afford full room for the spread and interlacing of the vegetable fibres, and where, finally, the membrane being less rapidly changed, due

time is allowed for the development of the vegetable growth. Such is the character of the epithelial covering from the beginning of the mucous membrane on the free edge of the lips, throughout the whole cavity of the mouth, the fauces, and œsophagus, down to the cardia, while the adhesion of the parasitic growth is greatly favoured on this surface by the numerous furrows lying between the thickly strown papillæ, and by the cloven summits of the latter as they are seen on the upper surface of the tongue.

This species of epithelial covering ceases however at the mouths of the salivary ducts, which are lined with a single layer of *cylinder epithelium*; it terminates likewise at the upper and back part of the velum palati; at the mouths of the Eustachian tubes, and at the base too of the epiglottis, immediately above the upper edge of the superior ligamenta glottidis. In all these points it is replaced by ciliated epithelium, while at the cardiac orifice of the stomach it gives place to a single layer of *cylinder-epithelium*" (pp. 3-5.)

Habitat. Dr. Berg questions the truth of the assertions of various authors that true aphthæ have been found in the respiratory organs or in the stomach. Most of those that maintain this opinion have not, he observes, examined the appearances they describe with the microscope, and without the aid of this instrument, he is unwilling to accept of their diagnosis. On the other hand, our author observes that nothing is more easy than for a small portion of aphthous matter to escape from the œsophagus into the stomach, or into the respiratory organ. He candidly acknowledges that in 1842, he himself announced the discovery of aphthæ firmly adhering to the mucous membrane of the stomach, but since that period he has never been able to detect such appearances, and he is now strongly inclined to doubt the accuracy of his former opinion.

Microscopical character. Our author again passes to the microscopical character of aphthæ. The powers to which he subjected these appearances for examination increased the objects by 200 to 300 diameters.

"Under a microscope of the above powers we find," says he, "that an aphthous crust consists of epithelial cells, with a more or less interweaved web of fibres, and a variable number of spherical or oval cells, without any sign of exudation-corpuscles, but only a small quantity of molecular albuminous deposit. These cells are, with the exception of the nucleus, everywhere transparent, and have a sharply-defined edge, they vary in size according as they are oval, spherical, or more prolonged (0·0004 — 0·0015 m. and more). Sometimes they present no nucleus, or an almost imperceptible trace thereof, but the larger ones contain almost always nucleated cellules, sometimes two or even more. These nuclei lie usually in the centre, but very frequently also at the end of the cells. We can often trace the successive development of these cells from a spherical one of the smallest size to an oval cell, and thence to a filament; and we have no doubt ourselves that the smaller cells are sporules, out of whose development the larger oval cells are formed, and finally, the filaments in the same manner as has been observed in other fungoid growths of this nature. The sporules above noticed bear a strong analogy to those of yeast (Torula cerevisiæ), and this holds good not merely as regards their external form, but likewise in their power of resisting chemical reagents." (pp. 7-8.)

Numerous projecting fibrils are observed on the circumference of an aphthous crust when submitted to the microscope, but these are rendered infinitely more clear by a weak solution of potash, which dissolves the albumen, and renders the cells of the epithelium transparent, while, at the same time, it diminishes their intimate cohesion, and the network of vegetable fibres is more plainly seen.

"These fibres are cylindrical, with sharply-defined dark edges, and their centres are transparent in transmitted light; they are generally equal in thickness, but at times they are, as it were, knotted together, and divided by distinct walls of separation (skiljovägg). Their diameter is from 0·001 m. to 0·004 m. and more; their length from 0·1 m. — 0·2 m. If the dividing walls are sharply defined, and lie near to each other, the fibrils take on the aspect of a string of beads, and occasionally they closely resemble the tubes formed in higher organizations out of cylindrical cells. In their interior, these fibrils often exhibit nucleated cells; occasionally these are very numerous, and of small size, but at times they are larger. In their course the fibrils divide into numerous branches, whose diameter is not less than that of the original stem, and I have occasionally observed these ramifications to increase in thickness, at their free extremity, and to terminate in a club-shaped end with a species of cell. From the sides of the fibrils spring numerous sporules forming a point of departure for new ramifications.

"These fibrils are, it seems, as little affected by chemical re-agents as the cells we have before described.

"Careful investigation has shown us that these cells are placed upon the sides of the fibrils, and in particular that they are congregated around the terminations of the latter. It must therefore be admitted that the cells and the fibrils are both constituent parts of one and the same organization. Where this growth vegetates undisturbed, its fibrils penetrate between the layers of the epithelial cells, but do not extend deeper than the inferior layer, though they spread laterally in every direction. On the free surface of the epithelium, the ramifications rise above the surface, exhibiting at the same time an abundant fructification, which gives a yellowish hue to the exterior." (pp. 8-9.)

Thrush a parasite. Dr. Berg considers it to be perfectly established that the parasitic growth just described is a constituent part of thrush, for the latter, he observes, is never met with without the parasite, which is to be seen at the very earliest appearance of the aphthæ, and continues present throughout the whole of their existence on the mucous membrane. "Indeed," continues he, "I do not consider those affections to be aphthous where this parasitic growth is not to be found; and lastly, I advise all who think they have observed true aphthæ without its parasitic constituent, to add a little caustic potass to the supposed aphthæ before they determine on its nature."

It is impossible indeed, according to our author, to explain the nature and course of this disease if we deny its parasitic character.

Conditions of life of the parasite. The next question discussed by Dr. Berg is the important one, whether the parasitic fungus can vegetate only on the living mucous membrane, or whether it can be produced and grow in other menstrua, and out of the human body. Many carefully conducted experiments, occupying some pages of the work, are here recorded to decide this point. We have only room for the conclusions arrived at by our author, and which he sums up as follows:

"1. The aphthous parasite can propagate itself in appropriate menstrua out of the body, and this not only when the aphthous crust is mingled with various animal fluids, but also when completely separated and cleansed from these.

"2. Its growth in such cases proceeds not only in a temperature equal to that of the human body, but likewise in one that is much lower.

"3. Aphthæ seem to require for their growth the presence of a body containing azote, such as albumen, as also that of materials for the generation of acid.

"4. Out of the body aphthæ seem to develop themselves in two different forms, either in that of a great preponderance of sporules (sporidier), when a white

filmy membrane forms on the surface of the fluid; or again they appear chiefly as stems ramifying through the fluid, or aggregated into a felt-like mass.

"A solution of potass will always dissolve the molecular deposit of albumen, leaving the fibres and cells of the parasite totally unchanged." (p. 14.)

Dr. Berg admits that the parasitic growth may be propagated in bodies previously unaffected, by sporules floating in the atmosphere, particularly under certain *electro-chemical* conditions of the air, and also of the bodies they affect.

Description of the disease. The disease in question is next described by Dr. Berg. As his theory of the malady differs widely from that of most pathologists, excepting a few of the most recent date, so we find his picture of the disorder, to present many discrepancies from the generally received descriptions. This was only what might be expected, when a totally new character is given to an affection of very common occurrence. It remains for the medical world to pass judgment on the parasitic doctrine of disease. We are however on one point fully satisfied, that should this theory, as we confidently believe, turn out to be in all respects well founded, the work of Dr. Berg will ever be a standard book with microscopical observers, and, like the observations of Leuwenhoek, will not lose its value amid the improved researches of future ages.

Our author distinguishes aphthæ into *discrete* and *confluent*, and likewise calls our attention to the different portions of the body in which it is found. He has not himself seen the redness and increased heat of the mouth which is stated by most observers to precede the outbreak of aphthæ, and he therefore does not recognise stomatitis as its necessary or even as its frequent antecedent. He does not however deny the possibility of thrush appearing on the inflamed mucous membrane, but he regards it then as a complication of the primary disease. The two varieties of stomatitis, *S. vesicularis* and *S. folliculosa*, have been too often described as true aphthous maladies, but Dr. Berg insists on the fact that these two forms, however severe they may be, never present the parasitic growth, characteristic of true thrush. It is this error which has given rise to the names of ulcers and pustular aphthæ. The question whether severe thrush ever appears without general disorder of the constitution, is answered by our author in the affirmative, but he owns that general symptoms are more frequently present. Those however which by medical writers are described as peculiar to thrush, are common, he thinks, to many of the other disorders of children.

The general torpor and great tendency to sleep, so often observed before a severe invasion of the malady, may, he thinks, act as a mechanical predisposing cause, by allowing the mouth to remain so long in a quiescent state, and thus favouring the rapid development of the parasitic growth. But Dr. Berg believes thrush mainly to depend on a "bio-chemical alteration of the fluids," and especially of the secretions of the buccal cavity. These secretions he has generally found to be acid, though on a few occasions they gave an alkaline reaction; but when thrush had decidedly appeared the secretions of the mouth were invariably strongly acid. Much of the disorder of digestion may, our author suggests, arise from the quantity of loosened aphthous crusts swallowed by the patient, but we do not ourselves believe that they can be sufficiently abundant to produce this effect.

Out of 115 patients affected with thrush in the Lying-in Hospital of Stockholm in the year 1845, only 29 had stools of the normal yellow colour throughout the whole course of the disease, while in the remaining 86 green evacuations appeared simultaneously with the invasion of the disease, or supervened at a later period of the malady. There seems therefore, to be some connexion between the green stools and the presence of thrush on the mucous membrane; and to the investigation of this relation our author accordingly proceeds.

In general the colic, vomiting, and diarrhea indicating disorder of the digestive organs, are in a direct relation to the extent and severity of the aphthous affection. The green evacuations are generally acid according to Dr. Berg, and he believes this peculiar hue of the fæces to be caused by an excessive proportion of acid, or by the presence of acids which do not normally exist in the digestive tube or at least are found there in exceedingly small quantities. Our author suggests that this excess of acidity may, in part, be occasioned by the admixture of fragments of the parasitic growth with the contents of the stomach, and he hints that the action thus induced is somewhat analogous to the process of fermentation from yeast. To elucidate and confirm these suppositions Dr. Berg has carefully performed a great number of experiments, the recital of which occupies five or six pages of his essay. We cannot of course in this brief notice give any of these experiments in detail, but they appear to us to have been scientifically conducted, and the instances where the results have been contrary to the author's expectations are candidly recorded. The conclusions he draws from these experiments are as follows.

"1. Aphthæ, whether attached to their original seat, or when loose and floating in a solution of starch, of sugar, or of sugar of milk, may cause the formation of lactic acid in a temperature from + 12° to — 15° Centigrade (79° to 30° Fahr.)

"2. Aphthæ have the property of throwing down albumen, and of forming casein, out of the aforementioned solutions, probably by the generation of acid.

"3. These operations take place much more rapidly in a temperature corresponding to that of the human body.

"4. The growth of the aphthous parasite in a solution of sugar containing albumen, and at the temperature of the human body, is accompanied with a *fermentation-like* development of carbonic acid gas, and with the formation of lactic acid. Under certain circumstances, acetic acid is produced by the influence of aphthæ.

"5. Yeast likewise produces the formation of acid in the above solutions, when it does not occasion fermentation, and the acid thus formed is probably the lactic, but yeast is much slower in producing this effect.

"6. Although it must be acknowledged that the coats of the stomach, and animal membrane in general, nay, even that albumen alone, may change a solution of sugar into lactic acid, and that thus the other constituents of the aphthous parasite, i. e, the molecular albuminous deposit, the epithelium, and portions of lactic acid may contribute to the generation of acid, yet it cannot be denied that the chief influence of aphthæ, in this respect, depends on the presence of the parasitic growth itself." (pp. 36-7.)

A great difference was found in the course of these experiments between the action of the reagents on cow's milk, and on that secreted by the human female. Dr. Berg regards this as a fact of great importance in reference to the question of bringing up children by hand, instead of their deriving their nourishment from its natural source, the mother's

breast; and as affording a partial explanation of the greater frequency of thrush in the one case, than in the other.

We would gladly have continued our extracts from this most interesting portion of Dr. Berg's work, but want of space obliges us to refrain. After tracing the production of acidity in the primæ viæ through the agency of the aphthous vegetations, he proceeds to consider its direct influence on the functions of digestion and nutrition. Dr. Berg inclines to the belief that the parasitic growth may vegetate without any fixed seat, when mingled with the fluid contents of the stomach, and it will thus tend to increase still further the excess of acid. Finally, our author repeats his conviction that both the local and the general symptoms which accompany thrush in the child are, in most cases, immediate or secondary consequences of the presence of the parasite, and are not to be regarded as the causes of that fungoid vegetation. If thrush were only a plastic deposit, the result of inflammatory action, we should find it in most cases preceded by symptoms of general disorder or of local inflammation, both of which are constantly wanting.

Our author does not regard the mildness or severity of the symptoms as dependent upon any variation in the nature or specific character of the parasite, but as entirely influenced by the part of the body in which it occurs; by the idiosyncrasy of the individual; and, lastly, by external circumstances—as bad food, deficient ventilation, and want of attendance.

The course of the disease, is divided by Dr. Berg into the following stages:—1. To the first he gives the expressive name of "*Inplantations-stadium,*" and no English word perhaps could so well express the period when the first germination of the parasite is taking place upon the mucous membrane. 2. The stage of local increase of the parasite. 3. The period of the secondary general symptoms, i.e. the excessive production of acidity in the primæ viæ, &c., as before described. 4. The stage of convalescence. Of these four stages the second is the only one which is constantly present. An interval of from twenty-four hours to seven or eight days is often observed between the second and the commencement of the third stage.

"If," the author continues, "it may be conceded that the general symptoms of the third stage arise from the increase of sporules of the parasite in the liquid contents of the intestinal canal, attended with the development of carbonic acid gas, we can easily comprehend the necessity of a brief interval between the appearance of the vegetation on the tongue and the commencement of the fermentation (?) process in the intestinal canal, during which time a sufficient quantity of sporules have been swallowed to produce a competent catalytical operation on the contents of the intestines." (p. 43.)

Dr. Berg has never seen death ensue as an *immediate* consequence of aphthæ. Out of more than 600 post-mortem examinations of children, he has never observed *attached* aphthæ further than the cardiac orifice of the stomach, nor has he ever detected them in the air-passages.

Aphthæ can occur as a complication of almost all the diseases of childhood, and their growth is then favoured by the general deterioration of the fluids produced by these different disorders.

Our author considers the employment of the microscope to be the only certain mode of deciding on the presence of thrush, as there are many occasions where the nature of the exudation is doubtful, as for instance,

where it is coloured by blood, or by pus from ulcerations or by dark ichor from gangrenous sores.

Causes of the disease. The theory of a "generatio æquivoca" to explain the immediate cause of the malady, cannot, Dr. Berg thinks, be adopted, as long as the possibility exists of the disease being otherwise conveyed. Dr. Berg suggests that the active principle of the sporules continues to exist after the crusts have been dried and pulverized, and on being again placed in favorable circumstances they will vegetate, in the same way as yeast when dried and then exposed to moisture, will still produce fermentation. This he has verified by ingenious experiments the details of which are too long for insertion here. From the consideration of this interesting point our author passes to the examination of those influences which eminently predispose the mucous membrane of the infant to the reception and increase of this disease.

The predisposing causes are of two kinds, mechanical and bio-chemical. The former require but a very brief notice, as they are only those which favour the localization of the sporules upon the mucous membrane of the mouth, &c. To the latter are referred the chemical constitution of the saliva and of the secretions of the mucous membrane, the presence of starch or of sugar or of other substances which might be converted into lactic acid, the presence of albumen or of any combination of protein containing azote, all of which more or less favour the growth of the sporules.

The first of the vital causes, which our author takes into consideration, is age. Aphthæ are not exclusively confined to the tender years of infancy, they are likewise found in childhood, and in maturer years, while in old age they again become more frequent. Dr. Berg observes that the somnolence, which is natural to very young children, is seen again in the aged, when suffering from other maladies, and he regards this symptom as a predisposing cause of aphthæ, from the mouth being left, for so long a period together, in a state of comparative repose. We confess that we do not ourselves attach much importance to this as a cause of aphthæ, but should the theory of Dr. Berg in regard to the true nature of this disease be the true one, it will of course acquire greater weight.

In considering the bio-chemical causes of aphthæ in children we must remember that the food of the child consists almost exclusively of fluids or solids containing sugar and starch, or albumen and casein, all of which the preceding experiments of Dr. Berg have proved to be eminently favorable to the growth of the parasite.

As regards *climate*, the warmer regions of the globe seem, as might naturally be expected, to be the most favorable to the production and growth of aphthæ, and they appear according to Auvity and Billard to be more frequent in summer than in the colder seasons of the year.

Deficient ventilation and want of cleanliness are acknowledged by all to be a powerful predisposing cause of aphthæ. A close, confined atmosphere is eminently favorable to the growth of a low type of vegetable life, while the unfavorable influence thus exercised on the health of the child, will render it more liable to all kinds of disease.

Food and feeding. The nature of the infant's food, and likewise the mode in which nourishment is given to it, is most carefully considered by

our author. If the milk be of bad quality, it must necessarily impair the health of the infant, more or less, but Dr. Berg denies the existence of any peculiar acridity in the milk of some individuals, as has been asserted and believed by the public, and by many medical men from the earliest ages to the present time. It cannot however, as he observes, be denied, that the milk occasionally undergoes such changes as to render it unfit for the nourishment of the child, but the real nature of these alterations is as yet in a great measure unknown.

Much influence is ascribed by our author to the operation of artificial food in favouring the growth of aphthæ. It is not only that such food contains an abundance of starch and sugar, but also, that in many cases it may undergo a partial chemical alteration and decomposition before it is received by the infant. Dr. Berg does not regard the practice of feeding the infant with the sucking-bottle, as injurious, but it is necessary, he observes, to be careful that the milk flows freely through the artificial nipple appended to the vessel, as otherwise he thinks that the mucous membrane of the mouth might possibly be injured by the child's strenuous efforts to extract its food. Should the opening however be too free, then the mouth would become filled to excess with food, and this remaining more or less in that cavity, would produce injurious effects. These hints may to some appear trivial, and fit only for the consideration of mothers and nurses; but we think otherwise, and regard them as proofs of the just care and attention which Dr. Berg has bestowed on every portion of his subject.

Altered health, from whatever cause, undoubtedly predisposes children to this disease, by either producing a tendency to prolonged sleep, which eminently, in our author's opinion, favours the growth of the parasite, or in other cases by so altering the character of the secretions from the mucous membrane of the mouth as to render them important agents in augmenting the disorder. Dr. Berg does not agree with Andral and Gavarret in regarding thrush as a product of an excessive formation of albumen; he believes it to result rather from a reaction between the albumen and an acid.

Contagion. Our author adopts to the fullest extent the doctrine of the contagious nature of the malady which he describes. He believes that thrush may be conveyed from one patient to another by sporules, or fragments of sporules, in the dried state, floating in the atmosphere, but that it still more frequently is propagated by the bottles from which children with thrush have been fed, or by the nipple, especially where, as in many hospitals, two children are suckled by one nurse. To decide the question of contagion our author made numerous experiments, and the results were uniformly favorable to his previous ideas. The experiments were made by applying aphthous crusts to the mucous membrane of the mouths of perfectly healthy children, and in every instance thrush was rapidly produced. We cannot, however, call these trials in one respect perfectly successful, for they occasioned great suffering to the children and so materially interfered with the health of one of the little patients that it died afterwards of bronchitis and pneumonia. Although the death of this child cannot directly be referred to the experiments made upon it, we confess we should not ourselves feel perfectly at ease were such a result to ensue in our own practice. It is, however, but justice to our author to

state, that after this untoward event he abstained from further researches of the kind.

Prevalence of the disease. Dr. Berg has spared no pains to make himself personally acquainted with the disease in different parts of Europe. Like many of his Scandinavian brethren, he has been an assiduous student in foreign schools. He has visited the foundling hospitals of Germany, Italy, and France, and has carefully perused the medical works relating to those countries he could not himself reach.

He does not regard the seasons of the year as having much influence on the prevalence of thrush. From 1842 to 1845, 733 patients affected with this disease were treated by him in the children's hospital in Stockholm. Of these 231 cases occurred in January, February, and March; 190 from April to June; 136 from July to September; and 176 from October to December; consequently there were 407 cases during the winter months, and 326 during those of summer. This proportion, he observes, would militate against the doctrine of the disease being more liable to occur in summer, but the fact is, that during the arctic winter of that high latitude, the apartments of the hospital are badly ventilated, and are kept at a high temperature by stoves, while the air is naturally less pure and less frequently renewed than in summer.

The fifth day after birth is the earliest period at which Dr. Berg has seen an infant affected with thrush.

One third of the children received into the hospital at Stockholm are attacked with the disease.

Relapses are not uncommon,—a previous attack does not seem to shield the patient from a return of the malady.

Danger. Much difference of opinion prevails in Stockholm as elsewhere as to the degree of danger incurred by patients labouring under this disease. Dr. Berg explains this by the fact, that these opposite opinions have been gathered under totally different circumstances. Those physicians who have observed thrush in private practice, or in lying-in hospitals, where children are suckled by their mothers, or by healthy nurses, are, he remarks, fully justified in asserting that thrush is a comparatively mild and insignificant disease. But where the malady is studied in large foundling hospitals, where the children are mostly brought up by hand, it assumes a much more fatal and malignant character, and fully bears out the assertion of the opposite party, that few disorders are more perilous to the infant. The more the disease is spread over the mucous membrane, the longer it resists treatment, the greater is the danger, especially if the vegetations be abundant and large, and if the whole assumes a yellow or dark brown hue.

Prophylaxis. The pages devoted to the prophylaxis of the disease are among the most interesting in the book. Dr. Berg insists strongly on the great advantages of cleanliness and of separation of diseased from healthy children. Without these all other precautions will be nearly useless. Were it possible, observes our author, to discover some chemical preparation which would effectually check the growth of the parasite, we might then regard the malady as one always within the power of medicine to heal. Perhaps the nitrate of silver in a solution of eight grains to the ounce of distilled water may effectually prevent the increase of the

malady. We ourselves know that it constantly checks its further progress, and we may perhaps hope that if it were applied in good time to the mucous membrane it would effectually arrest the tendency to the increase of the vegetation.

Treatment. As the true nature of thrush has been an object of controversy, so the treatment of this disease has varied according to the doctrines adopted regarding its character. Some of the ancient and many too of the modern writers maintain the malady to be almost entirely local, and under this impression apply their remedies directly to the seat of the disease, while others, deriving it solely from milk of a bad quality supplied by the nurse or mother, restrict their treatment entirely to the latter, giving medicines to improve the quality of the milk, without any direct remedial interference with the child.

Dr. Berg steers a middle course between these opposite opinions, and while he bestows the utmost attention on the milk of the nurse or mother, he states that local treatment can never be entirely neglected; nay, that in many cases it forms the most important part of the cure. The variations in the character of the milk should be carefully studied, both chemically and with the microscope; aids to medical investigation which were formerly unknown, and even now are but imperfectly appreciated by the great mass of practitioners.

General Treatment. In the treatment of thrush, Dr. Berg observes, it is necessary to make a distinction between simple aphthæ or thrush occurring in an otherwise healthy child, and those forms of the disease which attend on other maladies. In simple thrush he regards general treatment as unnecessary; local applications will usually cure the disorder. Even when gastric affections supervene, a perseverance in the mere local treatment is often all that is required. But when the digestive organs become seriously affected, general remedies must also be had recourse to, and as these affections often remain after the aphthæ have disappeared, constitutional treatment should be persevered in till the health is completely restored.

Our readers will remember that Dr. Berg considers the disorder of the digestive functions in thrush to arise mainly from a superabundant formation of lactic, butyric, acetic, and carbonic acids, and consequently the alkalies and their carbonates are strongly recommended to neutralize the excess of acid. Of these he gives the preference to soda; but should an excessive development of gas ensue from its employment, he advises us to have recourse to "aqua calcis" and "magnesia usta," as a change of remedies is often urgently required.

Emetics are indicated when there is reason to suspect that the aphthous crusts have been swallowed to a great extent by the patient, and that they are acting injuriously on the contents of the stomach. When the child vomits spontaneously coagulated or uncoagulated milk or mucus, an emetic will have the most favorable effect. The quantity of acid generated in the intestinal canal causes an excessive formation of casein, and particles of a cheesy appearance are then passed in abundance by stool. In such cases purgatives are extremely useful in clearing the intestinal canal. For this purpose castor oil answers best, but when the bowels become slow, and much mucus is secreted from them, rhubarb and jalap should be

exhibited. Calomel has very seldom been employed by Dr. Berg as a purgative in thrush.

To relieve the colicky pains and to obviate the tendency to intussusception so frequent in infants, Dr. Berg has generally employed antispasmodics and sedatives. The favorite preparation of this kind with our author is the *liq. succin. ammon.*, given either by the mouth, or in the form of an enema. In the majority of the milder cases, he has contented himself with administering a little of the *pulv. rhei* and magnesia. Dr. Berg warns us against giving any medicines in sugar to children suffering from aphthæ. He believes it to be an injurious practice, as the sugar being converted into lactic acid, will favour the increase of the parasitic vegetation.

Local Treatment. After a brief survey of the local remedies praised by the earlier writers, some of which are curious indeed, he observes that the local applications should be of such a nature as not to injure the epithelium on which the parasite is seated. "There is a period in the disease," he remarks, "when the aphthous crusts fall off of their own accord, and little more is then required than slightly pencilling or rubbing the affected part with a soft brush dipped in water." Dr. Berg, however, strongly advises us not to wait for this time, but, from the very beginning of the disorder to use chemical and mechanical remedies to destroy the parasite or to hinder its further extension. We cannot, however, expect to remove by direct mechanical interference the fibres of the parasitic growth, which have been shown by Dr. Berg to be so intimately interwoven with the uppermost layers of the epithelium. Our author, therefore, advises that in this stage we should have recourse to those remedies that have a gently solvent and macerating action on the epithelium. Among these he enumerates the alkaline carbonates, and also borax, while he considers the caustic alkalies to be more active in this regard, but, at the same time, less safe in their application. He has also tried solutions of alum, but not with very favorable results. The solutions of corrosive sublimate recommended by Columbier and Eisenmann are, he thinks, too perilous in their application to be ordinarily prescribed. *A certain quantity of acid* appears, from our author's researches, to be requisite for the growth of the parasite, while its increase is hindered by *an excess of acid*, and therefore the stronger mineral acids could not be used to arrest the disease, were not their local action too severe upon those parts of the mucous membrane that are still in a healthy condition. Alkaline remedies, consequently, are Dr. Berg's especial favorites. They counteract the excess of acidity so favorable to the growth of thrush, and, at the same time, soften the epithelium and aid in the throwing off of the aphthous crusts. An excess of alkali in any menstruum has been proved by Dr. Berg's experiments to arrest the increase of the parasite, and when administered to the child, alkalies probably counteract the excessive formation of lactic acid on the mucous membrane, though Dr. Berg does not positively state that they exercise a direct influence on the parasitic growth. Our author uses for this purpose solutions of subcarbonate of soda and borax, varying their strength according to the necessities of the case. The local treatment does not however end here. The constant application of alkaline solutions tends, according to our author, to diminish

the thickness of the epithelial layers by their macerating and solvent properties, and thus the blood, he suspects, comes more immediately into contact with the fluids in the mouth; and he suggests that albumen may in this manner escape and become mixed with mucus; and when it comes in contact with lactic acid it will form one of the most favorable menstrua for the parasitic growth. It is in this way that Dr. Berg explains the frequency of relapses after alkaline treatment has been long employed, and he accordingly advises us then to adopt other remedies, especially astringents and the mineral acids. It is at this period too that nitrate of silver is so useful; and we ourselves have frequently administered it in the proportion of three or four grains to the ℥j of *syrup. rheados*, though perhaps the *infus. salviæ* or some other mucilaginous preparation containing less sugar would be a more appropriate vehiculum.

Finally, Dr. Berg opposes the doctrine of the older writers that thrush is usually followed by an eruption on the skin, and that this is to be regarded as the same exanthematous process transported to the external integument. Our readers will be aware that the exanthematous nature of aphthæ is entirely denied by our author, and in this respect we fully agree with him. The eruption in question is, as Dr. Berg observes, chiefly confined to the nates and the adjacent parts, which in young children are constantly wetted by urine, or are in contact with the fæces. When the secretions from the bowels become disordered and of an acrid character, they will necessarily irritate and inflame the neighbourhood of the anus, and in this way produce what by the vulgar is still regarded as the external and subsequent appearance of thrush. Cleanliness and attention to changing the linen is therefore of the utmost importance in preventing these sequelæ.

The second portion of Dr. Berg's Essay presents us with a nearly complete historical analysis of the descriptions of thrush, from the earliest ages down to the present time. We say nearly a complete analysis, for our author is candid enough to give a full list of those works which he has not been able to consult: these however, are comparatively unimportant, and while the former and purely original part of his Essay is a lasting monument of his great intelligence and unwearied zeal and of his scientific acquirements, we must give due praise also to our author for his diligence in collecting so large a body of information on the subject of aphthæ as is presented by the historical division of his work. His criticisms too on the researches of others are mild and temperate; he is no blind adorer of his favorite theory, for where experiments have turned out unfavorably to his preconceived opinions, he has recorded them as candidly as those which were successful. Dr. Berg has avoided the too common error of the present day in not hurrying with his newly formed doctrines to the press, fearful lest some more fortunate competitor might snatch the anticipated laurels from his brow. He has calmly and steadily pursued his experiments and matured his doctrines, ere he produced them to the medical world; and we trust that he will continue his researches, and extend them to other branches of infantile disease. Such a modest little volume as that now before us will be readily perused by the student who would shrink from a more extended work, while the high character of its contents will ensure for it a cordial reception from the medical world.

In the matter of printing and paper, the little book is superior to many of the works that have emanated from the Swedish press, while the errors

are remarkably few, though quotations from seven or eight different languages are given in the historical portion. The Swedish school already occupies a favorable position in medical science, and we are convinced that this little volume will add to its reputation.

From the date of the publication of this monograph, the name of Dr. Fr. Th. Berg must take its permanent place in the list of original observers who have had the honour and good fortune of conferring a positive benefit on medical science.

Art. XIII.

1. *The Phytologist, a Popular Botanical Miscellany.* Conducted by George Luxford, a.l.s., f.b.s.e.—*London,* 1841-7.
2. *The Zoologist, a Popular Miscellany of Natural History.* Conducted by Edward Newman, f.l.s., z.s., &c.—*London,* 1843-7.

The works above mentioned are issued as monthly journals, and are, for the objects for which they are intended, works of very considerable merit; and though not bearing on the practice of our profession, and so far not constituting that class of work, which as a general rule, we consider the more proper objects of notice in these pages, yet as treating on subjects which are or ought to be interesting to all the members of our profession, we are induced to give them a passing notice, and the more so as we believe they have not received from medical men in general, that attention and support to which we think they are entitled from a class of men whose education and tastes naturally bear on the subjects of which they treat. Though treating on distinct branches of natural history, yet, as being conducted in the same spirit, and being sister productions, and owing their origin to the same spirited individual, who is the proprietor of both as well as the editor of the 'Zoologist,' we feel, on these accounts, disposed to consider them together. Their object is to afford to the numerous class of those who are interested in the subjects of zoology and botany, an opportunity of perusing, at small cost, a readable account of the observations and findings of the day, and of recording their own observations on the same subjects. The pages of these works are open to the small facts which must of necessity be excluded from the elaborate transactions of societies, but which possess an interest for many, who, with the true love of the contemplation and observation of nature, have neither time nor inclination for abstruse researches. To the active members of our profession, who in the course of their studies have acquired a taste for these subjects, but who would find a sustained attentive study of them incompatible with the important and anxious duties of active practice, the works before us are peculiarly adapted,—recording facts interesting to all, and offering their ready pages to observers who may be willing to record what passes before their eyes, but who would shrink from the task of an elaborate paper.

We must be careful, however, by the above remarks, not to mislead by giving the idea that we have only before us the results of the "'*prentice hands*" of natural history tyros. Open, as are these works, to *all*, yet in the table of contents amongst the names of the *unknown*, we see also

those of our well-known and justly celebrated authors, who here record the facts, which, though valuable in themselves, could not find admittance to the manual or condensed synopsis. *Facts*, however, are the words of nature, and whether recorded by the unknown Mr. A. or the celebrated Professor B. if faithfully related, speak the same lesson, and possess an equal value: and in accordance with this view, it is gratifying to find how small a portion of nonsense finds its way to the open pages of these liberal journals, the preface of one of which declares (Zool. vol. i, p. 6,) that "every one who describes a single fact is welcome,—nay more than that,—has a claim to be admitted as a contributor."

The Phytologist we shall notice first as being the elder sister, though we believe hitherto the less sought. Its immediate origin and object we quote from the proprietor's preface.

"The Phytologist owes its existence to the desire of recording and preserving FACTS, OBSERVATIONS, and OPINIONS relating to botany in general, but more especially to British botany. Prior to its commencement *these* had no appropriate receptacle. There was no periodical to which they would be acceptable. For works of a general character they were esteemed too dull; for those of high scientific pretensions they were supposed too trifling. By field-botanists alone were they considered worth preserving: to such the utility, the value of an unpretending monthly journal was most manifest; and these field-botanists—these observers—these labourers in the delightful field of botanical inquiry, have freely availed themselves of its pages: they have done all that was anticipated, and 'the Phytologist' has become the medium of their communications with each other and with the botanical public." (vol. i, p. v.)

The above we have extracted from the preface of the first volume; in that of the last we are informed that its aim is that it should be "a complete register of the botany of Britain," and that with this object information given to other journals is invariably *noticed* in the 'Phytologist.' From an intimate acquaintance with the work from its commencement, we may say that it has from the first fulfilled the intention thus only recently expressed. All newly-discovered species of plants during the last few years, may be found here recorded; and considering how many years British botany has been cultivated, and since the earlier days of Smith, with only an occasional addition to our flora, the number which have of late years been added is truly surprising, and marks the habit of careful analysis, which characterizes the present generation. In addition to these matters of detail, interesting, most interesting, to the collecting botanist, are numerous very readable excursions, and some truly scientific papers on the philosophy of botany, the latter chiefly from the pen of Mr. Hewett Watson, the well-known author of the 'Geography of British Plants;" while with regard to the material for the further consideration of the interesting subjects of the last-named work, ample funds may be here found from all parts of Britain.

A very remarkable fact is recorded by Mr. Spence, (vol. ii, p. 85,) of small branches growing from the disk of a leaf in *Jungermannia juniperina*, we extract the account from the observer's own words.

"I noticed that, at the summit of one of the stems, the leaves were so closely crowded together as to form a sort of coma; and that, from amongst these leaves, which were spread out on every side and recurved, there proceeded several mi-

nute branchlets, some nearly erect, others gracefully pendulous, resembling in miniature the flagelliform ramuli of Mnium undulatum, *Hedw.* I immediately placed the specimen under the microscope, and proceeded to dissect it; but what was my surprise to find that the branchlets issued from the coma, instead of being attached to the main stem, *actually grew on the surface of the leaves*, where they had all the appearance of having been stuck by art!.....They were clad with leaves and stipules, precisely as those of the stem.....The base of each branchlet is slightly dilated into a sort of bulb, which is affixed to the leaf by a very small surface (covering exactly one cellule), and when detached by gentle force, it brings away with it the upper paries of the cellule." (Phytol. ii, p. 85.)

The case is interesting as illustrating that, independently of buds, seeds, and spores, mere nucleated vesicles may in certain cases reproduce all the vegetable organs, a fact now becoming extensively made use of by gardeners who are succeeding in propagating many plants by mere leaves or part of leaves.

We have some interesting remarks (vol. ii, pp. 87, 116) on the subject of the Œnanthe crocata, of which so many instances are on record of its destructive poisonous results in England, while Dr. Christison affirms that in Scotland the plant is not poisonous at all. The existence of the yellow juice from which the species is named is equally capricious. From the limited observations here recorded, it would appear that the yellow juice and active poisonous properties are coincident.

There are numerous papers on monstrosities, illustrative of the theory of the shrewd and philosophical Göthe, on the metamorphoses of the parts of plants or *morphology*, as it is now termed, and also some very elaborate articles on vegetable embryology (vol. i, pp. 625, 675, 731, 881; vol. ii, pp. 914-22.)

The subject of the potato disease has given rise to many communications, some of which (vol. ii, pp. 528, 631, 880) are of considerable interest, and in particular, one of the best we have anywhere seen on this subject, is a communication by Dr. Tyerman, of Bodmin, read before the Royal Cornwall Polytechnic Society, and published in this Journal (vol. ii, p. 826). We fear, however, that the phytologists' communications, like those which have appeared elsewhere, still leave this very mysterious subject in a most unsatisfactory position. The supposition of Mr. Smee, of the influence of an aphis in producing this result, scarcely needs a serious denial; and, on the other hand, though the universal existence of a vegetable parasite (*Botrytis infestans*) be observed, and though it be conceded that it is a true parasite, inasmuch as it subsists on living and not dead vegetable matter, yet for its finding a suitable pabulum, we are told the plant must be in a feeble condition, and it is this *aliquid prius* which, in our opinion, is *the* disease, while, to use the words of Mr. Ward (vol. ii, p. 816), the fungi are, "in fact, the result and not the cause." The explanation of this catastrophe on the untenable and unphilosophical theory of Knight, that plants propagated by division all die off together, is now happily receiving (vol. ii, p. 911) the best refutation in the great diminution, if not the total extinction of the disease this year. This disease has its parallel in the epidemics of man and animals. It has no *affinity* with these, but they are nevertheless its true *analogues*.

During the last two years an arrangement has been made by the Botanical Society of London with this Journal for the publication in its pages of the Papers read before the Society, which are appointed by the council

for publication in the Transactions, by which they both enrich this work and receive a wider circulation.

We feel tempted to enter into the popular subject of progressive development, as relating to botanical subjects, on the limits of species, or on the debated value of certain well known forms of plants whether as being real species or only varieties, subjects on which we have ample matter in the work before us, but to dwell on which we feel would be foreign to our purpose.

We cannot, however, take leave of the 'Phytologist' without noticing the excellence of the wood engravings with which many of the communications are executed, and especially some very excellent articles on our Ferns from the pen of the proprietor of the work who is peculiarly strong on this point; his beautiful work on 'British Ferns' being our best monograph on the subject. We now pass on to make a few remarks on the other work.

The Zoologist. This work, though more recently commenced, has already proved to be the more popular. The illustrations on wood are beautifully executed and very numerous. The subject which more than any other appears to have received the most copious contributions is the very interesting one of the migration of birds, on which the pages of the Zoologist contain a mass of matter affording considerable materials from which to arrive at the knowledge of this very curious fact.

The plumage of birds, as varying by age, has received some attention from some of the contributors, and the contributions noticing the occurrence of various of the rarer insects offer very materially towards furnishing *data* for a very improved knowledge of their geographical distribution.

In a recent communication (p. 1637) we find it stated, that it is the parent cuckoo who ejects from the nest the offspring of the rightful owner, and not the young cuckoo himself. "This," we agree, "differs widely from the common opinion and deserves closer investigation."

Mr. Newman, the editor, has just added some valuable observations in confirmation of a most interesting communication read by Mr. Newport, before the Linnæan Society, in November 1845 (reported p. 1241). It appears by the observation of these gentlemen that certain *coleoptera*, prior to their existence in the ordinary form of the larvæ in this order, first emerge from the ova, or are produced viviparously as active hexapods, which immediately become parasitic on other insects, by which means they are conveyed to the localities—the nests of various bees, for instance—where their proper nourishment is provided, and where, assuming the form of nearly apodous maggot-like larvæ, they acquire their full dimensions, and after the usual transformations emerge as perfect insects. This is a very curious fact, and is particularly striking when viewed in relation to the important details lately brought before the world in that most interesting work by Steenstrup, 'On the Alternation of Generations,' recently issued by the Ray Society.* It appears, moreover, that one link in the chain of observations, as recorded by Messrs. Newport and Newman, is yet wanting, and that the mode of transition from the active parasite to the lazy maggot-larva has not yet been observed. We confess we

* On the Alternation of Generations; or, the Propagation and Development of Animals through Alternate Generations: a peculiar mode of fostering the young in the lower classes of animals. By Joh. Japetus Sm. Steenstrup, Lecturer in the Academy of Söro. Translated from the German version of C. H. Lorenzen, by George Busk.—London, printed for the Ray Society, 1845.

should not be surprised if this should prove an instance of true alternation, and not a *mere* metamorphosis, and that the little parasite is the nurse (*amme*) of the subsequent larvæ.

We find (vol. i, p. 175) several instances recorded of the fact of insects of different species being found *in copula:* this occurrence between *Smerinthus Populi* and *Sm. ocellatus* is mentioned by two observers, and by one of these the hybrid results were obtained. In the same page is recorded the yet more remarkable fact of insects of different *genera* being so found, and these, though of the same tribe, yet insects of such different conformation and habits as *Sphinx Ligustri* and *Smerinthus ocellatus*, "and what renders this the more singular is, that there were several individuals of both sexes of the two species at the same time in the cage." This fact is of importance, as showing that the circumstance of insects being found *in copula* ceases to be of any value for the purpose of deciding, with variable insects, whether certain forms be of the value of species or varieties only. While speaking of insects, we may notice another fact here recorded, (vol. iii, p. 1198.) which is also very interesting; it has reference to the effect of light and colour on caterpillars. *Vanessa Io* was the species chosen for experiment, and numerous individuals were placed in four compartments of a box, one darkened, another having common light, and the others lighted, one through blue and the other through yellow glass. Those in the yellow light appeared less healthy but all formed their chrysalis at the same time; they emerged, however, thus:—"those in the dark all made their appearance first (August 12 and 13), and were all out before a specimen had appeared in the other three cells. Next came those in the common light (August 14 and 15); then the blue and yellow together (August 15—20).

The anatomy of the horse-leech is very elaborately described by the well-known microscopist, the late lamented Mr. John Queckett. As the following passages are interesting we transcribe them:

"The jaws themselves are firmly imbedded in strong muscles which are disposed in such a manner as to move them backwards, and forwards as in the act of biting, with great force and rapidity. . . . . But the dental apparatus of the medicinal leech is much more marked than in this species, and this fact, combined with the striking differences in their alimentary canals (the medicinal leech being furnished with very large *cæca*, which in this are little more than rudimentary,) affords a most satisfactory explanation of the reason why the horse-leech is unfit for medicinal purposes, for instead of the fifteen blunt molar-like teeth, with which the latter is provided, the jaws of the medicinal species are much larger, more convex, and of very much firmer consistence. Each jaw is provided with a row of teeth, varying from sixty to eighty in number; their points or apices are much sharper than those of the finest needles; they are likewise strongly imbedded in the cartilaginous matter of the jaw; in the horse-leech, on the contrary, the teeth appear to be *loosely connected* to the upper part of the jaw." (Vol. i, p. 21.)

In conclusion, we unhesitatingly recommend these works to our readers, and we consider that all who are interested in their respective subjects are very much indebted to Mr. Newman's spirited conduct in originating them, and in persevering till they are become established on a firm footing, as well as for offering them at a price so low as not to be a consideration with any.

Art. XIV.

1. *Researches into the Physical History of Mankind.* By James Cowles Prichard, M.D., F.R.S., M.R.I.A., Corresponding Member of the National Institute of France, &c. &c. Third Edition.—*London,* 1836-47. Five Volumes, 8vo, pp. 2547. With numerous Plates.

2. *The Natural History of Man; comprising Inquiries into the Modifying Influence of Physical and Moral Agencies on the different Tribes of the Human Family.* By James Cowles Prichard, M.D., F.R.S., M.R.I.A., Corresponding Member of the National Institute of France, &c. &c. Second Edition.—*London,* 1845. 8vo, pp. 614. With 49 Plates and 97 Wood Engravings.

In the article under this heading in our last Number, we entered into a general examination of the question, whether the differences at present observable between the several races of men are such as to justify the idea that these races have had diverse origins; or whether they are such as present themselves among other races of animals, whose constitutions and habits (like those of man) are capable of adaptation to a great variety of external conditions. From the evidence adduced by Dr. Prichard and others on this question, we felt ourselves warranted in the conclusion that there is no such fixity in the physical characters of particular races of men, as would be required by the zoologist for their separation as distinct species; the characters in which they differ being such as are known by the analogy of other animals to be very liable to variation; and a sufficient amount of testimony as to their variability in man being derivable from actual observation. Moreover it was asserted that a careful examination of all the tribes included under any one designation,—e. g. the African race, the Malay race, or the American race,—would be quite sufficient to dispel the belief that they agree among themselves and differ from all others in any one character or set of characters, such as would be required for the definition of species; since each of these races includes forms that differ more from each other, than they do from some of those included under other races. And it was also stated that a conformity in external conditions and mode of life among people of different races, is so closely associated (where these influences have been sufficiently long in operation) with conformity in certain physical characters, as to warrant the opinion that the latter is mainly dependent on the former. If this be admitted to be the case, it must be obvious that the notion of the origin of these races in five or any number of pairs, differing from each other in the same manner and degree as the European, the Mongolian, the Negro, &c., now differ, must at once fall to the ground; and that we have no resource but to suppose, either that all mankind originated from one pair, or that if the original parents were more numerous they must have had similar physical and mental constitutions.

In their practical bearing upon the estimation in which we are to regard the races that seem at present most degraded below our own level, we do not see that there is any essential difference between these two latter hypotheses. And we cannot think that the last of them is at once to be set aside as inconsistent with the Scriptural narrative; since the

liberal interpretation of primeval history, which geological researches have shown to be necessary, might almost as well admit the existence of numerous Adams, as coincide with the now generally-received doctrine of well-informed and unprejudiced thinkers, that the Noachian deluge was very limited in its extent, and did not affect by any means the whole either of the men or animals existing on the earth at the time of its occurrence. We are not aware that anything like demonstrative proof of either of these hypotheses can be obtained by scientific investigations. All analogical probability, however, is in favour of a single centre or original pair; since to such centres, as Dr. Prichard shows in his first volume, the naturalist now refers the origin of all his species of plants and animals. And although there would appear, at a certain stage of the investigation, to be greater facility in accounting for the dispersion of mankind from several stocks than from a single one, yet we believe that the more extensively and profoundly the inquiry is carried on, the more will there be found that *fundamental accordance*, in the midst of great diversity of aspect, which indicates original unity. For ourselves we may say that we commenced the examination of the mass of details which Dr. Prichard has brought together, with a prepossession in favour of numerous centres of radiation; but that we have been gradually led to the belief (upon scientific grounds alone) in the probable existence of but a single original pair.

With these preliminary observations, we shall proceed to give our readers such account of the characters of the principal tribes of mankind, as may enable them to form an opinion for themselves; and we shall commence, as Dr. Prichard has done, with a survey of the African nations, which fills the second volume of his Physical History.

The northern region of the African continent to the westward of Tunis, vaguely termed Mount Atlas, is an elevated country, mountainous in some parts, stretching from the great Sandy Desert to the shores of the Mediterranean and Atlantic. Notwithstanding the subsequent admixture of foreign elements, the remains of the aboriginal inhabitants are still sufficiently distinct to enable their affinities to be traced by their language. Of these, the *Berber* races (from which the north of Africa appears to have received the designation of Barbary) may be regarded as the types; the Kabyles of Algiers and Tunis (the people of Abd-el Kader), the Tuaryks of Sahara, and the Shelahs or mountaineers of southern Morocco, being branches of the same stock. Their dialects vary considerably; but not so far as to constitute distinct languages. The small remains preserved to us of the language of the Guanches, or ancient population of the Canary Isles, appear sufficient to show that they formed another branch of the same great Lybian stock. Now these races are all distinguishable from the Negroes on whose region they border, not only by their language, but also in general by the cast of their features, which is decidedly (to use the common designation) Caucasian; and yet in the southern portion of the country they inhabit, which forms part of the torrid zone, and is not of great elevation, their complexion is often perfectly black, and the cast of features approaches that of the Negro. There is perhaps no better example to be anywhere met with, of the influence of climate in modifying physical characters, than that which is here presented to us within a very limited range of country, and amongst races connected by the closest affinities of language. The inhabitants of the lofty table-land called

Mount Aurasius, although they speak the Kabyle language, are fair and ruddy, and their hair of a deep yellow; whilst the Kabyles in general are of a swarthy colour with dark hair. The tribe of Mozabi, also, has been ascertained by Mr. Hodgson to be remarkably white, though belonging to the Kabyle race. On the other hand, some of the Tuaryk inhabitants of particular Oases in the Great Desert, who are almost as insulated from communication with other races as the inhabitants of remote islands in a wide ocean, have hair and features which approach those of the Negroes; although they speak the Berber language with such purity as to forbid the idea of the introduction of these characters by an admixture of races. That these diversities were not original, but have been acquired under the prolonged influence of external conditions, appears obvious from two considerations. In the first place, the investigations of Professor Newman leave no doubt that the Berber language is to be regarded as a primitive branch of the Syro-Arabian or Semitic group,—a coëval dialect with the ancient Syrian, the Phœnician, &c.; and consequently that we must believe the Berber and other allied tribes to have originally formed part of that stock, and to have migrated westwards, along the northern coast of Africa, long before the first colonization of that coast by the Phœnicians. Secondly, the proper *Arab* tribes, who have spread themselves over the same country, at a date long subsequent to the settlement of the Berber races, and who unquestionably maintain their purity of descent, exhibit very obvious indications of the same influences, though in a less striking degree—as might be anticipated from the comparative shortness of the time during which they have been subjected to them.

To those who maintain the complete isolation of the Negro stock from every other, the characters of the ancient Egyptians and of the nations still inhabiting the eastern portion of North Africa must present peculiar difficulties. "There is no ancient people," says Dr. Prichard, "of whose personal characters, manners, and habits of life, we have half so many testimonies as of those of the Egyptians; and yet there is certainly none respecting whose physical history so much difference of opinion has prevailed." Our sources of knowledge respecting this are at least fourfold; namely,—the testimony of ancient travellers,—the delineations and sculptures executed by the Egyptians themselves,—the mummies preserved in their sepulchres,—and the physical characters of the existing Copts. From the first of these sources it may safely be inferred that the Egyptians were a dark-coloured people, and at the same time that great varieties existed among them, which is the case among the Abyssinians and Hindoos in the present day. The prevailing complexion of the Egyptian nation, as represented by their own artists in those wonderful paintings which have been preserved through thousands of years with all the freshness of the original, would seem to be of a red copper or light chocolate colour; closely resembling that which is at present exhibited by the reddest of the Fulah and Kafir tribes now existing in Africa. The same paintings display their very peculiar physiognomy, which is also transmitted to us in their sculptures; and the impression produced on the mind of an unprejudiced observer by these representations (an impression which may be readily verified by a visit to the magnificent collection which our own British Museum contains) is thus stated by Dénon:—

"Instead of the sharp features, the keen, animated, and restless visages, and the lean and active figures of the Arabian, there were to be seen in the land of the Pharoahs full but delicate and voluptuous forms; countenances sedate and placid; round and soft features; with eyes long, almond-shaped, half shut, and languishing, turned up at the outer angles, as if habitually fatigued by the light and heat of the sun; cheeks round; thick lips, full and prominent; mouths large, but cheerful and smiling; complexions dark, ruddy, and coppery; and the whole aspect displaying (as one of the most graphic delineators among modern travellers has observed) the *genuine African character*, of which the Negro is the exaggerated and extreme representation." (Natural History of Man, p. 152.)

The conformation of the cranium of the ancient Egyptians has been a fertile subject of discussion; from the fact, which the researches of Blumenbach and the more recent inquiries of Dr. Morton of Philadelphia appear to have satisfactorily determined, that a very considerable amount of diversity existed amongst individuals or families of this race. Dr. Morton, like Blumenbach, has been struck with the remarkable differences which present themselves among the Egyptian crania; and he refers them to the Pelasgic or most perfect Caucasian form, to the Semitic type as seen in the Hebrew communities, to the proper Egyptian form, and to the true Negro shape. Besides these typical forms, there are others which present intermediate or mingled characters, as the Pelasgic blended with the Egyptian, the Caucasian mixed with the Negro. About eight tenths of the whole number of crania in Dr. Morton's collection belong to the three forms which he terms Pelasgic, Semitic, and Egyptian; and among these the Egyptian greatly predominates, being as eight to one of the Semitic, and as three to two of the Pelasgic. The Negroid conformation preponderates in about eight instances, or one thirteenth of the whole; and it is to be observed with respect to them, that although the osteological development is more or less that of the Negro, the hair is not crisped but long; so that these crania could not have belonged to genuine Negroes.

There is no reason whatever to believe that the large proportion of Egyptian crania exhibiting the most perfect Caucasian form is to be accounted for by any intermixture of races; on the contrary, there seems to be sufficient evidence for the conviction that one collection of crania from the Memphite Necropolis, in which the proportion of Pelasgic crania was sixteen to only seven of the proper Egyptian form, was of an antiquity long anterior to that of the introduction of the Grecian form under the Ptolemaic dynasty.

The characters of that which is regarded by Dr. Morton as the proper Egyptian type of cranial conformation, from its dissimilarity with either the Negro or the Pelasgic, correspond sufficiently well with the prevailing physiognomy of the national paintings and sculptures; and similar features are still preserved in the Coptic race, which is the nearest representative of the ancient Egyptians, although their descent has perhaps not been altogether pure. In this people many points of decided approximation to the physical characters of the Negroes have been observed by those who have had opportunities of studying them; and amongst these we may cite the testimony of Ledyard, a traveller of the highest intelligence and veracity, without any theory to support, who suspected the Copts to be the origin of the Negro race.

We have thus a coincidence between all the sources of evidence with

regard to the physical characters of the ancient Egyptians, so remarkable as to leave but little ground for doubting the conclusion to which they lead, namely, that this nation was more closely united to the Negro race than to any other of the great subdivisions of the human family. Consequently, unless we regard them as having originated from a parentage altogether distinct from that of any of the nations by which they were surrounded, we must believe that they constituted a branch of the great African stock; and thus that they are either elevated forms of the Negro type, or that the Negro races are degraded forms of the Egyptian type. The latter will perhaps appear the most probable hypothesis, when it is borne in mind that, in spite of what appears to be a fundamental dissimilarity of language, the ancient Egyptians and the early inhabitants of India were connected by such numerous and striking features of resemblance in manners and customs, in social and political institutions, in religious observances, and in philosophical belief, as to forbid the idea that such a correspondence can have arisen out of any similarity in external conditions, or can have had its origin in anything else than the same source of mental culture. A remarkable resemblance has also been noticed between the Egyptian features, and those expressed in the most ancient Hindoo sculpture. On the other hand there are many indications of similar relationship between the Egyptians and other African nations; and these relations seem to present themselves also in the general laws governing the structure of their languages. Great difficulties attend the analysis of the ancient Egyptian language, arising from the very imperfect knowledge of it which has been yet attained. The Coptic of the present time is undoubtedly its nearest existing representative; but this has been so changed from its original form by the influence of the various colonizations and conquests which the country has undergone, as to be anything but a trustworthy guide. This much, however, may be stated: that in the tendency to effect modifications in the roots (such as the distinctions of gender, number, and case in nouns, and the various inflections of verbs) by *prefixes*, instead of by terminations as in the great family of Indo-European languages,—a feature of grammatical structure, which (as the readers of our former article will remember) must be regarded as of much greater philological importance than any mere coincidence of vocabulary,—there is a most remarkable correspondence between the Coptic on the one hand and the Kafir (one of the best known of the proper African languages) on the other.

The various nations at present inhabiting the portion of Africa adjacent to the Red Sea exhibit characters which approximate on the one hand to the Negro type, whilst in some respects differing remarkably from it, and on the other side appear to preserve the impress of the stock from which the Egyptians were derived. In fact it is not difficult to show an uninterrupted gradation from the Egyptian to the Negro type, in the several tribes which people the north-eastern portion of the African continent. This gradual change has been supposed by those who regarded the ancient Egyptians as of Caucasian origin, to result from an intermixture of races on the confines of regions originally allotted to each separately. But such an idea is not at all borne out by the close, accurate, and extended observations which have been in progress since the period of the French expedition into Egypt, and which have been of late prosecuted with the

greatest zeal by M. d'Abbadie. These intermediate tribes are not Mulattoes, nor do they at all resemble mixed races; but they have each their distinguishing physical features and peculiarities of language, which mark them out as races distinct from the Negroes on the one hand and from the white races on the other, whilst they present points of resemblance to both. Here as elsewhere we find that the lightest complexions and the most elevated physical conformation are to be found among the inhabitants of the highlands; whilst the dwellers on the low plains beneath the same latitudes present a much nearer approach to the Negroes, not merely in the blackness of their skins, but in the thickness of their lips, the flatness of their noses, and the crispness of their hair.

It must be regarded, then, as an established fact that the Negro races are not separated from other branches of the human family by that definite line which has been commonly supposed to mark them out; and we shall presently find that amongst those nations which are usually included in that designation, the varieties of conformation are nearly as great as among the inhabitants of the northern and north-eastern portions of the continent. But before proceeding with this inquiry, the sense in which the word Negro is to be understood must be properly defined.

"It ought to be remembered that the word Negro is not a national appellation, but denotes the ideal type constituted by the assemblage of certain physical characters, which is exemplified in the natives of Guinea in Western Africa, and in their descendants in the West Indies. When these characteristics are not at all found, it has often been said that the African nations, though black, or nearly black, and woolly-haired, are not Negroes. Thus the Kafirs and Hottentots are said not to be Negroes. On the same principle we ought to except the nations of the interior of Africa or of Soudan, in some of which we could scarcely recognise any considerable resemblance in features to the Negroes of Guinea." (Natural History of Man, p. 292.)

There is an extensive region in Central Africa, bounded on the north by the Great Desert, and on the south by a chain of mountains that stretches across the continent about ten degrees north of the equator, which is inhabited by nations comparatively elevated in the scale of civilization. Our knowledge of these, which previously rested on the exaggerated and therefore incredible accounts of the earlier travellers, and on such information as might be transmitted through the border countries with which they trade, has of late been rendered much more precise by the labours of Mungo Park, Denham, Clapperton, Lander, and other adventurous explorers. As might be anticipated, the physical characters of these nations are far more elevated than those of the tribes that have not emerged from the state of degradation in which they have existed from the time of our first acquaintance with them.

"The tribes in whose prevalent conformation the Negro type is discernible in an exaggerated degree, are uniformly in the lowest stage of human society; they are either ferocious savages, or stupid, sensual, and indolent—such are the Papels, Bulloms, and other rude hordes on the coast of Western Guinea, and in the Bight of Benin, countries where the slave trade has been carried on to the greatest extent and has exercised its usually baneful influence. On the other hand, wherever we hear of a Negro state, the inhabitants of which have attained any considerable degree of improvement in their social condition, we constantly find that their physical characters deviate considerably from the strongly marked or exaggerated type of the Negro. The Ashanti, the Súlima, the Dahomans, are

exemplifications of this remark. The Negroes of Guber and Haúsa, where a considerable degree of civilization has long existed, are perhaps the finest race of genuine Negroes on the whole continent, unless the Iolofs are to be excepted. The Iolofs have been a comparatively civilized people from the era of their first discovery by the Portuguese." (Physical History of Mankind, vol. ii, p. 338.)

So far as our information extends, it seems pretty certain that here, as in other cases, the relative lightness or darkness of complexion among the different tribes depends in great degree upon the elevation of the country which they inhabit. That external conditions have an effect in degrading or elevating the physical conformation, seems evident from the circumstance that those African races which have the Negro character in an exaggerated degree, and which may be said to approach deformity in person—the ugliest blacks with depressed foreheads, flat noses, crooked legs—are in almost every instance either inhabitants of low countries, often of swampy tracts near the sea-coast, where many of them, as the Papels, have scarcely any other means of subsistence than shell-fish and the accidental gifts of the sea; or they occupy thick forests in the hollows beneath lofty chains of mountains, whose alpine heights are inhabited by tribes of lighter complexion and more elevated physical characters. This coincidence is so remarkable, that we feel confident that no unprejudiced person who shall attentively trace it out to its full extent, can continue to feel any hesitation as to the fact. The evidence is further strengthened by the changes which may be proved to have taken place in the physical characters of particular races, in consequence of an alteration in their external conditions. Thus the Barabra or Nubians are descended from the Negro mountaineers of Kordofan, a branch of whom settled on the banks of the Nile about fifteen centuries ago, where they have become partially civilized. Their complexion has become much fairer, and their features bear more of the European stamp; their hair, too, is long and slightly crisp without being woolly. Their descent is attested not merely by historical evidence, but by the community of language which is still traceable; and there appears to be sufficient reason for the belief that this alteration in their characters is not the result of any intermixture with the Arabs or other inhabitants of the Nile-Valley. A similar change has taken place, apparently under nearly corresponding circumstances, in the characters of the Funge, the conquerors of Sennaar; who, though descended from the Shilúkh Negroes, have no longer the genuine characters of the Negro race. One of the peculiarities of this last-mentioned nation is the frequent appearance among them of a red complexion and of red hair. Indeed the individuals thus distinguished are stated to form a separate caste; being known under the name of "El Akmar" or "the Red People." In other parts of Africa the xanthous variety often appears, but does not multiply; individuals thus characterized being like seeds which perish in an uncongenial soil.

Having thus indicated the chief characters which present themselves in the several nations inhabiting the portion of Africa north of the equator, we must briefly notice those of the southern tribes. Of these, however, with few exceptions, our knowledge is much more limited; the interior of South Africa having been as yet unexplored by civilized man; and the only people well known to us being those of a few points on the coast, such as Kongo on the one side and Mozambique on the other, and those

of the southernmost extremity, or the region of the Cape. As we pass southwards from the equatorial region, we find a gradual departure from the proper Negro type; and this is the greater according to the degree of civilization, and the general improvement in external conditions. Thus on the sea-coast, and among the more savage races, as the Makua of Mozambique, much of the true Negro physiognomy prevails; but even here a milder and more intellectual expression is observable than among the natives of Guinea. Their hair is woolly, and their colour black; but their skulls are more vaulted and capacious in the anterior part, and have much less of the prognathous character. Many of the people of Kongo, Benguela, and Loango recede even more from the Negro physiognomy than do the inhabitants of the eastern coast. When we arrive at the Kafirs, the race of warlike nomadic people, which inhabits the eastern parts of South Africa to the northward of the Hottentots, we find so great a departure from the Negro type, that many travellers have regarded them as having had a different origin. The degree of this departure, however, varies greatly in the several tribes; for whilst some of them are black and woolly-headed and display the general characters of the Negro (although not so strongly marked as in the natives of Guinea), others recede considerably both in complexion, features, and form of head from the typical prognathous races, presenting a light brown colour, high forehead, prominent nose, and tall robust stature. The thick lips and curly hair are generally retained, however; but the hair is sometimes of a reddish colour, and the features almost European. Even among the tribes which depart most widely from the Negro type, individuals are found who present a return to it; and it is interesting to remark that the people of Delagoa Bay, though of the Kafir race as their language indicates, being degraded by subjugation, approach the people of Guinea in their physical characters. Generally speaking, the Kafirs are a people very superior to the destitute savages who occupy the insulated hamlets of central Negroland. They are associated together in large communities under chiefs or kings; they have considerable herds of cattle, practise agriculture and gardening; and though some are semi-nomadic, living in large camps or moveable towns, others inhabit fixed residences, build good houses, and lay up stores. They are even acquainted with the use of iron and copper, and have the art of working these metals, and of manufacturing articles of use and ornament. In the country of the Tammahas, Mr. Campbell saw fields of corn several hundred acres in extent, near the town of Mashow, which contains 10,000 people. And the same traveller describes the Murútsi as cultivating sugar and tobacco, manufacturing razors and knives of iron almost approaching steel, building their houses with masonry, and ornamenting them with pillars and mouldings.

Here we see a vast advance in civilization, and a great elevation in physical characters, among a people who cannot be considered in any other light than as improved Negroes. To say that they are of a different stock from the true Negroes, is to make an assertion for which there is no ground whatever, and which is disproved by evidence of the most satisfactory kind. For between the most elevated Kafir and the most degraded Negro, every possible gradation is presented to us as we pass northwards and westwards from Kafir-land towards the Guinea coast; so that it is

impossible to draw any line of demarcation, or establish a distinction between the two races on their respective physical characters. And all the evidence yet collected in regard to their languages tends to show their fundamental analogy. On this head we shall have more to say immediately.

The following is Dr. Prichard's summary of the facts which he has collected with regard to the physical characters of the proper African nations.

"The dark-coloured nations of Africa do not appear to form a distinct race, or a distinct kind of people, separated from all other families of men by a broad line, and uniform amongst themselves, such as we ideally represent under the term Negro. There is, perhaps, not one tribe in which all the characters ascribed to the Negro are found in the highest degree; and in general they are distributed to different races in all manners of ways, and combined in each instance with more or fewer of the characters belonging to the European or the Asiatic. The distinguishing peculiarities of the African nations may be summed up into four heads; viz. the characters of complexion, of hair, of features, and of figure. We have to remark,

"1. That some races, with woolly hair and complexions of a deep black colour, have fine forms, regular and beautiful features, and are, in their figure and countenances, scarcely different from Europeans. Such are the Iolofs near the Senegal, and the race of Guber or of Haûsa in the interior of Súdan. Some tribes of the South African race, as the darkest of the Kafirs, are nearly of this description, as well as some families or tribes in the empire of Kongo; while others have more of the Negro character in their countenances and form.

"2. Other tribes have the form and features similar to those above described: their complexion is black, or a deep olive or copper-colour approaching to black, while their hair, though often crisp and frizzled, is not the least woolly. Such are the Beshari and the Danákil and Hazorta, and the darkest of the Abyssinians.

"3. Other instances have been mentioned, in which the complexion is black, and the features have the Negro type, while the nature of the hair deviates considerably, and is even said to be rather long and in flowing ringlets. Some of the tribes of the Zambesi are of this class.

"4. Among nations whose colour deviates towards a lighter hue, we find some who have woolly hair, with a figure and features approaching to the European. Such are the Bechuana Kafirs, of a light brown complexion. The tawny Hottentots, though not approaching to the European, differ from the Negro. Again, some of the tribes on the Gold Coast and the Slave Coast, and the Ibos in the Bight of Benin, are of a lighter complexion than many other Negroes, while their features are strongly marked with the peculiarities of that race.

"These observations can hardly be reconciled with the hypothesis that the Negroes are one distinct species. We might more easily adopt the notion that there are among them a number of separate species, each distinguished by some peculiarity which another wants; but on that supposition the deviation will be so gradual from the physical characters of other human races, as to undermine the ground on which the opinion of a specific and strongly-marked distinction has been founded. Separate species of organized beings do not pass into each other by insensible degrees." (Physical History of Mankind, vol. ii, p. 342.)

Our acquaintance with the African languages is extremely imperfect; and, strange to say, the materials are most deficient where we might have supposed that they would be most abundant; namely, in reference to the true Negro languages of the Slave Coast. Putting aside the languages of the nations bordering on the Nile, it is of those of the Kafirs and of

the empire of Kongo that most is known. These agree in the most essential characters of affinity; those, namely, which are derived from their grammatical structure. And with these dialects, spoken in the remotest portions of the African continent, the ancient Egyptian seems to be connected by similar relations. Every probability seems to exist, therefore, in favour of a conformity of this nature amongst the African languages in general; although, from the absence of any of those circumstances which tend to *fix* a language or dialect, there may be little correspondence in their vocabularies. If this be the case, it must be admitted to afford a strong corroboration of the opinion that the proper African nations (from which those that are distinctly traceable to the Syro-Arabian stock must be excluded) have had a common origin; and that the ancient Egyptians formed a branch of the same trunk. This view derives further confirmation from the remarkable coincidence in religious notions and observances which may be traced between the ancient Egyptians and the existing inhabitants of remote portions of Africa. Among these may be mentioned the traces of animal worship, the belief in metempsychosis, the observance of circumcision and a variety of other rites, which are described by travellers as existing among the Kafirs, the native people of Madagascar, and some of the tribes inhabiting the western parts of Africa. These, as Dr. Prichard justly remarks, are too extensively diffused, and occur in too many instances, to be attributable to accidental coincidence.

The arguments which are scattered through this most interesting portion of Dr. Prichard's larger work may be thus briefly summed up. The proper African nations must be regarded as having had a common origin, and therefore as belonging to one and the same great family; or they must have had several distinct pairs of ancestors. The latter supposition appears to be entirely refuted by the very gradual nature of the transition between the physical characters of the several nations, which renders it impossible to draw any line between them; and corroborative evidence to the same effect is derived from relations in the languages of remote nations, and from community in religious and other observances. These arguments tell equally, of course, in favour of their community of origin, one with another; and there is no difficulty in accounting on this supposition for the diversities in physical characters which present themselves amongst them, since these diversities are distinctly traceable in many instances to the influence of external conditions, operating through long periods of time, and never transgress the ordinary limits of variation in those species of animals which have a constitution of such a nature as to adapt themselves to different external circumstances. Now if it be once admitted that the proper African nations have a common origin, it is impossible to give assent to the proposition that the original stock is one of decided inferiority to that of the white races; since we find at the present time the closest approximation to the latter, among the more elevated tribes of Africans, both in physical characters, and in moral and intellectual nature; whilst history informs us of the important share which the ancient Egyptians had in the civilization of the world.

In our account of the African nations we have hitherto left out of view the Hottentots and Bushmen, which depart from the general type more widely than the rest; the differences between these and the true Negroes, both in language and physical characters, being such as to lead many

ethnologists to rank them as of distinct origin. Of the real identity of the two nations just named, we gave the chief proofs in our former article; and we shall now, therefore, treat them as constituting a single race. Their physical characters present a remarkable mixture of the Negro with that of the Mongolian type. They are thus described by Dr. Prichard, chiefly on the authority of Mr. Barrow.

"'The Hottentots,' says Mr. Barrow, 'are well proportioned, erect, of delicate and effeminate make; not muscular; their joints and extremities small; their face generally ugly, but different in different families, some having the nose remarkably flat, others considerably raised. Their eyes are of a deep chesnut colour, long and narrow, distant from each other; the inner angle being rounded, as in the Chinese, to whom the Hottentot bears a striking resemblance. The cheek-bones are high and prominent, and with the narrow pointed chin form nearly a triangle.'" (Physical History, vol. i, p. 273.)

"The complexion of the Hottentots is like that of the palest Negro, but still more dilute. Dr. Sparrman compared it to the colour of a person affected with jaundice. Mr. Barrow says it is of a yellowish brown, or of the hue of a faded leaf. According to Mr. Burchell, the complexion of the whole race of Hottentots is of a tawny buff or fawn-colour; such as a painter might imagine that of a Guinea Negro would be, if it were half washed off, and a light tint of ochre put over the remainder.

"The hair of the Hottentots is said by Sparrman to be more woolly than that of the Negroes. It is thus described by Mr. Barrow. 'The hair is of a very singular nature; it does not cover the whole surface of the scalp, but grows in small tufts at certain distances from each other, and when clipped short has the appearance and feel of a hard shoe-brush, except that it is curled and twisted into small round lumps about the size of a marrow-fat pea. When suffered to grow, it hangs on the neck in hard twisted tassels like fringe.'" (Op. cit. vol. ii, p. 278.)

There are few skulls belonging to this race in European collections; but such knowledge as we possess in regard to their conformation accords with the foregoing description in the indication of a decided approximation to the Mongolian type. The width of the orbits, their distance from each other, and the large size of the occipital foramen, are points in which there is a remarkable conformity. The sparse distribution of the hair, the large admixture of olive with black in the complexion, and the great obliquity in the apertures of the eyelids, are characters which seem to separate this remarkable race from all the other African nations; and this separation appears further justifiable on the ground of the peculiarities of their language, of which, however, little is yet known philologically. The buttock-hump or steatopyga is a peculiarity supposed to be characteristic of the Hottentot race, which has attracted much attention. It is a well-ascertained fact, however, that this accumulation of fat is by no means universal amongst the Hottentots and Bushmen,—being, as Mr. Burchell remarks, about as frequent as corpulency among European nations; and also that it occurs in other tribes of Southern Africa, as the Makuani of the Mozambique coast. It cannot, therefore, be regarded as affording the least indication of difference of race; but would seem to result from the operation of the same local influences, whatever be their nature, as those which produce a similar accumulation of fat in the rumps and tails of the sheep inhabiting the same regions.

The relation of the Hottentot race to the other African nations is a question of the highest interest; and one which leads us into the most speculative department of ethnology,—that which relates to the earliest

dispersion of the human race over the earth. Two views may be taken of their origin. First, we may regard them as branches of the general African stock; and must then look for the explanation of their physical peculiarities in the same kind of coincidence between the operation of external agencies and the inherent tendency to variation, as that which has produced the changes in the conformation of other members of the same family of nations to which we have already referred. Or, secondly, we may regard them as legitimate descendants of the Mongolian stock, which, under the direction of the migratory propensity of that race, might have wandered into Africa at a period anterior to the settlement of the ancestors of the proper African nations; and which were afterwards pressed upon by the latter, until they were at last driven down into the remotest corner of the continent, just as the ancient Britons have been pent up in Wales and Cornwall. The former of these suppositions is evidently that to which Dr. Prichard is most inclined; and it certainly derives a remarkable degree of support from the coincidence between the physical characters of the portion of Southern Africa inhabited by the Hottentot races, and that part of the continent of Asia which seems the proper dwelling-place of the Mongolian stock.

The *karroos*, or hard dry elevated plains of Southern Africa, bear a very striking resemblance to the *steppes* of Central Asia; except that the latter have the advantage of a more abundant and equable supply of water, which of course modifies their vegetation. To the hypothesis that this similarity of conditions may have been the chief cause of the points of resemblance which the Hottentot race bears to the Mongolian type, and that they have really the same descent with the proper African nations, it may be objected that there is not that *gradation* of resemblance which we see between the other most distinct forms of the latter; and that the Kafirs, some tribes of which reside in districts of a very similar nature to the Hottentot country, do not show much resemblance to them in physical characters. That the tendency to variation in the original Ethiopian stock does not run in the direction of the Mongolian type, but leads on the one hand towards the more elevated form of the European head, and on the other towards the degraded type of the true Negro, would appear from the observations of Dr. Morton already cited upon Egyptian crania. The entire difference of language between the Hottentots and Kafirs (so far, at least, as our knowledge of them enables us to judge) would seem another cogent argument against the idea of their family relationship.

In favour of the latter of the two suppositions above offered, may be urged the resemblance in physical characters between the Hottentots and the Mongolian nations, which is so strong as to have led Dr. Knox (who has had ample opportunities of seeing these people in their native country) to regard them as of Kalmuck descent; the chief points of difference being the darker complexion and thicker lips, two characters which we know to be among the first to undergo changes in accordance with external influences. It may be further remarked that there is strong ground for the belief (quite independently of theory) that the Hottentots made their way from the north into their present region along the western side of Africa; and that they arrived at the southern extremity many ages before the Kafirs advanced towards the same quarter by the eastern coast.

However this may have been, the latter people have certainly encroached extensively upon the Hottentots; as appears from the circumstance that the names of rivers and of places now within the territories inhabited by the Kosas and Bechuana Kafirs are plainly derived from the Hottentot language. And this gradual encroachment, by which the unfortunate Hottentots have been dispossessed of their rightful territory and have been degraded, one tribe after another, into the wretched condition of savage Bushmen, has continued ever since this part of the world has been known to Europeans; not only on the part of the lawless but enterprising Kafirs, with whom "might makes right," but also by civilized and christianized Europeans, who profess moral and religious principles that cover their practice in this respect with the deepest shame.

We shall not here enter upon the general argument from analogy in favour of the supposition in question; since it will be more appropriately explained and developed, when we have pointed out the peculiarities in the distribution of nations of undoubted Mongolian descent. But we must stop to remark that whatever view we may take of the origin of the Hottentot race, there is no adequate ground for regarding them as descended from a stock primarily distinct from that which has given origin to other branches of the human family. We have seen that there is nothing in their physical characters which really departs more widely from the general type of the African nations, than these depart from each other; the Hottentots being ordinarily classed with the Negro races by those who have not attended to their peculiar distinctive features. And Dr. Prichard has brought together a most interesting body of evidence, derived chiefly from the accounts of the various missionaries who have devoted themselves to the civilization and christianization of the South African races (the most successful of whom have undoubtedly been the United Brethren), in proof of the fact that there is no essential psychological difference between the Hottentot races, even when degraded to the condition of Bushmen, and those which we are accustomed to regard as far more elevated in the mental scale. We must attempt to estimate the character of the Hottentot race, not from their present degraded condition, after the cruelty and oppression which they have endured from European colonists during so many generations have broken their spirit and reduced them to bondage or exile, but from the much more favorable accounts left by the older writers of the condition of these tribes soon after the first settlement of the Dutch colony, which are singularly at variance with the descriptions given by later writers. Copious extracts from these will be found in both of Dr. Prichard's works; as well as a most interesting collection of evidence as to their capability of receiving religious instruction.

The third volume of Dr. Prichard's Physical History is occupied with his account of the nations of Europe; and the fourth with that of the Asiatic nations. As there is a considerable mixture of races, however, in these two quarters of the world, and as our object is not so much to trace out the history of individual tribes, as to take a general survey of the whole, it will be more convenient for us to group the several nations, not according to the quarter of the globe they inhabit, but according to their agreement in physical and glottological characters. Only two principal types of physical conformation can be discovered amongst the people of

Europe and Asia; those, namely, which are commonly termed the *Caucasian* and the *Mongolian*. Both these names, however, are in the highest degree inappropriate, as will hereafter appear; and we only employ them, in preference to the *Iranian* and *Turanian* of Dr. Prichard, in order that our meaning may be the more readily apprehended by our readers. For the first we may, however, use the term Indo-Atlantic; which expresses sufficiently well the present geographical distribution of those nations which possess, with few exceptions, that type of bodily configuration of which the Greeks seem to display the most perfect model. These nations inhabit the south-western portion of the Asiatic continent,—that, namely, which includes the whole of Hindoostan and the Deccan, as well as Persia and Arabia,—together with the north of Africa, and nearly the whole of Europe. The Mongolian nations, on the other hand, inhabit the centre, northern, and eastern portions of the Asiatic continent, and extend themselves into Northern Europe. We shall afterwards find that the distribution of this race is probably still more extensive.

The skulls of the *Indo-Atlantic* nations generally differ from those of other races in having all their parts moderately developed; and in their oval figure, and the absence of any considerable antero-posterior or lateral projection, they hold a middle place in many respects between the crania of the Negro races and those of the Northern Asiatics. But within the limits of this general form there is a very large amount of variation, both among particular tribes, and among individuals of the same tribe; and many of these variations lead us towards the other types of conformation. In some cases there is distinct evidence that the original type has degenerated. No uniformity whatever exists amongst the Indo-European nations in regard to *colour;* for they present every intermediate gradation between the white or florid hue of the northern Europeans to the jet black of many tribes in Libya and to the south of Mount Atlas. As remarked in our former article, the close relation which we everywhere find in these nations between colour and climate (whether the latter depends upon latitude or elevation above the sea), coupled with the known changes which have taken place in the hue of tribes or nations of whose migration we have historical evidence, leaves no reasonable doubt that this character is one peculiarly liable to variation, and not in the least to be relied upon as an indication of diversity or unity of race. The *hair* is generally long and flexible; but variations present themselves in this respect also, both amongst the larger groups and between individuals. Thus we meet, every now and then, amongst the fairer nations, with individuals having the short, crisp, black hair of the Negro, who have not a particle of Negro blood in their veins; or the dark, straight, but scanty hair of the Kalmuck; whilst, on the other hand, red or auburn hair is not uncommon in individuals or even whole families amongst nations in which the characteristic hue of the hair is black.

This great subdivision of the human family must be further separated into two groups of nations; which, though they agree with each other in their general physical conformation, differ entirely as regards their language. And this difference existed in the remotest periods of which we have any historical or even traditionary knowledge. The first of these groups consists of the *Syro-Arabian* nations, which anciently extended over the south-western portions of Asia (including the countries lying

between the Euxine and the Indian Ocean, and bounded on the east by Armenia and Persia), and early spread themselves over Northern Africa. The bond of union amongst these nations is their language, which differs altogether from that of all other human races, whilst its varieties are much more closely allied to each other than we usually find elsewhere. In the opinion of Baron Larrey, who has had ample opportunities of observation, the skulls of the existing Arabian race furnish at present the most complete type of the human head; combining the fullest development of the cranial cavity, with the most perfect organization of the parts subservient to the senses: and he states that experience has proved to him that their intellectual perfectibility is proportional to this higher development of physical organization, a great aptitude for acquiring knowledge being united with extraordinary acuteness in the use of the organs of sense. We find, however, that the habits and manners of different branches of this family of nations have varied very widely, from the earliest period of which we have any account of them; some being nomadic, others agricultural, and a third class devoted to foreign commerce and domestic manufactures. Their physical characters, also, have undergone considerable modifications. The varieties of language afford the best means of dividing this assemblage into subordinate groups; of which the following are the principal.

"1. The northern and eastern branch, termed *Aramæan* or *Syrian*. The Syriac of the versions and the Chaldee of the late Scriptures of the Old Testament and of the Targums, are specimens of this language from early times. If the Cappadocians were really Syrians, this was doubtless their idiom. It appears to have been the original idiom of the Hebrews, until the Abramidæ occupied the Promised Land in Canaan, and adopted, as it would appear, from its previous inhabitants, the Canaanitish or proper Hebrew.

"2. The Hebrew, Canaanitish or Phœnician, for they were the same or very nearly the same, as Gesenius has lately proved, was spoken by the Hebrews from the time when they adopted it on their arrival in Palestine to the Babylonish captivity, when they are supposed to have exchanged it for Chaldee, or to have returned to the use of a dialect more akin to their primitive ante-Abramic speech. This language, with perhaps some very slight variations, was the idiom of the Sidonian and Tyrian states, and of Carthage and the Carthaginian settlements. Even the language of Numidia is supposed by Gesenius to have been a pure or nearly pure Hebrew.

"3. The third division of Syro-Arabian dialects are those of the Arabic properly so called, including the Mogrebbyn or Western Arabian language.

"4. It is supposed that a fourth language, belonging to the Syro-Arabian stem, has been discovered lately in the southern parts of Arabia. . . . . These discoveries render it probable that an ancient language cognate with the Syriac, and the Hebrew, and Arabic, but distinct from all, and having a character of its own, once prevailed over an extensive region to the southward of that occupied by the proper Arabic." (Natural History of Man, pp. 141-4.)

It has been also rendered almost certain, through the analysis made of the *Berber* language of North Africa by Professor F. Newman, that this is to be considered as a fifth cognate dialect of the great Syro-Arabian stock; although it differs more from the rest than they do from each other, probably on account of its complete separation from them at an earlier period. And it even appears from the acute researches of the same accomplished scholar, that the languages spoken by some of the Negro

nations of Central Africa are certainly, though more distantly, related to the Syro-Arabian stock.

The *present* distribution of the Semitic nations (as they are frequently but erroneously termed) does not at all correspond with the above subdivisions, which express their condition at the earliest period of which we have any knowledge of them.

"Of the several nations who are connected by this community of language, some who were formerly celebrated have become nearly extinct; while others have spread themselves, either as the exiled followers of a persecuted faith, or as the conquering apostles of a victorious one, over the world, and seem destined, through the energy of their invincible mind, to survive to the end of time. The Syrian race scarcely exists: their language only survives in some districts on the borders of Kurdistan; everywhere else they have been lost under the predominant Arabs. The Homerites in Arabia, if there they exist, are little known; the Abyssinian Homerites are the only inhabitants of the province of Tigre, to the eastward of the Tacazze, whose idiom still resembles the ancient Gheez. The Arabs, who spread Islam by their victories from the Atlantic to the Ganges, and the Jews, who are wanderers over the whole world, are perhaps now more numerous than were even their forefathers." (Natural History of Man, p. 145.)

We referred in our former article (p. 69) to the important facts in regard to the influence of climate on complexion, resulting from the comparison of the different branches of the Jewish race, now dispersed through so many diverse climates, yet preserving so completely their national distinctness; and when treating of the nations of Syro-Arabian descent in Northern Africa, we have found the same general correspondence to hold good. A detailed survey of the characters of the proper Arab branch, now so extensively diffused, would lead to the same conclusion.

The *Indo-European* group of nations is more widely spread, and consists of more numerous tribes, than the preceding. Nations who speak languages of cognate origin, and who are proved by that connecting bond to be descendants of one original stock, are now spread from the mouth of the Ganges to the British islands and the northern extremities of Scandinavia. This stock seems to have been early divided into two primary branches: the *northern* or Median; and the *southern* or *Indian*. Between the original languages of these races a marked resemblance can be traced; the *Zend*, or earliest idiom of the Medes, Persians, and Bactrians, being intimately related to the *Sanskrit* or ancient language of Hindoostan. And the traditions of both races point to contiguous regions as their original seat; the earliest records of the Persians indicating that they migrated westwards from a spot in the ancient Bactria, not far from Balkh, to the westward of the Indus; whilst the traditions of the Brahmans refer the origin of the Hindoos to the north-western part of the country lying between the Himalaya and the Vindhya mountains, whence they afterwards moved eastwards and southwards into the peninsula of India. Both the Median and Indian nations, moreover, originally designated themselves as *Arians;* a term of which Dr. Prichard suggests the use, as a concise appellation for the Indo-European group of nations.

The physical history of the Hindoo nation, which must be regarded as the nearest representative of the ancient Indian branch, is interesting, as presenting within moderately narrow limits great diversity in those points of conformation, which some have held to be most permanent and definite

characters for the classification of the human races into distinct species. Many circumstances favour this diversity; especially the distinction of castes, which perpetuates the same mode of life in particular families from generation to generation; and the marked differences of climate between the several regions, as when we compare the mountainous elevations of Kashmir and Kafiristan with the plains bordering the great rivers of India. In many instances the origin of the varieties which we encounter can be clearly traced by historical evidence, as well as by affinities of language and conformation; and it cannot be questioned that Hindoos as black as Negroes, others of a copper colour, others little darker than the inhabitants of Southern Europe, and others of a fair complexion with blue eyes and auburn or even red hair, have all had a common parentage; some having become darker than their ancestors, and others lighter, generally in accordance with changes in their residence and habits. There would also seem to be a stronger tendency to variation in complexion amongst families or individuals of the same descent and living under the same circumstances, than exists in the fairer nations. It is especially, however, among the high caste people of the northern parts of India that the fairest complexions are to be met with.

Of the ancient Median race, the main or *Persian* stock has fallen under Turkish domination; and the so-called Persians of the principal towns are in reality Turks. But a large part of the country is still inhabited by the descendants of the ancient Persians, who are termed Kuzzilbashes by the Turks. There are several races inhabiting the borders of Persia, but not properly belonging to the Persian nation,—though probably cognate with them,—which present strongly-marked diversities of conformation, analogous to those already noticed. Among these are the Affghans, inhabiting the mountainous region to the northward of the low country of the Punjab or plain of the Indus, speaking a dialect derived from the ancient Zend, and having every variety of complexion from that of the dark Indian to that of the fair European; the Kurds, inhabiting the high mountainous tract, intersected by deep valleys, which lies between the great upland of Persia and the plains of Mesopotamia, divided into many tribes, which differ from each other in language and in degrees of barbarism and improvement, but which have a peculiar idiom which shows them to belong to the Arian race; and the Armenians, now constituting a numerous and civilized people, connected by early tradition with the Medes and Persians, speaking an idiom allied to the most ancient dialects of the human race, and remarkable for their fine forms, fair complexions, and regularity of features.

The collective body of *European* nations are, as it is now almost universally admitted, a colony, or a series of colonies of the Arian or Indo-Median race. The evidence of this position is chiefly derived from the fact that the European languages, in spite of their diversity, constitute but one philological group; being united not merely by their community in some of the most important roots or primary words, but also by the general similarity of their grammatical construction; and that they obviously have either the Sanskrit or the Zend as a common source or fountain. The conclusion, being strengthened by the general similarity in cranial conformation which presents itself, with subordinate differences, amongst all the nations to which we should thus attribute an Arian origin,

becomes almost irresistible; although neither history nor tradition enable us to trace with any certainty at what time nor by what path the Arian colonisers passed into Europe. The European branches of the Indo-European stock are arranged by Dr. Prichard, by the combined assistance of historical and philological evidence, under the following heads:—1. The *Celtic* nations of Western Europe, represented at the present time by the Irish, Scots, and Manks, which form one subdivision; and by the Welsh, and Armoricans or Bretons, which constitute another.—2. The *Germanic* family, which, in like manner, consisted of two principal subdivisions; the Northmen, or ancestors of the Icelanders, Norwegians, Swedes, and Danes; and the proper Teutonic, from which sprang the Saxon or Western German, the Suevian or High German, and the Gothic or Eastern clan.—3. The *Pruthenian* or Old Prussian, including the Lettish, Lithuanian, and Pruthenian proper.—4. The *Slavic* or *Slavonian*, consisting of a western and an eastern branch; from the former of which sprang the Poles, Bohemians, and other nations near the Baltic; and from the latter the Russians, Servians, and other tribes.—5. The *Italian* family of nations, including the Umbrian, Oscan or Sabine, Latin, Sicelian or Œnotrian, and other people of Italy, with the exception of the Tuscans.—6. The *Rassenian* or *Tuscan*, who were a people of different physical characters from the other inhabitants of Italia, and spoke a language that seems to have had little or no affinity to any of the other dialects of the Peninsula.—7. The *Illyrian*, from which have descended the Thracians, Arnaouts, and Albanians.—8. The *Hellenic*, including the nations of ancient Greece, which formed the cradle for the development of most of the elements of modern civilization; and in which the intellect first obtained a permanent predominance over mere animal power. The classical languages of Greece and Italy appear more referable to the Sanskrit or Indian stock than to the Median; whilst the Germanic languages seem rather to have originated in the latter. Of all the extant European dialects, the Lettish and Lithuanian approach most nearly to the ancient Sanskrit. These resemblances, however, cannot be held conclusive with regard to the origin of the several European races in one or the other of the great Asiatic branches; for it is a well-established fact, that each member of the Indo-European class of languages bears traits of particular affinity, or rather of peculiar resemblance, to nearly every other member. Each also contains an admixture of a barbaric or foreign element; the proportion of which varies in different cases. This admixture may possibly have been derived from the languages of the people who inhabited Europe at the time of the first immigration of the Arian races; since there is much ground for the belief, as we shall hereafter see, that this quarter of the globe was by no means in a vacant state at that period, although the traces of the previous population have been generally lost.

Putting aside for a time the question of the mutual relationship of the nations which at the present time inhabit the central, northern, and eastern parts of Asia, and of those which we find still holding to certain countries in Northern Europe, or whose traces we meet in the south-west, Dr. Prichard includes them under the general term of Allophylian races.

"The Allophylian races are spread through all the remotest regions of the old continent, to the northward, eastward, and westward of the Iranian nations, whom they seem everywhere to have preceded, so that they appear, in compari-

son with the Indo-European colonies, in the light of aboriginal or native inhabitants, vanquished and often driven into remote and mountainous tracts by more powerful invading tribes. The latter seem to have been everywhere superior to them in mental endowments. Some of the Indo-European nations, indeed, retained or acquired many characteristics of barbarism and ferocity; but with these they all joined marks of an early intellectual development, particularly a higher culture of language as an instrument of thought as well as of human intercourse.

"Though rude in respect to many of the arts of life, the Indo-European nations appear to have brought with them a much higher degree of mental culture than the Allophylian races possessed before the Iranian tribes were spread among them. Even the most simple of them had a national poetry, and a culture of language and thought altogether surprising, when compared with their external manners and condition, as far as they can be known or properly estimated. They had bards, or scalds, vates, ἀοιδοί, who, under a divine impulse were supposed to celebrate the history of ancient times and connect them with revelations of the future, and with a refined and metaphysical system of dogmas, of which it is difficult to imagine the original source. Among these, in the west as well as in the east, the metempsychosis held a conspicuous place, implying faith in an after-life of rewards and punishments, and a moral government of the world. With this was connected, among most if not all the Iranian nations, the notion that the material universe had undergone, and was destined to undergo a repetition of catastrophes by fire and water, and to be renewed in fresh beauty, when a golden age was to commence, destined in its turn to inevitable corruption and decay. The emanation of all beings from the soul of the universe, and their refusion into it,—a doctrine which appears to have formed an essential part of this system wherever it was preserved in a tolerably entire state—borders closely on a species of pantheism and fatalism; and is strongly contrasted with the theology of the Syro-Arabian nations, who only of all mankind appear in the early ages to have recognised the existence of an extra-mundane God, and a real maker and originator of the universe. The Iranian system, which was a religion of poetry and philosophy, and which everywhere produced an abundant growth of mythology, was still more strongly contrasted with the superstition of Shamanism, connected with a belief in sorcery and spells, and the rude materialism which prevailed among all the Allophylian tribes. This last form of superstition, resembling in many particulars the fetissism of Africa, appears to have differed in different parts of Europe and Asia, among the rude aborigines of which it was once universally spread. It has given way, in most instances, to the influence of more systematic modes of belief, introduced by more polished nations; but Buddhism, which is a form of the Indo-European system, has not extinguished in China and Japan the original superstitions of Tao-sse and Sin-mu; nor did Islam, though early adopted by the whole Turkish race, triumph over all the native superstitions of Siberia. Among all nations of Asia and Europe, we discover an order of persons who were venerated as mediators between the invisible powers and their fellow-mortals; but the priests, whether Druids, or Brahmans, or Magi of the cultivated nations, were revered as the depositories of ancient sacred lore, of primitive traditions, of the will of the Gods expressed of old to the first men and handed down, either orally in divine poems, or preserved in a sacred literature known only to the initiated; they were the constituted intercessors between weak mortals and the powers which govern the universe, and which they only knew how to approach by ordained rites. In most instances they were an hereditary caste, into which none were admissible who were aliens to the sacred race. Far different were the twice-born sages of the Hindoos, who sprang from the head of Brahma to govern the multitude that issued from his legs and feet, from the sorcerers or shamans of the northern worshippers of fetisses, who by horrible distortions, cries, and yells, by cutting themselves with knives, by whirling and swooning, assumed the appearance of something preternatural and portentous, and impressed the

multitude with a notion that they were possessed by demons. Of this latter description were the wizards of the old Finnish races, whose successors, the sorcerers and witches of Lapland, sell wind to the English mariners. Such were the angekoks of the Esquimaux, discovered by the missionaries to Greenland; and such are the shamans of all the eastern and northern countries of Asia, whither neither Buddhism nor Islam have yet penetrated. By such traits as these, which display more fully and certainly than external manners and the modes of sustaining life, the culture or rudeness of the mind, the barbaric tribes, dispersed over all the extreme parts of the ancient continent, are distinguished from the cultivated nations of Upper Asia, and from the European races allied to them in language and descent." (Physical History of Mankind, vol. ii, pp. 9-13.)

We have given this extract in full, because it shows how particular forms of religious belief may become, when we look below their superficial resemblances or differences and investigate their fundamental characters, a most important means of tracing affinity amongst remote nations. Any assistance of this kind is most welcome as an aid to the ethnologist in his investigations into the mutual relationships of the Allophylian nations, whose history is involved in greater obscurity than that of the tribes which belong to the Indo-European family; the sources of information respecting them being more scanty and difficult of access, and in many instances remaining yet unexplored. We shall commence with a brief account of that group of nations which are commonly regarded as forming the *Mongolian* race; being characterized, more or less decidedly, by the pyramidal form of the skull, and by the xanthous or olive complexion. The original seat of these nations appears to have been the great central plain of Asia, in which all the great rivers of that continent have their sources, whether they are afterwards to flow northwards into the Arctic ocean, eastwards to the sea of Okhotsk and Japan, or southward into the Indian ocean. This extensive district, constituting perhaps one fourth part of the whole area of the Asiatic continent, seems to have been tenanted originally by nomadic tribes, closely resembling in their physical characters, their mental habitudes, and their mode of life, those which still inhabit the same region. The increase of population, however, caused various offsets to diverge from this centre; and these have undergone a greater or less degree of modification, according to the length of time which has elapsed since their separation, and the degree of change in the external conditions to which they have been subjected. Some of these migrations effected or gave occasion to some of the most remarkable revolutions recorded in history; and there seems good reason to believe that similar excursions, at earlier periods of the history of the race, were the means of spreading a human population through countries as yet untrodden by the foot of man. In addition to the physical characters which have been already mentioned as distinguishing these nations, we may stop to remark that the greater proportional width of the head at the plane of the zygomatic arches affords room for greater development of the organs of sense; and in correspondence with this, the pastoral tribes of Northern Asia display a remarkable perfection in the sensitive faculties.

"The Kalmucks especially, according to Pallas, have the finest sense of smelling, the most perfect hearing, and a singularly penetrating and extensive sight; they see objects on their steppes with the naked eye which are invisible to Europeans even with the aid of glasses, and they discover, by smell, fires or the scent of a camp at a surprising distance; and by their hearing they recognise at

a remote space the movement of an enemy or of a herd of cattle." (Physical History of Mankind, vol. iii, p. 407.)

Now, it is not a little remarkable that the Hottentots are especially characterized by the possession of this same acuteness of the senses, especially that of sight, which has been noticed in almost precisely the same terms by Europeans whose expeditions they have accompanied. Whether we regard this coincidence,—like the resemblance already noticed in the form of the head, the position of the eyes, the tawny complexion, and the scantiness of the hair,—as indicating an identity of race between the Hottentots and Mongolians, or as resulting from the similar influence of corresponding external conditions, the fact is remarkable and instructive. The American Indians, too, are well known to possess remarkable acuteness of the special senses.

The group of nations whose origin from the central Asiatic plateau seems most distinct, appears to have early consisted of five tribes, who spoke languages which were long regarded as altogether dissimilar, but which are now known to have a common foundation. Of these races three—namely, the Turkish, Mongolian, and Tungusian—occupied the central portion of the plateau; the north-western border was peopled by the Ugorian or Ugrian race, which was probably an early offset from the central stock, retaining least affinity with it; whilst the south-eastern was tenanted by the Bhotiya or mountain people, whose descendants border upon Hindoostan.—The *Ugorian* or *Ugrian* race seems to have extended itself in a north-westerly direction at a very early period, since we find its several tribes in possession of the whole region extending from the Baltic to the rivers Obi and Irtish in Siberia, in times long anterior to the arrival of the German and Slavic nations in the north of Europe. The western branch constitutes the peculiar people still tenanting the northern Baltic countries under the names of Fins or Lappes; to the relative degrees of civilization of these two people of similar origin, and the change in physical characters which has taken place in the former, we have already adverted (p. 75). The people called by the Russians *Tschudes* were of the same stock. Further eastwards the name of *Ugrians* predominated; and these gave origin to the designation of Ogres, which were prototypes of savage monsters, dwellers in forests and mountains, whose name is better known in its fabulous than in its historical import. From the eastern branches of this race, namely, the Vogouls of the Uralian mountains, and the Ostiaks on the Obi, are descended the Magyars or Hungarians, a warlike and energetic people, unlike their kindred in the north; in whom a long abode in the centre of Europe has developed the physical and mental characters of the Arian race, proving the (so-called) Mongolian stock to be susceptible of the highest culture.

The *Turkish* tribes have been commonly but erroneously termed Tartars; the real Tartars (or rather Tatars), however, were a people belonging to the proper Mongolian tribe. The relation of the Turks of Europe and Western Asia to the tribes which still preserve their nomade life in the vast regions that lie north of Persia and Hindoostan has been already discussed (p. 74), and we shall therefore pass on to speak of the proper *Mongolian* division. This constitutes but a very small part of the entire group; for though it covers an extensive tract of country, reaching from Lake Baikal to the north-western boundary of China, yet the people are

insignificant in numbers when compared with the Turkish tribes. They still everywhere retain their nomade habits, and exhibit the physical characters of the group in a very well-marked degree, and with considerable uniformity. The *Tungusians* wander over the immense mountainous regions which extend north-eastward from Lake Baikal to the sea of Okhotsk, and are also dispersed along the courses of the great rivers which discharge themselves into the Icy Sea. The tribes within the limits of the Chinese dominion bear the general name of Mantschu (or Manchew), and are improperly called Tartars; the remainder form part of the Russian empire. The former established themselves, in the sixteenth century, as the conquerors of the Chinese empire, and have continued to govern it ever since, and although they still retain some of the characters of the nomadic Tungusians, these are usually much softened down, under the influence of a settled and more civilized life. The *Bhotiyahs* are the nation, often termed Tartars, who inhabit a great part of Tibet and the Himalayan chain; they have a strongly-marked Mongolian countenance; but in vigour of body and in stature they are very superior to the nomadic races to which they are related.

It is very probable, though the fact (from the paucity of information hitherto brought to bear upon the subject) cannot be yet regarded as proved, that all the other tribes on the northern border of Asia are allied to the same class of nations.

The vast region of Asia forming the south-eastern corner of that continent is inhabited by races of people who resemble each other so strongly in moral and physical peculiarities, and in the general character of their languages, as to give rise to a suspicion that they all belong to one stock, which may be designated as that of the Indo-Chinese nations. Under this category may be included the *Chinese*, which have long been the most numerous and powerful of the group, the *Koreans*, and the *Japanese*, all of which present the physical characters of the Mongolian type, though in a form more or less mitigated and variable. The races of the great *Indo-Chinese* peninsula constitute another subdivision, intimately related to the Chinese, both in language and physical characters; such are the people of Siam, Cochin China, Kambogia, Pegu, &c. The physical characters of these tribes present much of that departure from one constant original type, which we elsewhere notice in people that are making advances in civilization. The great peninsula of Hindoostan appears to have been peopled long previously to the immigration of the Hindoo or Arian race; and remains of the aborigines are still found in the hilly parts of Northern India, in the Deccan, and in Ceylon. They constitute numerous tribes which are now for the most part isolated from each other, and which seem to have nothing in common; and very different degrees of civilization are to be met with among them. Thus, of the tribes making up the aboriginal population of Southern Ceylon, namely, the Cingalese, the Kandians, and the Vaidahs, the two former are considerably advanced, in proportion to the last which remain in a state of complete barbarism. Yet there is evidence derived from affinities of language, peculiar customs and traditions, &c., that they had a common origin. Another important aboriginal race is the Tamulian, inhabiting the northern part of Ceylon and the southern portion of the Deccan, and having offsets in different parts of India. Numerous tribes not of Hindoo stock are also found in-

habiting tracts of no great extent in the valley of the Brahmaputra, and in the countries near the mouth and lower course of that river and the borders of the Bay of Bengal.

The relations of the Indo-Chinese nations to the great nomadic group of Central Asia are by no means clear. The physical characters of the Chinese would decidedly indicate their descent from the Mongolian stock, and their early history refers to the eastern border of the great central plateau as their original seat; but the Mongolian type is, as it were, softened down or improved; and in many instances the countenance and form of head approach those of Europeans. This departure is still more evident in some of the tribes of the Indo-Chinese peninsula, which are still connected with the Chinese nation by affinities of language. And among the aborigines of Hindoostan, some present the Indo-Chinese form, whilst others approximate the Hindoos. It is not difficult to account for these changes, on the supposition that they have been induced in the races most remote from the central stock by the alteration in their external conditions, just as we find similar changes to have been effected in the Turks, Fins, and Magyars. It is curious to observe that, in all instances, the advance seems to be towards the European or (so-called) Caucasian type. Notwithstanding the indications of affinity, however, to the great central Nomadic group, the great dissimilarity in the languages of the Indo-Chinese nations (of which the Chinese may be taken as the most perfect example) to those of most other Asiatic nations, interposes a great difficulty in the way of the determination of their actual relationship. These languages have been sometimes termed, for the sake of distinction, *monosyllabic;* but this term does not express their real peculiarity; since the *roots* of most other languages are generally monosyllabic, words of two or more syllables being usually either compound (that is, made by the union of two or more roots), or formed by inflections of the roots.

"The main peculiarity of the monosyllabic idioms consists, as M. de Humboldt observes, in the double circumstance of the total want of affixes or syllables subjoined for the purposes of inflection, and the peculiar manner of pronunciation, by which, even when the mind connects ideas, the sounds of syllables are left separate, and, by a distinct intonation or accentuation, still retain the character of individual or particular words. It has been remarked by the great philologer whom I have just cited, that the Chinese and the Sanskrit languages exemplify the two most opposite methods of construction. The Sanskrit denotes all the relations and connexions of words and of ideas by grammatical forms, written and expressed in pronunciation; the Chinese leaves the perception of these relations to be the work of the mind. The use of some particles being excepted, of which the Chinese can, however, in a great degree dispense, this language expresses all grammatical relations of words by mere *position*, fixed according to certain invariable rules, and by the explanation of sense which the context or connexion of the sentence supplies." (Physical History of Mankind, vol. iv, p. 541.)

The perpetuation of this kind of language appears to be partly a result of the use of hieroglyphic symbols instead of alphabetic writing; each of these symbols standing for a distinct word, which would thus continue to be remembered and recognised in its individual sense. Now although there is so wide a separation between the Chinese and allied languages, and those of the Nomadic tribes of Central Asia, yet the Bhotiya or ancient Tibetian language seems to present relations to both; so as to warrant the conjecture that the Indo-Chinese nations are really offsets from

the great central stock, the Bhotiya probably representing their original form. The similarity of the Tamulian language of Hindoostan to the same type justifies the same conclusion with regard to some at least of the aboriginal tribes of the Indian peninsula. That small groups of aborigines, separated from each other at a very early period, before their language had become fixed by advances in civilization, should gradually lose this connecting link by the almost entire alteration in their vocabulary which would take place in successive generations, under the influence of new circumstances, and more particularly from intercourse with surrounding people speaking languages of an entirely different character, cannot be regarded as unlikely. Whether any traces of fundamental connexion can yet be discerned amongst all the languages of the supposed aboriginal tribes of Hindoostan, we have no sufficient data to determine.

Passing from south-eastern Asia to the extreme western border of Europe, we find on the flanks of the Pyrenean range the remains of a people now known under the name of the Basques or Biscayans. There is no doubt that these are the representatives of the ancient Iberians, a people who inhabited the northern coasts of the Mediterranean, from Italy westwards, before their occupation by the Celtic nations. The national appellation of these people in their own idiom is *Euskaldunes;* and they term their language the Euskara or Euskarian speech. This language has been attentively studied, especially by the late celebrated Baron William von Humboldt; who, during a residence in Spain, devoted himself to this subject, and to the collection of materials illustrative of the ancient literature of the Iberians. He has hence come to the conclusion, which corresponds with that founded upon other data, that the Iberians belong to the very earliest stock of European nations; and so far from their language having been derived (as some writers have supposed) from the Celtic, it must have been in existence at a period long anterior to the migration of the Celtic nations into Western Europe. But the Euskarian has some remarkable traits of resemblance to the Finnish language, and thence to the general family of languages in High Asia; corresponding with them in two remarkable peculiarities,—the first being the absence of difference of gender in nouns substantive, and the second being the practice of subjoining to nouns all particles which modify their meaning, and to verbs and clauses all the pronouns personal and even relative. But it differs from the High Asiatic languages in the variety of its inflections, and in the extensive use of auxiliary verbs; as well as in its tendency to form words by *agglutination*, which has been noticed as so remarkable a peculiarity of the American languages. (p. 53.) It must be borne in mind that, as M. de Ponceau has observed, the Euskarian language, "preserved in a corner of Europe by a few thousand mountaineers, is the sole remaining fragment of perhaps a hundred dialects, constructed on the same plan, which probably existed and were universally spoken at a remote period in that quarter of the world." We should do very wrong if we were to attempt to estimate the former extent of the Iberian nations by that of the existing representatives of them. From the manner in which they are spoken of by the ancient geographers and historians, there is good reason to believe that the Iberi inhabited even in their time the greater part of the peninsula of Spain and the southern parts of Gaul; and this opinion has received the most satisfactory confirmation

from the analysis which M. de Humboldt has made of the local names throughout the peninsula, as well as in the countries beyond its limits, which are said to have been tenanted by people of the same stock with the Iberi.

We think it impossible to give a candid consideration to these most remarkable facts, without coming to the conclusion that a large part of Europe was inhabited by aboriginal tribes descended from the nations of High Asia, anteriorly to the north-westerly migration of the tribes of the Indo-Median stock. The irruption of these tribes into Central Europe seems to have separated the aborigines into two great divisions; of which one was gradually pressed northward and eastward, so as to be restricted to Finland and Lapland; and the other southward and westward, so as to be confined at the earliest historic period to a part of the peninsula of Spain and the south of France, gradually to be driven before the successive irruptions of the Celts, Romans, Arabians, and other nations, until their scanty remains found an enduring refuge in the fastnesses of the Pyrenees. The Magyars of Hungary seem to be the only descendants of the High Asiatic stock that have maintained their footing in Central Europe; and this may have been due to their having made greater advances in civilization,—if we may judge, at least, from their present rank in the scale of nations, and the corresponding improvement in their physical characters.

To those who have been accustomed to the use of the term *Caucasian* as designating the most improved type of physical structure, or as indicating the supposed original seat or centre of the races which exhibit that type, it will appear very strange to be informed that the proper Caucasian and Georgian race is made up of a group of nations which has been confined, from the earliest periods at which we have any knowledge of them, to the mountainous regions in the neighbourhood of the great Caucasian chain; that these nations differ so completely in language and manners from all those by which they are surrounded, that it is difficult if not impossible to settle their true descent; and that of the numerous tribes of which they consist, some are even unintelligible to each other. They agree, however, in their warlike habits and in their hatred of foreign domination; which, with the advantages of their position, have enabled them to maintain their independence against Persians, Greeks, Romans, Mongolians, and Turks, and even to keep at bay the armies of Russia. The reputation which they have acquired for personal beauty is probably much beyond the truth; those individuals only being selected for the Turkish slave-market who are distinguished in this respect. We are told by Pallas, that both males and females are better formed than are the people of most other unpolished nations, although the women are by no means uniformly "Circassian beauties;" but other travellers inform us that glaring red hair is by no means unfrequent among them. The languages of the several Caucasian nations, although so unlike one another as to check their mutual intercourse, have yet that species of affinity which evinces a common origin at some very distant era. With the Indo-European family of languages they seem to have no relation whatever; and their correspondence in physical characters cannot, therefore, be admitted as a proof of similar descent, any more than it is in the case of the Turks, Magyars, or Biscayans. The only relations which the

Caucasian languages present to other known forms of speech, are to the dialects of Northern Asia, particularly those of the Finnish nations and of the Samoiedes. With these they correspond alike in the coexistence of numerous roots, and also in the characters already noticed as common to the Euskarian tongue and the languages of High Asia. Hence, upon the general principle of giving more weight to fundamental affinities of language (especially when there is evidence that these cannot be accounted for by intercourse of the nations which present them) than to resemblance in physical characters, we should be led to the remarkable conclusion that the Caucasian races are really descended, like the Eskaldunes, from the Northern Asiatic stock; and there is nothing in their present aspect or past history to negative such a conclusion, although the evidence upon which it rests must be admitted to be very imperfect.

The fifth and last volume of Dr. Prichard's great work includes the Oceanic and American races. The length to which our survey has already extended requires that our notice of these should be comparatively brief.

The name of *Oceanica* has been given by Malte-Brun and other geographers to the great Southern Ocean, with the numerous islands and groups of islands within its limits. Its boundaries are very wide; for it is regarded as extending from the eastern coast of Africa, where Madagascar is the first land it includes, to the western shores of America, the last or most easterly land it embraces being Easter Island. In latitude it extends from the coast of Asia southward without limit, including all the insulated lands that have been discovered in the Austral seas; and it is also considered to embrace the greater part of the Northern Pacific Ocean and the islands scattered through it. The Kurilian and Aleutian Islands are not usually reckoned as belonging to it, because they are known to be inhabited by races of people who came immediately from the adjacent continents, and who are unconnected with those tribes of the human race who peopled the remote islands of the great ocean. The most northerly groups within the circuit of Oceanica, and inhabited by its peculiar races, are the Sandwich Islands in the eastern part, and the Marian Islands or Ladrones, and the great Archipelago of the Carolines to the west. In this great region, which presents such immense varieties of climate and soil and geographical position, we should expect to find, if anywhere, great diversities in the physical characters of the human tribes to which it affords habitation. The following is Dr. Prichard's general account of them:

"I propose to distinguish the whole collective body of these native races of the Great Ocean by the name of Oceanic or Pelagian nations. Polynesian they have often been called, but that term has been applied of late to a particular division of them, and is no longer fit to be used as a general name. Under this appellation of Oceanic races several different descriptions of people are comprehended.

"The whole collective body of these native races of the Great Ocean are termed Oceanic or Pelagian nations. They are divided into three principal groups. The first, which alone can be described with propriety as a particular race or family of nations, comprehends the numerous and widely-dispersed Malayo-Polynesian tribes, who, though in some instances displaying certain diversities in physical characters and manners, are proved by a decided affinity of dialects to be originally of one kindred. Next to these we must place a group of nations who are very inferior to the Malayo-Polynesians in arts and civilization, and differ from them remarkably in physical characters. These nations also differ

from each other in stature and bodily conformation; the characteristic which is common to them is a certain approach in colour, features, and particularly in the nature of their hair to the Negro races of Africa. In this last particular there is, however, a great difference among them. Some tribes, as the Papuas of New Guinea, have spiral and twisted hair growing in large tufts to a considerable length, which, when combed out, forms an immense frizzled mass enveloping the head with a sort of periwig of great circumference. Other tribes have hair growing in short and closely frizzled curls like the Negroes of Guinea. We cannot comprise all these nations under the designation of Papuas, which belongs to a particular division of them. I shall term them, for the sake of distinction, Pelagian or Oceanic Negroes. I am aware that some objections may be made to this appellation, but I can discover no other name that is on the whole more suitable. The third department of Oceanic nations have been termed Alfourous, Haraforas, and Alforians. To this division the Australian tribes have been referred, and the Alforas have been described as resembling the Australians in the shape of their heads, which display a peculiar type, and in the nature of their hair, which is not crisp or woolly, but straight and long. Of the history of these nations we have very little information on which reliance can be placed. The Australians are the only race included among them whose language is known to be distinct from the Polynesian. As to the various tribes of the islands in the Archipelago referred by voyagers to the Alforian people, it is still undetermined whether they are allied to the Polynesian or Australian stock or constitute a separate family." (Physical History of Man, vol. v, pp. 3-5.)

The name of *Malayo-Polynesian* is given to all those nations of the great Oceanic region whose dialects have been found to bear an affinity to the language of the Malays or people of Malacca. This is well known to be the case with a large proportion of the tribes dispersed through the whole extent of this quarter of the globe; their affinity, which was first suspected from the resemblances of their dialects by Captain Cook and his companions, having been made evident by the subsequent researches of Marsden, Crawfurd, and other learned writers, but more especially by the profound investigations of Baron William von Humboldt. This great stock seems naturally divisible into three branches: the first including the *Indo-Malayan* or *Western Malays* of the Indian Ocean; the second, the *Polynesians* of the Pacific; and the third, the *Madecassians* or inhabitants of Madagascar. A much closer affinity prevails between the idioms of the tribes of the first group, than between their languages and those of the Pacific Ocean. The former, indeed, may be almost said to be dialects of one language; since they are susceptible of analysis by the same grammatical rules, and each contains elements common to all. It is in the Philippine Islands that this common speech displays the fullest and most varied and elaborate development of grammatical forms; but there is reason to regard the same idioms as extending over the whole Indian Archipelago from Sumatra to New Guinea, and as prevailing among the natives of the Ladrones and Caroline Islands, which lie on the borders between the Indian Archipelago and the Polynesian region. The proper Malays are well known as a people of short and slender stature and small limbs, but well-formed and vigorous; they have flat faces, oblique eyes, and features generally resembling the Chinese; they are in complexion considerably darker than that race, as might be expected from their proximity to the equator; but they are much fairer than the Hindoos. They have, in fact, much of the tawny yellow tint of the Northern Asiatics, and resemble them also in the character of the hair, which is black, par-

tially tinged with a deep reddish-brown hue, commonly lank, but sometimes waving in curls. The natives of the Caroline Islands have a decidedly Mongolian countenance; and their complexion is originally of a citron yellow, though it becomes brown by exposure.

The *Polynesian* branch consists of numerous tribes scattered over groups of islands in the Pacific Ocean; and these are for the most part similar in manners and customs to the most rude of the West Malayan islanders, or to those tribes whose simple and primeval state has not been altered by modern habits introduced by the Mahommedan Malays, or by an earlier intercourse with the people of continental India. There is amongst them, however, a much wider diversity, both as to physical and mental characteristics; so that they would not seem, but for their fundamental community of language, to present any near approach to each other, or to the preceding group. This community, however, is so close, as to leave no reasonable doubt of a common origin; for there is no other feasible method of accounting for the fact that the languages of people so remote from each other as those of New Zealand, of Tahiti, of Tonga, and of the Sandwich Islands, should not merely have a fundamental vocabulary common to all, but should have been formed by the same laws of construction. Although their resemblance to the languages of the Malayan branch is less evident than the mutual resemblance of the dialects of the latter, yet it is quite decided enough to place them in one family of languages. The Polynesian tribes are also connected by a general correspondence in their social condition, and in their religious and political institutions.

In regard to physical characters, the Tahitians and the Marquesans may be regarded as the finest specimens of the Polynesian race. They are tall, well made, their figures combining grace and vigour, and their countenances presenting much of the European physiognomy, except in the spreading out of the nostrils, and the thickness of the lips—characters which, though more constant among them, are frequently to be met with amongst individuals of our own race. The skull is for the most part remarkably symmetrical; there is occasionally, however, a slight tendency to the formation of a ridge along the sagittal suture, and to prominence of the upper jaw, as if indicating a slight degradation towards the Negro type. The complexion, especially in the females of higher classes who are sheltered from the wind and sun, is of a clear olive or brunette, such as is common among the nations of central and southern Europe; and the hair, though generally black, is sometimes brown, auburn, or even red or flaxen. The Hawaii or Sandwich islanders stand next to the Tahitians, to whom they are nearly allied in language. Their form and physiognomy are less elevated; their complexion somewhat darker; and their hair, though often soft and flexible, is frequently also crisp and frizzled. The New Zealanders and Ombai islanders constitute the next subdivision. They present great varieties of conformation and complexion; some of them being tall, well-formed, and comparatively fair; whilst others are dark or almost black, and inferior in stature and figure. Yet the language of these people, being a simple Polynesian dialect, gives no indication of the mixed descent between the Polynesian and a supposed dark aboriginal race, which has been imagined by some writers; and they have themselves no tradition which would warrant such an explanation.

Similar varieties in physical character are to be traced among all the other races scattered through the Great Southern Ocean; and if we once admit such a conjectural mode of accounting for them in one case, we must adopt a similar hypothesis in every other, and even this will give no explanation of the appearance of a type so very near the European in several of them. The inhabitants of the Tonga or Friendly Islands, which constitute a fourth group, bear considerable resemblance to the New Zealanders, both in physical characters and in language. They are of stout, vigorous form, less rounded and graceful than the Tahitians, but more muscular. Their features are very various; a truly European cast of countenance, with Roman nose, being frequently to be met with; and few have any peculiar thickness about the lips. The general colour is a cast deeper than the copper brown; but several have a true olive complexion, and some of the women are still fairer. The hair is generally thick, straight, and strong, though bushy and frizzled in a few instances; its general colour is black.

The *Madecassians, Malecassians,* or inhabitants of Madagascar, constitute the third primary branch of the Malayo-Polynesian stock; the chief ground for referring them to it being the affinity of language, which is of the most fundamental character, and not such as could be accounted for by any subsequent intercourse between nations originally distinct. Throughout the whole of this large island only one language is spoken; notwithstanding that the several tribes by which it is inhabited differ remarkably in their physical characters. Much yet remains to be known, however, respecting them. It appears that some among them bear a striking resemblance to the Malayan race; whilst others show a transition to the Negro type in the darkness of the complexion, the thickness of the lips, the breadth of the nose, and the curly and bushy character of the hair; and others, again, approximate to that type still more closely in having black skins and woolly hair. Dr. Prichard thinks that the most feasible method of accounting for these diversities is to suppose that a great part of the Malecassian population was originally derived from Africa, and that the Malayo-Polynesian language was introduced by a colony from the Indian seas, probably from Java, at a time when the Javanese were skilful navigators and formed settlements on various islands of the Indian Archipelago. It is not unlikely, he thinks, that an accurate examination of the different dialects spoken among the black and woolly-haired tribes (which has not yet been made) might display resemblances with the idioms of the Kafirs and the Mozambique Negroes.

The second principal division of the Oceanic races is formed of those black-skinned, ill-favoured savages who are found in the interior of many of the larger islands of the Indian Archipelago, and in some instances constitute their principal or even their entire population. Of these people, termed by Dr. Prichard *Pelagian Negroes,* he gives the following general account, which places them in striking contrast with the Polynesians.

"Ferocious and sullen, of savage and menacing aspect, naturally averse to intercourse with strangers, they have ever shunned the approach of civilized people as uniformly as the Polynesians have courted it. Hence these tribes, their national affinities, and their languages, are still almost wholly unknown, in spite of the diligent inquiries of many intelligent persons who have devoted themselves to the

investigation. Their physical characters are likewise very different from those of the agile, graceful, and comparatively fair Polynesians. Among these tribes are to be seen some who recede farthest from the almost European or Asiatic beauty of the Tahitian and Marquesan islanders, and exceed in ugliness the most ill-favoured brood of the African forests, whom they rival in the sooty blackness of their complexion. The natural covering of their head is of various kinds; some have the woolly hair of the Negroes of Guinea; others have lank straight locks, which may be compared with those of the Esquimaux or the Algonquins; while many are striking to the beholder by their broad, bushed-out, and frizzly periwigs, reaching the circumference of three feet, by which they obtained from Dampier the epithet of 'mop-headed Papuas.'" (Physical History, vol. v, p. 213.)

In his 'Natural History of Man' Dr. Prichard had adopted the opinion of Quoy and Gaimard, who, followed by Lesson, maintained that these mop-headed Papuas are a hybrid race, resulting from the admixture of the Malays and the Pelagian Negroes; but he now considers them (on the authority of Mr. Earle, who has had more extensive intercourse with them, and perhaps greater opportunity of correct observation of several of these tribes than any European before possessed) as a pure breed, forming one of the three divisions under which the black races of this part of Oceanica may be arranged. The other two are the puny Negroes of the Indian Seas, which have woolly hair and features resembling those of the Guinea Negroes; and the Australasians and Alforians, which have straight hair and a less Negro physiognomy. To the portion of Oceanica inhabited by these races Dr. Prichard gives the name of *Kelænonesia* or Oceanic Negro-land. Under this designation must be comprised all the insulated countries which lie immediately around Australia, including New Guinea and the whole of Papua-land on the northern side; Tasmania (Van Diemen's Land) on the southern; New Ireland, Solomon's Islands, the New Hebrides, and New Caledonia, on the north-east and east; whilst on the north-west it seems to have originally comprehended the greater part of the Indian Archipelago, extending into the mountainous interior of the Malayan peninsula and to the Andaman Islands in the Bay of Bengal, and northwards to the Philippines. In all these situations, traces of the Pelagian-Negro races remain; but wherever the Malayan races have established themselves, they appear either to have exterminated the Negro tribes, or to have driven them to the mountain fastnesses of the interior. It is in New Guinea, New Britain, New Ireland, Solomon's Islands, New Hebrides, and New Caledonia, that the Pelagian Negroes constitute the chief or sole population. The aborigines of Tasmania (now almost extirpated) appear, from Captain Cook's account of their physical characters, to have belonged to the same group. The inhabitants of the Fijian or Feejeean Archipelago, which lies between Kelænonesia and Polynesia, present us with the remarkable phenomenon of a people speaking a dialect of the Polynesian language, yet distinguished by physical characters bearing a close approximation to the Negro type.

The third and last division of the Oceanic races is that which includes all the native tribes of the great island-continent of Australasia, together with a mountain race in the interior of New Guinea. These alone are properly entitled to the designation of Alfourous or Alforians; the people known by this name, in the Indian islands, according to the recent and searching inquiries of Mr. Earle, being all tribes of the Malayo-Polynesian

race who remain in their uncivilized and primitive state. So far as is yet known, there is a general correspondence in physical characters among all the Australian tribes; the differences being only such as may be very reasonably attributed to the scantiness or plenty in the supply of food, operating through many successive generations, according to the districts inhabited by the respective tribes. The following is one of the latest descriptions of them,—that given by Captain Wilkes of the American Exploring Expedition.

"The natives are of middle height, perhaps a little above it; they are slender in make, with long arms and legs. From their wandering life, irregular habits, and bad food, they are extremely meagre; and as their thinness is accompanied by considerable protuberance of the abdomen, it gives to their figure a distorted and singular appearance. The cast of the face is between the African and Malay: the forehead usually narrow and high, the eyes small, black, and deep-set; the nose much depressed at the upper part between the eyes, and widened at the base, which is done in infancy by the mother, the natural shape being of an aquiline form; the cheek-bones are high, the mouth large, and furnished with strong well-set teeth; the chin frequently retreats; the neck is thin and short. Their colour usually approaches chocolate, a deep umber, or reddish black, varying much in shade; and individuals of pure blood are sometimes as light-coloured as Mulattoes. Their most striking distinction is their hair, which is like that of the dark-haired Europeans, although more silky. It is fine, disposed to curl, and gives them a totally different appearance from the African, and also from the Malay and American Indian. Most of them have thick beards and whiskers; and they are more hairy than the whites." (Physical History, vol. v, p. 264.)

It was for a long time supposed that a great variety of languages exists in Australia; every little tribe or horde into which the population is divided being imagined to have an idiom of its own, totally different from the speech of its nearest neighbours. But more extensive and profound researches have had the effect which might be anticipated; showing that there is a much greater coincidence in the several vocabularies than had been imagined; and that there is such evidence of identity in grammatical construction, as to make it obvious that they are all dialects of a common family of languages. Between these languages and the Polynesian dialects, Dr. Prichard points out some curious affinities; but these affinities serve also to connect them with the Tamulian group of languages (derived, as we have seen, from those of High Asia); and the resemblances between the Tamulian and Australian seem to be closer than those which are presented by either to the Polynesian. For example, the particles, which in most languages are prepositions or prefixes, are in the languages of High Asia added to the terminations of words; this character is found also in the Tamulian and Australian languages; but the Polynesian, like many African languages, and like the European, has prefixes and prepositions. These resemblances would lead to the conclusion, which does not seem improbable on other grounds, that the native Australians are an offset from that southern branch of the great Nomadic races of Asia, which very early spread themselves through the Indian and Indo-Chinese peninsula. There is perhaps no nation, except the Bushmen and the most degraded of the Negroes, whose physical condition is so miserable, or whose mental state appears so low; yet the statements of recent observers, especially of Captain Gray, indicate that the usual ideas on this point are greatly exaggerated.

Notwithstanding the degradation of their physical condition, and the

want of success hitherto experienced by those who have made attempts at Christianizing them, there is no reason whatever to believe that the Australian aborigines are destitute of any of the intellectual or moral attributes which are possessed by the highest races of men. The remark has been frequently made by those who have adequate opportunities of observation, that Australian children are in general quite equal to English children in their manifestations of intelligence and capacity for acquiring knowledge; and although their mental development may earlier cease (as seems to be the case with the Negro races), so that they remain, as it were, great children all their lives, yet there is every reason to believe that the education of a few generations would produce the same kind of improvement in them as it has done in other races previously uncultivated. Although they appear to have less susceptibility than many other rude nations to religious impressions, yet it is quite certain that they are not destitute (as some have represented them to be) of all idea of a God: they seem to have a notion, also, of a future state, and believe in good and evil angels. They have likewise a superstitious belief in magicians or sorcerers; another point of connexion with the nations of High Asia. It is remarkable that in many of their most complex and singular institutions, they present a very peculiar resemblance to many tribes of North American Indians.

The almost total ignorance which at present prevails in regard to the languages of the other black races of Kelænonesia, prevents us from being able to determine their probable relationship to the Australians; but at present it would seem proper (in the absence of any sufficient evidence to the contrary) to associate them all together, and to regard them as the primitive inhabitants of this extensive region, including nearly the whole of the Indian Archipelago with Australia. The relations of the Australian languages to those of High Asia seem to confirm the idea which the geographical position of this region would suggest,—namely, that these races originated from the great stock of Central Asia; coming off from its southern or south-eastern extension at a very early period, before language and civilization had made any great progress. At a later time a second colonization took place,—that of the Malays; who seem pretty certainly to be an offset from some of the nations, now more elevated in civilization and more fixed in physical characters, of the Indo-Chinese peninsula. These people spread themselves through the Indian Archipelago; but their tendency was rather eastwards than southwards; so that instead of possessing themselves of New Guinea and Australia, they gradually migrated through the islands of the Polynesian region. There may seem more difficulty in accounting for the peopling of those remote islands from any one source, than there is in understanding the spread of the human race over the whole remainder of the globe. But the difficulty is more apparent than real; for not to mention the analogous phenomena in regard to the introduction of plants and animals from distant regions, which the creation of new islands by the growth of coral is continually presenting to us, there is evidence that the accidental transport of parties of the islanders to lands very distant from their own is of no very unfrequent occurrence. Several cases of this kind are collected by Mr. Lyell, from the accounts of Cook, Forster, Kotzebue, and Beechey, within whose own knowledge they occurred; and these would of course bear no comparison to the actual number of such occurrences. In the instance men-

tioned by Kotzebue, a party which was passing from one island to another in a sailing boat was driven out of sight of land, and remained at sea for eight months, during which time they supported themselves by fishing. They at last drifted to an island *fifteen hundred miles* from the one they had left.

If the Malayo-Polynesian nations are to be regarded as really the offspring of one family, we find in the ramifications of that stock examples of almost every variety of the human species. Thus the proper Malayan tribe differs little in its physical characters from the Indo-Chinese type; but among the well-fed Polynesian islanders, the stature and fulness of the body have increased, and the form of the skull and the character of the physiognomy have come to approximate those of the Hindoo or European. In many of these islands, the prevalent complexion among the mass of the people, who are continually exposed to the agency of the climate, is much darker than that of the genuine Malays; the hair becomes somewhat crisp, and the features ugly, presenting a decided approach to the characters of the Pelagian Negroes; and yet among the very same people, the superior caste, who pass their days in luxurious ease, and have little occasion to expose themselves to the sun, have a fair complexion and all the characters of a highly developed or xanthous constitution. This contrast is very remarkable in the Tahitians and Marians.

"It seems to have been the ultimate and full persuasion of all those persons who have made a long abode in the islands of the Pacific under circumstances favorable to accurate investigation, that these phenomena can only be explained on the supposition that they result from the agency of climate and physical influences on the original race. The appearance of a xanthous complexion under moderate temperature and among people living in a state of protection from severities of climate is so common an observation, and one that we have already traced in so many instances in almost every other part of the world, that we may well look for it in the Polynesian Islands; and there, when we find this change connected and coextensive with another physical change, we may fairly infer that these connected phenomena have one and the same cause. I allude to changes in the stature, the form of the head, the quality of the hair, &c. There seems to be no other hypothesis, if we open the widest field to conjecture, that can in any way explain all the phenomena of physical variety that display themselves in the Oceanic region, and this sufficiently accounts for all of them,—namely, the deviation of the primitive Malayan or Indo-Chinese type on the one side to the character of the European, and on the other to a conformation of body very similar to that of the African." (Physical History of Mankind, vol. v, p. 285.)

The aboriginal people of *America* are generally considered as a department of the human family very distinct from the inhabitants of the Old World; and attempts have been frequently made to define the race by physical characters. But these attempts have been founded upon a very limited acquaintance with the nations of this vast continent; for, taken in the aggregate, they are by no means uniform either in physical or moral qualities; nor is the line of distinction between them and the rest of mankind so obvious or strongly marked as is usually imagined. Thus the native Americans have been described as "red men;" but there are tribes equally red, and perhaps more deserving that epithet, in Africa and Polynesia; and the American nations are by no means all of a red or coppery hue, some being as fair as many European people, others being brown or yellow, and others nearly if not quite as black as the Negroes of

Africa. Again, it has been attempted by anatomists to distinguish the American races by a certain configuration of skull and form of features; and even Dr. Morton, in his splendid 'Crania Americana' (noticed by us in our tenth volume), has given his authority in support of the opinion that such distinctive characters existed "in the squared or rounded head, the flattened and vertical occiput, the high cheek-bones, the ponderous maxillæ, the large quadrangular orbits, and the low receding forehead." Nevertheless he is obliged to admit that very considerable diversities present themselves in cranial conformation; and he altogether excludes the Esquimaux, who, according to the evidence of language, must be regarded as being as truly an American people as any other, in spite of the manifest approach which they present to the Northern Asiatic type of cranial configuration. The testimony of those who have actually travelled in different parts of America, and who have scrutinized with observant eyes the physiognomy and form of head, not in individuals only but in nations, must be admitted to be much more satisfactory; and amongst these that of the distinguished naturalist, M. d'Orbigny, must be regarded as especially worthy of attention. "As a general position," he says, "we may regard each particular nation as having between its members a family resemblance, which, distinguishing it clearly from its neighbours, permits the practised eye of the zoologist to recognise in the great assemblage of nations all the existing types, almost without ever confounding them. *A Peruvian is more different from a Patagonian, and a Patagonian from a Guarani, than is a Greek from an Ethiopian or a Mongolian.*" Nor will any epithet derived from their habits of life apply to all the tribes of this quarter of the globe. The native Americans are not all hunters; there are many fishing tribes among them; some are nomadic, others cultivate the earth and live in settled habitations; and of these, a part were agriculturists before the arrival of the Europeans, whilst others have learned of their conquerors to till the soil, and have changed the ancient habits of their race, which, as we may hence infer, were not the necessary result of organization or of fixed congenital propensity.

Yet it will be found on close examination that certain characters are discoverable, which are common, or nearly so, to the whole of this group of nations; that there are strong indications, if not proofs, of a community of origin or of very ancient relationship between them; and that, in surveying collectively the people of the New World, we are still contemplating the same bodily and mental constitution as that which we have seen to prevail among the people of the Old, although it presents itself, in many respects, under a peculiar aspect. When we attempt, however, to ascertain their family relationship to the races of the eastern hemisphere, we find every indication tending to prove the antiquity of their separate existence; so that we must regard them as having constituted a distinct department or family of nations from the first dispersion of the human race. It is not to be expected, therefore, that we should be able to discover proofs of their derivation from any particular tribe or nation of the Old Continent; though, as we shall hereafter see, there are not wanting indications of their community of descent with certain races of the latter.

The most decided and clearly marked evidence of mutual relationship between the American nations is to be found in the characteristic *structure* of their languages. On this subject we have enlarged sufficiently in

our former article, (pp. 53-55); and we need not, therefore, dwell further on it here, except to remark, that although this mutual relationship has been found in all the American languages which *have been* examined, including the most remarkable dialects in very distant parts of America, yet that many are still altogether unknown; so that the evidence cannot yet be regarded as complete. It is a very curious circumstance, and one which cannot be regarded as a mere accidental coincidence, that the language of the Old Continent to which the American languages bear the nearest resemblance in their most characteristic feature, that of the formation of words by the process of *agglutination*, should be the Euskarian or Basque. Upon this point we shall have more to say presently.

There are, besides, many remarkable traits in the moral and social state of the American nations, which indicate some near relationship amongst them, and serve to distinguish them from the races of the Old World. Attentive observers have been struck with the manifestations of greater energy and mental vigour, of more intense and deeper feeling, of a more reflective mind, of greater fortitude, and of more consistent perseverance in their various pursuits and enterprises, when they have compared the natives of the New World with the sensual and volatile, and almost animalized savages, who are still to be found in some quarters of the Old Continent. They have been equally impressed by the sullen and unsocial character, by the proud apathetic endurance, by the feeble influence of the social affections, by the intensity of hatred and revenge, and by the deep malice-concealing dissimulation, so remarkable in the dwellers amid the dark solitudes of the American forests. Among many of the American nations, moreover, traces have been observed of ancient institutions, of a nature that seems to imply the existence of much refinement and of an artificial state of society. Of this description are,—a complicated form of government, regulated despotisms or monarchies, privileged orders, hierarchical and sacerdotal ordinances, systematic laws (the results of reflection and a settled purpose) connected with marriage, inheritance, and family relationships, and other customs, all of which are strongly contrasted with the simple and unreflective habits of rude and uncivilized nations. Their opinions, moreover, respecting a future state, and the nature and attributes of invisible agents, are strikingly different from those of nations who have never emerged from primitive barbarism. They have had in use, too, from time immemorial, cultivated plants and domesticated animals, different from those of the Old World; and their earliest traditions refer the knowledge of these to some fabulous person who descended from the gods, or who suddenly made his appearance among their ancestors; thus indicating the remoteness of the era of their separation from the inhabitants of the Old World, who have similar mythical legends in regard to the introducers of *their* first arts and acquirements.

Abundant evidence is afforded, by architectural and other remains still existing, as well as by the accounts of the early Spanish historians, of the high degree of civilization which some of the nations of America had formerly attained, especially in the warmer regions of that continent. Thus the natives of Mexico erected stupendous edifices, which rivalled those of Egypt; and although they could not yet attain to the greatest of human inventions,—that of symbols representing the sounds of words,—they had long aspired after it, and had contrived a method of recording events and of

handing down to memory the passages of their ancient history. They had even made great advances in science, and had a solar year with intercalations on the principle of the Roman calendar. They were diligent cultivators of the ground, and also expert miners and workers in metals; even astonishing the workmen of Europe with the skilful manner in which they set gems. They appear, too, to have been influenced by a deep sentiment of religion, and had a very stately and majestic ceremonial. Nevertheless they do not seem to have derived from these advances in civilization any moral improvement, or any mitigation of that sullen malignity which seems common to the native tribes of the New World; and their religion was far from having an exalting influence, since their gods had no attribute of clemency or mercy, but were invested with the worst forms of their own dark passions. In Peru, also, we find remains of Cyclopean structures erected during the government of the Incas, which bear comparison with those of ancient Egypt; and the wonder is increased when it is recollected that no beast of burden save the llama existed in Peru before the Spanish invasion. At a time when no public highways existed in England but such as were relics of Roman greatness, there were roads of 1500 miles in length in the empire of Peru, carried over heights which overtop the Peak of Teneriffe. The ancient Peruvians were ignorant of the manner of forming an arch; but they had constructed suspension bridges over frightful ravines. They had no implements of iron; but they could move blocks of stone as huge as the Sphinxes and Memnons of Egypt.

Among the peculiar customs of the American races, is that of altering the form of the skull by artificial compression, which may be traced in different parts, both of the northern and southern divisions of the continent. For an account of this custom, and of its effects, as given by Dr. Morton, we must refer to our former notice of his work. (Vol. X, p. 478.)

We cannot venture upon any survey of the individual nations of the American group, but must confine ourselves to a general summary of the results of such an investigation. In the first place, then, whether it be supposed that they have originated in an offset of the Asiatic races, or that they have had a distinct parentage, it must be admitted that, if the American nations have had a common ancestry, most of them have subsequently departed widely from the original type; and no other reasonable cause can be assigned for such departures, than the tendency to variation inherent in the human constitution, especially under the influence of a change in external conditions. Secondly, if this modifying influence be *not* admitted, but it be still maintained that the characters of the several races of mankind are fixed and unalterable except by mutual admixture, we must allow not *one* only, but *many* distinct progenitors for the several groups of American nations that present well-marked diversities from each other; a supposition altogether improbable, especially when the mutual affinities of their languages are duly considered. Thirdly, as the modifying influence of external circumstances, once admitted, enables us to account for the descent of the American races from an Old World stock, as readily as from a parentage peculiar to themselves, there is no *a priori* improbability in the way of the supposition that the American continent was first peopled from the Asiatic. Fourthly, the strongly-marked resemblance presented by the Esquimaux to the Northern Asiatics

in physical characters, whilst their language is decidedly American, seems to indicate that they form the connecting link between the two great races.

"There seems to be no great difficulty in the supposition that people from some tribe or tribes of the extreme north-east crossed over Behring's Strait, or passed along the Aleutian chain of islands from Asia to America in an early period, and formed a nation in the New World, who, after constructing or rather developing into its complex form a primitive speech, of which all the polysynthetic idioms are derivatives or imitations, spread themselves over the whole continent of America, and being thereby scattered, soon separated into particular hordes, which became the germs of many particular nations. The number and diversity of languages at the present day require that we should assume an early era for this event; and the rapidity with which the human species is propagated under favorable circumstances removes all difficulty that may attend the supposition." (Physical History of Mankind, vol. v, p. 546.)

The people who thus colonized America would doubtless be some of the northern Asiatic tribes who braved the cold of the Icy Sea, and supported themselves chiefly by fishing. The Esquimaux constitute just such another race; and may not improbably be regarded as the nearest representatives of the ancestral type, from which the offsets that have migrated in a southerly direction have long since departed. Thus, we see a coincidence between the geographical probability (if we may use the expression)—that is, the probability that the first American colonization took place from some northern Asiatic nation in consequence of the comparative facility of access from this part of the Old Continent,—and the correspondence in physical characters between the most adjacent of the Asiatic and American races, which strongly supports the idea of their close family relationship; and although the entire difference between the American and Asiatic languages might at first sight appear to be a powerful argument on the contrary side, yet the reappearance of the most universal and characteristic feature of the former, in what there is strong evidence for regarding as a remote offset from the latter,—namely, the Basque or Euskarian language,—must be admitted as an argument of no inconsiderable weight in the other direction. On this supposition, the original languages of High Asia contained an element which has never been manifested in themselves, but which has been partly developed in one of the early offsets of that race, the Iberian nation, and still more fully in the American, which was probably the result of a still earlier migration. Nor should the strong resemblance which has been pointed out between the peculiar customs and institutions of the Australian savages and the North American Indians be without its influence on our decision; for if *other* facts tend to indicate the common origin of these races—remote and almost antipodal as they now are,—the coincidence must be regarded as a confirmation of that supposition, not to be lightly set aside.

Thus, then, the most careful and extensive researches into the physical history of mankind tend to break down the barriers which ignorance and prejudice have erected between its different races: by showing that there is nothing more improbable in the supposition of their common origin in a single pair, than in three, four, five, or any small number of contemporaneous ancestors; since in each case the modifying influence of external circumstances must alike be admitted to account for the marked diversities

that can be shown to exist amongst the existing races whose origin would be referred to a common ancestry. And the supposition that each race at present distinguishable by well-marked physical characters had an originally distinct parentage, would lead to such a multiplication of primary stocks as we fancy that none of the advocates for such a doctrine are prepared for; besides being inconsistent with all the evidence of common descent, which is furnished by physiological, psychological, and philological considerations. In Dr. Prichard's first volume will be found much interesting evidence in favour of the specific unity of the human races, derived from their conformity in physiological characters,—such as the average duration of life, the periodical phenomena of the constitution, the phenomena of hybridity, and the liability to the same diseases; the chief points of which were adverted to by us, at a very early period of our critical labours, soon after the publication of that volume (see Vol. III, pp. 365 et seq.) He has also brought together a most interesting mass of evidence of identity in psychical nature between the highest or most civilized and the lowest or most degraded nations; which, in his shorter work, he thus sums up:

"We contemplate among all the diversified tribes who are endowed with reason and speech, the same internal feelings, appetencies, aversions; the same inward convictions, the same sentiments of subjection to invisible powers, and (more or less fully developed) of accountableness or responsibility to unseen avengers of wrong and agents of retributive justice, from whose tribunal men cannot even by death escape. We find everywhere the same susceptibility, though not always in the same degree of forwardness or ripeness of improvement, of admitting the cultivation of these universal endowments, of opening the eyes of the mind to the more clear and luminous views which Christianity unfolds, of becoming moulded to the institutions of religion and of civilized life: in a word the same inward and moral nature is to be recognised in all the races of men." (Natural History of Man, p. 546.)

The philological evidence in favour of the specific unity of the human races is to be found not merely in the coincidence of particular features of the languages of remote families of nations, but also in the more general fact of the universality of spoken language, and in the power of translating from one language to another. Dogs and monkeys *may* have languages of their own; but there is no such relation between either of them to ours as may enable us to become acquainted with them; and where brute animals have been taught to comprehend human language, it has been that only which related to certain objects well known to them with which the sounds of the words could readily be associated. Let this limited comprehension be compared with that which is manifested by such human beings as seem most completely cut off, by the deficiency of organs of sensation, from communication with their fellows, and the contrast is at once evident. The lighting up of the whole countenance of Laura Bridgman, which her benevolent and persevering instructor, Dr. Howe, describes as having taken place when she first made the discovery that she could express her thoughts in words by a combination of literal signs, and the gradual elevation of her whole mind through the medium of language, from the level (but little above that of the brute) in which it would have probably ever remained if she could not be made to comprehend its use, afford most striking testimony to the fundamental importance of this common characteristic of humanity. The power of fully rendering the thoughts conveyed in one language into another tongue, must of course

depend in great part upon the relative advancement of the two languages. The profound ideas of a German philosopher could not be expressed in the Hottentot gibberish; nor does the peculiar style of eloquence cultivated in the East produce its adequate effect when rendered in our own language. But any two barbarous languages, or any two which are highly cultivated, are so pervaded by a sameness of character as to bear witness to the identity of their internal source.

Contenting himself with having established to his satisfaction the main positions of the common origin of the human races, Dr. Prichard does not offer any speculations in regard to their primary source. Perhaps he considers it safer not to give the weight of his authority to any such speculations, until they may have a better foundation than that on which they can be at present built. But as we are not restrained by any such consideration, we shall briefly state the idea which had occurred to our own minds, and which has gained strength by the more detailed survey which we have been recently led to make (for the purposes of this review) of the vast collection of materials brought together by Dr. Prichard.

The stock from which the globe was originally peopled appears to us to be most nearly represented at the present time by the people of High Asia, namely, the Mongolians and their allies; and some part of that region was probably their original seat. We ground this hypothesis (for we wish it to be regarded as nothing else) upon the following considerations. The physical characters of these people are such as peculiarly adapt them to a nomadic life; they have a vigour of constitution which enables them to brave great diversities of climate; and they readily accommodate themselves to a great variety of hardships. Their natural disposition leads them almost to seek these rather than to avoid them; and to wander over the surface of the earth, rather than to settle in any one spot. If it had been intended that by such migrations the whole globe should be peopled, no better *centre* could be thought of than Higher Asia; whose connexions with all other lands are such as are possessed by no other region; and whose climate is so intermediate between that of the frigid and the torrid zones, that the passage into either may be effected without any violent transition. This *a priori* argument, however, would be worth but little, if we did not find it in correspondence with the very curious fact, that the most ancient inhabitants of almost every part of the globe are connected with the nations of High Asia, more or less closely, by affinities of language or of physical characters. This we have seen to be the case, not merely with the aboriginal people of Northern and Southern Asia, but with those of Northern and Southern Europe, with those of the Caucasus, with the first settlers in the Indian Archipelago, and (as seems highly probable, though the proof is less complete) with the American nations. The only region regarding which there is not the same decided evidence is Africa; but until the contrary shall have been shown, we cannot but think that the existence of a people presenting so strongly the original Mongolian characters (modified, however, by change of climate, &c.) as do the Hottentots, affords strong evidence that this continent also was early peopled from the same stock. There is ample evidence that the Mongolian type is capable of being elevated on the one hand towards the Indo-European, or degraded on the other to the Negro or Australian; and we are not

aware that either of the other types exhibit the same capacity for metamorphosis.

In certain spots of the globe thus peopled with races derived from a common centre, varieties in physical conformation sprang up; new and more refined languages were originated; and subordinate centres were thus formed, from which more limited radiations have subsequently taken place, impressing their own features of civilization upon the countries through which they have spread. Thus we have, at a very early period, indications of the Egyptian, the Syro-Arabian, the Indian, the Indo-Chinese, the Mexican, and the Peruvian races; and although some of these may possibly have been mutually connected at their origin (as the Egyptian and the Indian), they seem to have been very early separated, and to have attained their fullest development quite independently of one another. Each of these had its own peculiar characters, departing more or less from the common original; and contained within itself those elements of progress, of fixity, or of early decay, which have exerted their influence upon the subsequent destiny of these races. The great body of African nations is the one whose origin has seemed to us most obscure; but the recent investigations which have been made in regard to the languages of some of their most elevated tribes, would lead us to regard this continent as having been peopled (after its first and most scattered colonization) by successive offsets from the stem which produced the Syro-Arabian races. The earliest of these, which wandered the farthest from the original centre, would seem to have most completely lost the traces of its original ancestry, both in respect to language and physical characters; whilst the later colonies, which have not extended themselves beyond the northern and eastern portions of the continent, have retained more or less of the primitive type of language, and usually show less approximation to the true Negro physiognomy. Nevertheless the Syro-Arabian variety does exhibit a greater tendency to this kind of change than we know to be the attribute of any other race; since instances are by no means wanting, in which, under the prolonged influence of similar external conditions, families and whole tribes of undoubted Arab descent have assumed all the most important characters of the Negro races.

It has been remarked by an acute critic, that the lapse of time requisite to bring about such changes as those required in any hypothesis of the single origin of the human races, is far greater than the limits of received history allow. This remark is commented on by Dr. Prichard in a supplementary note at the close of his last volume; which is not one of the least interesting parts of his work. He points out that all those writers who have entered upon the investigation of primeval history, have felt a difficulty in reconciling the proofs of the early existence of powerful empires and high grades of civilization with the ordinary chronology founded upon the Mosaic records; even though not at all disposed to impugn the authority of the latter. And then, in a truly philosophical but not less reverential spirit, he proceeds to inquire how far that chronology is necessarily to be regarded as complete and authentic, by those who receive the narrative as the genuine record of Divine Revelation. He comes to the conclusion which might be expected from so learned and judicious a critic, —that the Book of Genesis was not intended to give us an exact chrono-

logy, any more than it was intended to teach us geology or astronomy; that it is a collection of different documents having no proper connexion with each other,—that the genealogies which it includes have no claim to completeness,—that the great length of lives recorded in the ante-Abrahamic times is founded on a mistake in the interpretation of numbers or numerical signs,—and that there is consequently no foundation whatever for a chronology of the earliest ages, or, in other words, that no means are to be found for ascertaining the real age of the human race. Consequently we need not be fettered by any considerations of this kind in our attempts to determine the mode in which the globe became peopled by the nations, which are now spread over every part of its surface that is fit for the habitation of man.

It now only remains for us to place before our readers a general estimate of the merits of Dr. Prichard's ethnographical labours; and we cannot do so more concisely or pointedly, than by adopting the language of a learned contemporary, to whom we have more than once had occasion to refer, and whose judgment, in this particular, we would most entirely and cordially adopt as our own. Speaking of the 'Physical History,' he says,—"It is impossible to even turn through the pages of Dr. Prichard without admiring the very varied erudition brought together for the illustration of one topic. The knowledge of a modern physiologist is in itself sufficiently multifarious. Blumenbach and some others had so far led the way upon this question, as to have preoccupied all the main physiological discussions and the most prominent facts concerning human races. But Prichard has not merely filled out with an almost inexhaustible learning the topics previously familiar, and endowed with new force arguments which had been almost abandoned as untenable; he has further investigated ancient and modern races with a detail perhaps previously unattempted, scraping together and illustrating with great sagacity all the scattered notices found in Greek or Roman writers, and combining for his purpose the ethnographical information furnished by modern travellers, missionaries, and philologists. The latest researches of acute and inquisitive Germans, whether concerning the inscriptions of ancient Italy, the characters of Persepolis, or the language of Biscay and India, with the splendid geographical generalizations of Karl Ritter, are all made to converge into the same focus. The value of the work before us by no means depends upon the question whether the writer has or has not proved, either that man is one species, or that this species descends from one pair. It is, in fact, a storehouse of information concerning the whole controversy. The adverse arguments, although not pressed and developed as by an advocate, are set forth without disguise or suppression, so far as we are aware; so that in no small degree the reader is put in possession of materials for an independent judgment. Add to this,—what is in our view of peculiar importance, quite independently of the great cause which the book is pleading—the facts themselves, so varied, so instructive, so picturesque, and so very eloquently set forth, are of high interest; and the learning, ancient and modern, crowded into its pages, is such as would with difficulty be gleaned from ample libraries by the most persevering study. The work, in short, has been the labour of a life. Thirty-seven years have elapsed since the accomplished author laid its founda-

tions, and three times has it been reconstructed."* As a striking contrast with the hasty manner in which some other writers have been accustomed to dogmatise on the subject, we may recall the attention of our readers to the fact, that the first volume of this third edition was published as much as eleven years since, soon after the commencement of our critical labours. That the delay in its completion has been owing to nothing else than the author's determination to render it as perfect as possible we can vouch for, from our own knowledge of the unremitting devotion of his disposable time to his laborious investigations. We rejoice that we are enabled, at the close of our own career, to congratulate him on the successful attainment of this step in his honorable course. That he will not consider it as here concluded, but will be constantly availing himself of every kind of information that may throw light upon the obscurer departments of Ethnology, all who have the privilege of a personal knowledge of him must feel the utmost confidence.

In regard to the smaller work it will be sufficient to say, that its object is to furnish for the use of general readers a brief and popular view of all the physical characteristics, and likewise of the moral and intellectual peculiarities which distinguish from each other the different races of men; and likewise to offer such an account of the nature and causes of these phenomena as the present state of our knowledge will afford. It may, therefore, be regarded as a popular abridgment of the larger work; comprising the chief results of the inquiries pursued in the latter, with a general outline of the evidence on which they are based: the 'Physical History' is the work for the man of science; the 'Natural History' for the general reader.

Art. XV.

A Treatise on Diseases of the Air-passages: comprising an Inquiry into the History, Pathology, Causes, and Treatment of those Affections of the Throat called Bronchitis, Chronic Laryngitis, Clergyman's Sore Throat, &c. &c. By Horace Green, A.M., M.D., formerly President and Professor of the Theory and Practice of Medicine in the Castleton Medical College, Vice-President of the New York Medical and Surgical Society, and Honorary Member of the Philadelphia Medical Society, &c. &c.—*New York and London,* 1846. 8vo, pp. 256.

Dr. Green's work is a monograph on diseases of the follicles which stud the mucous membrane of the fauces, pharynx, and larynx, and is a practical work of very considerable merit. Its object is to point out the great value of nitrate of silver as a local application to the cavities of the larynx and pharynx, in cases of chronic laryngitis, bronchitis, and allied affections, many of which are so nearly similar in their symptoms to more serious pulmonary disease as to be mistaken for phthisis pulmonalis, or so urgently complicate the latter affection, as to render their treatment of importance, even in the alleviation of the sufferings to which the moribund of phthisis are so often exposed.

Dr. Green commences his essay with the anatomy of the fauces, pha-

* New Quarterly Review, vol. viii, p. 98.

rynx, and larynx. The mucous membrane lining these passages to the alimentary and respiratory organs is studded more or less freely with glandular follicles or muciparous glands, which are analogous to the glandules of other mucous surfaces. From Dr. Green we learn, that in the mucous membrane, which unites the base of the tongue with the epiglottis, are clustered together several follicles, having their excretory ducts opening into a common dilated orifice, the *foramen cæcum*, situated at the back of the tongue. Placed near this opening, are the *lenticular papillæ* of the tongue; they consist of twelve or fifteen large mucous follicles, of a conoid shape, disposed in two lines, which converge to an angle just before the foramen cæcum. The *amygdalæ* or *tonsils* seem to be composed entirely of an aggregated mass of follicles, enveloped in folds of the mucous membrane. On the internal and convex surface of these bodies are seen a large number of deep and irregular sulci or depressions. The walls of these cavities are lined by mucous membrane, whose surface presents numerous small apertures which lead into follicles or cells, that secrete the mucous and viscid fluid with which the cavities are generally filled. The *glands of the pharynx* are large and abundant; they are of an ovoid form, and are situated beneath the mucous membrane, but are not imbedded in the submucous tissue, as are those of the œsophagus and trachea. These glands are particularly numerous around the posterior nares, and under the cervical portion of the pharyngeal membrane; two of them, more complicated in their structure, being lobulated, and of larger size than the rest, are situated at the margin of the opening of the Eustachian tube. The *follicles of the uvula* are also large, and they are particularly numerous around the inferior extremity of this organ. The *œsophageal glands* are like those of Brunner, and are imbedded in the submucous tissue of the œsophagus; they are composed of small lobulated bodies or cells, several of them having their excretory ducts united in one common tube, which opens upon the surface of the œsophageal membrane. In the *larynx,* the mucous follicles are very numerous in that part of the lining membrane which occupies the upper part of this organ. On its surface may be seen the openings of some sixty or seventy excretory tubes, which pass into follicular cells situated in the submucous tissue. Placed in the substance of the epiglottis are numerous other glandulæ which have their openings on the laryngeal surface of this structure; one of these, consisting of several granules, is imbedded in a mass of fat which is situate between the epiglottis and the os hyoides; ducts pass backwards from it through foramina in the epiglottis, to open upon the posterior or laryngeal surface of this cartilage; it is named the *epiglotic gland.* Other glands are placed in the thickness of the superior vocal cords within the ventricles of the larynx, and in the folds of the mucous membrane in front of the arytenoid cartilage. The secretion from the laryngeal follicles is intended for the lubrication of the vocal ligaments, and it is directed upon them by small valvular folds of the mucous membrane, which are arranged in such a manner as to effect this object. The *follicles of the trachea* are still more numerous than those of the larynx; they are small, flattened, ovoid cells, situated between the fibrous and muscular layers of the membranous portion of the trachea, and in some places, beneath the muscular fibres; so that their excretory ducts have to penetrate not only the muscular layer but the mucous membrane,

in order to open upon its interior surface. In their normal condition, the fluid secreted by the mucous follicles of the air-passages is bland and transparent, and not abundant in quantity; but disease, as we shall find, greatly increases and vitiates their secretion.

Previously to discussing the pathology of these structures, Dr. Green observes that affections of the throat are ordinarily yet incorrectly arranged with those of the œsophagus:

"Pathologically considered, the relation which exists between the fauces, tonsils, and pharynx, on the one hand, and the respiratory tubes, on the other, is much more intimate and important than the connexion which exists between the throat and the œsophagus. In almost all the inflammatory affections of the air-passages, whether primary or consecutive, the diseased action has its origin in the fauces and pharynx, and extends by continuity from thence to the respiratory tubes; whilst the membrane lining the œsophagus may escape inflammatory action altogether, or become but partially implicated. These pathological relations, therefore, and the community of symptoms, will be kept in view, in examining into the nature of those lesions which affect these organs. The exact pathological conditions which exist in the throat and the air-passages in diseases of these parts have been until recently but imperfectly understood. Indeed, at the present day, several affections of the larynx and trachea are confounded by different writers, or their origin assigned to morbid conditions which do not exist; whilst, on the other hand, accumulated pathological facts show conclusively that there are other affections of the air-passages, whose characteristics and morbid relations are still involved in obscurity. To point out the seat and nature of one of these affections, to investigate its causes, and from a knowledge of its true pathology to establish correct principles of treatment, are among the objects of the present inquiry." (p. 24.)

Before, however, entering upon the special subject of his work, Dr. Green takes a brief survey of the principal pathological changes upon which the diseases known as chronic bronchitis, laryngitis, &c., are supposed to depend, with the object of affording his readers the means of appreciating his own views more fully.

The structural changes to which, according to Dr. Green's views, the mucous follicles of the throat are liable, are inflammation, ulceration, hypertrophy, induration, and tubercular degeneration, attended, in most of these conditions, by a greatly increased and vitiated mucous secretion. The disease of the mucous glandulæ may be *primary* and *uncomplicated*, and be limited entirely to the fauces and pharyngo-laryngeal membrane; or it may be *complicated* with *hypertrophy and induration of the tonsils*, and with *elongation* of the *uvula*. It may accompany, or be consecutive to other affections of the air-passages, and coexist with laryngitis, bronchitis, or pulmonary phthisis.

The literary history of the follicular disease of the pharyngo-laryngeal membrane is very barren. Dr. Badham is referred to by Dr. Green, as being the first who expressly called attention to it, under the designation of bronchitis. In the United States it first attracted attention as a sequel to an epidemic influenza.

"The first case of well-marked follicular disease, which came under my notice, was that of a clergyman, and occurred in the early part of 1832. During the preceding year, however, the attention of practitioners in different parts of New England had been called to the fact, that many clergymen in different sections of the country were seriously affected, and in some instances were wholly incapacitated for public speaking by 'a distemper of the throat,' which was characterized

by symptoms of a peculiar nature and of unusual severity. By inquiries directed to this point, I have not been able to ascertain that any strongly marked cases of this form of disease had been observed prior to 1830. During this year it will be remembered that an epidemic influenza prevailed in this country, which not only extended over all the United States but spread throughout Europe, and so far as is known, over the whole civilized world. Whether this epidemic, or the causes upon which it depended, had any agency in increasing the frequency of the disease under consideration, or in changing its character into one of a more malignant nature, it is impossible now to determine. Certain it is, however, that while the influenza of this period was the precursor of epidemic cholera in some parts of the world, in many portions of the United States it was early followed by the above form of follicular disease. That this affection in its more aggravated form is of recent origin, or that it was formerly confounded with other diseases of the air-passages, we have the concurrent testimony of various writers who have alluded to the prevalence of the complaint. Professor C. A. Lee, of New York, who published in 1836, 'An Inquiry into some of the Causes of Disease among the Clergy,' remarked, under the head of 'Chronic laryngitis,' upon the alarming degree of prevalence which this disease had attained among that class of persons, and adds, 'It is but a few years since this disease was unknown almost by name, or if now and then a case did occur, it was generally of so mild a character, as to yield to very simple treatment.' For some time after the appearance of the disease in this aggravated form it seemed to be confined in its attacks to public speakers, and of these the clergy were the most frequent sufferers. Hence the affection was early called the 'Clergyman's Sore Throat.' It was soon found, however, that individuals of every profession, of all occupations, and of different ages and sexes, were liable to the disease. Of nearly four hundred cases that have fallen under my observation, only about seventy-eight, or one in five of this number, were in any way public speakers. When, however, the affection does occur in those persons who are in the habit of exercising the vocal organs by public speaking, singing, teaching, &c., it is always, for obvious reasons, attended with symptoms of a more aggravated nature, than when it appears under ordinary circumstances." (pp. 45-7.)

The first stage of follicular disease of the pharyngo-laryngeal membrane is characterized by subacute inflammation of the mucous follicles of the fauces and pharynx, and in its uncomplicated form, it is only by an extension of inflammatory action that those of the epiglottis, larynx, and trachea are affected. This is Dr. Green's opinion, but we think he will find it necessary to modify it, as it excludes the probability of the follicles of the epiglottis and larynx being affected primarily.

We subjoin Dr. Green's description of the progress of the disease:

"So insidious frequently is the onset of the disease, and so gradual its progress, that in some instances it will be found to have continued many months and to have made considerable advance before the presence of any prominent local symptom shall have called the attention of the individual to the existence of the affection. He then perhaps becomes aware of an uneasy sensation in the upper part of the throat, accompanied by a frequent inclination to swallow, as if some obstacle in the passage might be removed by the act of deglutition; or, more frequently, there is an attempt made, and often repeated, to clear the throat by a kind of screatus or hawking, and to relieve it of a sensation of 'something sticking at the top of the windpipe.' About the same time there is observed an alteration in the quality or *timbre* of the voice; there is experienced in the vocal organs a loss of power, and a hoarseness is present which at first is hardly perceived in the morning or after a full meal, but which is increased towards evening, and after speaking or reading longer or louder than usual. The mucous secretion, which, in a healthy condition of the glands is bland and transparent, becomes viscid, opaque, and adherent, and is increased in quantity. Frequently there is a slight

soreness felt about the region of the larynx, but seldom is any cough present at this stage of the disease. In this condition the symptoms may remain for a long period, sometimes for years; nearly disappearing at times, and then again being greatly aggravated by vicissitudes of temperature, increased exercise of the vocal organs, and by various other morbific causes. If we inspect the throat and fauces during the progress of the above symptoms, we shall find the epithelium, which in the healthy state of the mucous tissue covers its surface, more or less destroyed; its absence being manifested by the slightly raw or granulated appearance which the membrane presents; the mucous follicles will be found hypertrophied, and will appear distinctly visible, especially those studding the upper and posterior part of the pharyngeal membrane. If the disease has been long-continued, a portion of the follicles may be found indurated, or, in some instances, filled with a yellowish substance, having a resemblance to and presenting the physical characters of tuberculous matter; whilst striæ of opaque, adhesive mucus, or of a muco-purulent secretion, may be seen hanging from the veil of the palate, or coating the posterior wall of the pharynx. As the disease advances, and the follicles, situated at the root of the epiglottis and in front of the arytenoid cartilage, and the still more numerous glandulæ of the laryngeal mucous membrane, become involved in the morbid action, all the above symptoms appear greatly aggravated; the hoarseness is much increased, and is constant; speaking or reading aloud is attended with great difficulty; and when continued for any period is followed by pain and increased soreness in the region of the larynx, and by a sensation of extreme languor, not only about the vocal organs, but throughout the whole system. In some cases, where the disease affects the glands situated in the ventricles of the larynx and near the vocal chords, the voice becomes completely extinguished; or, if by great effort, the patient essays to speak aloud, the vocal resonance is uneven, harsh, and discordant. In such cases, notwithstanding the situation and extent of the disease, there is seldom present any decided or troublesome cough; and, in this respect, follicular disease differs essentially from all other equally grave, laryngeal affections. Cases have fallen under my observation repeatedly, where the affection had advanced until the symptoms present indicated extensive disease of the follicles of the larynx and of the membrane covering the vocal ligaments; until the ulceration of these glands, situated at the root of the epiglottis, could be felt upon the laryngeal surface, and yet the patient would remain free or nearly free from a cough, notwithstanding an abundant acrid secretion, poured out by the diseased follicles, would occasion an incessant hawking to clear the upper part of the windpipe and the pharynx of this tenacious mucus." (pp. 50-3.)

It is of importance to remember, however, that the disease will commence and become inveterate without any symptom whatever being referred to the throat. The patient is teased by a constant irritating cough, his breathing is difficult, he snores loudly in consequence of the thickened membrane, and awakes suddenly from the short slumbers which constitute the sole repose he gets, and coughs up a viscid mucus; yet he will express himself as having never experienced any pain or uneasiness in the throat or larynx, and is surprised that such particular reference should be made to the condition of the throat, inasmuch as the seat of the irritation, as felt by himself, is at the root of the neck, about the point corresponding to the bifurcation of the bronchi. The symptoms vary in fact so much in different individuals, that it is evident a much more minute and accurate investigation is requisite, with a view to a proper classification of the different forms under which this follicular disease appears. The following case will serve to illustrate its origin and progress as the "clergyman's sore throat:"

"In June, 1841, Rev. Mr. S., of this city, aged 34 years, came under my care

for an affection of the throat, which, for the four years preceding this period, had incapacitated him for public speaking. The disease came on much in the same way as its access is described to have been in the preceding cases. Whilst engaged in his official public duties, he had observed for some time an increasing irritation and soreness about the fauces and throat, accompanied by a hoarseness and a frequent inclination to clear the voice when speaking. These symptoms, which ordinarily passed off after a few days of rest, were disregarded until his voice failed, and he suddenly broke down under his pastoral labours. Finding himself entirely unable to discharge his professional duties, although still possessing a good degree of general health, and after resorting to various measures to restore to a sound condition his vocal organs, all of which proved ineffectual, he resigned his ministerial charge, and sailed for Europe; cherishing the hope—in which his numerous friends ardently participated—that the sanative influence of a sea-voyage and a foreign tour would prove efficient in removing his most troublesome and perplexing malady. After an absence of many months he returned to his home, invigorated by this long relaxation from his duties, and although there still remained some sensibility of the larynx, and a slight huskiness of the voice, yet he felt that with due caution he might with safety gradually resume his public duties. A single attempt, however, to speak in public soon after his return, entirely dissipated this hope. His voice again gave way; hoarseness, with every other morbid symptom which previously attended his disease, returned in a still more aggravated form than at first. In this condition his case remained—marked by an occasional remission of the symptoms, but attended by no permanent improvement—until the period above mentioned, when, as I have stated, he came under my care. His condition at this time was as follows: His general health, although impaired by the long-continued local disease, was still very good; yet his countenance exhibited a sallow hue, and was marked by that care-worn and anxious expression which I have often observed in those cases of tuberculous sore throat which have been protracted through a long period of time. Constant hoarseness was present, and his voice when uttered aloud was rough and hollow. Speaking was accomplished with difficulty, and if continued, as in ordinary conversation, for a short time only, was followed by soreness and increased hoarseness, and, sometimes, for a short period, by complete aphonia. His throat, on being inspected, presented an enlarged and cavernous appearance; as if the pillars of the fauces and the pharyngeal muscles had become atrophied, or had been wasted away by disease in a manner greatly to enlarge the posterior fauces. The mucous membrane lining these parts was covered by diseased follicles, some of them greatly enlarged and indurated, others slightly hypertrophied, and filled with a semi-fluid substance resembling tuberculous matter. On pressing down the tongue, the epiglottis could be seen standing above its base, erect and œdematous; its edges red and slightly ulcerated; whilst a vitiated mucous secretion was being constantly poured out from the diseased glands, occasioning an incessant hawking to relieve the throat of this cause of irritation. Pressure upon the thyroid cartilage increased the pain and soreness, which were constantly felt in the larynx. This last symptom, together with the permanent hoarseness and the partial extinction of voice, was plainly indicative of the mucous follicles of the ventricles of the larynx and of those around the chordæ vocales being involved in the disease. Yet there was no cough present, nor could a rigid examination of the chest detect any morbid alterations in the pulmonary organs. All mental excitement affected him injuriously; it had a tendency invariably to aggravate the local difficulty, although no effort whatever might be made to exercise the vocal organs. The tongue was coated, his pulse seventy-six in the minute, and quite feeble." (pp. 58-61.)

The subjoined illustrates the progress of the affection in a barrister:

"K. H. E., Esq., a lawyer of eminence in this city, aged 38 years, suffered from an attack of acute bronchitis, in April, 1840. Under the most active treatment he recovered from the disease, and resumed his professional duties. In

1841-2 he was a member of the Common Council, and, in addition to the duties of a full practice which necessarily involved much public speaking, he was frequently engaged in the exciting debates of the honorable body of which he was a member. Early in 1842 he began to be sensible of a slight huskiness of the voice, and of an uneasy sensation in the throat after public speaking. These symptoms would all subside after a little rest, but only to be renewed at each subsequent public exercise of the vocal organs. It was observed that this hoarseness gradually increased, and that the irritation about the throat impelled the individual to make frequently repeated efforts at hawking, as if to remove some obstruction from the larynx. Being in attendance upon his family during the progress of these symptoms, I had frequent opportunities to inspect his throat, and I observed that the follicular glands of the isthmus of the fauces and of the superior portion of the pharyngeal membrane were slightly hypertrophied, and were pouring out an altered and increased secretion. Believing on his part that these morbid symptoms would pass away, no special attention to his case was required or given until the latter part of July, 1842. At this time a permanent hoarseness was present; the voice was rough and uneven, with a constant irritation and a sensation of soreness in the laryngeal cavity; symptoms that were all greatly increased by every effort made and continued to speak or read aloud. The diseased follicles now presented a very different appearance from that which they had exhibited a few weeks before. The posterior fauces and pharyngeal membrane were studded with elevated tubercles, with inflamed bases, or granulations of different sizes, like pustular inflammation, bearing a marked resemblance to the papulæ of varioloid. The most pendant portion of the uvula, which was greatly elongated, was now covered by similar diseased follicles." (pp. 64-5.)

In some examples, the disease goes on until the symptoms attain so great a degree of intensity as to simulate tubercular disease of the lungs. Of this by no means uncommon circumstance, the subjoined case is an instructive example; it is that of a merchant, aged 37:

"Coming on in the insidious manner that has been described, the disease had made considerable progress before his case was deemed of sufficient importance to require medical treatment. At length he placed himself in the hands of some homœopathic physicians of this city, under whose treatment he remained for two or three years. In the meantime the disease, of course, continued to advance, until extensive ulceration of the pharyngo-laryngeal follicles had taken place. Still placing his confidence in this 'fabric of a vision'—Homœopathy, he visited Paris, and put himself under the care of the celebrated Hahnemann, who treated his case for three or four months; but with as little success as had attended the prescriptions of his satellites in New York. Discouraged at last, or losing confidence in the plan of treatment proposed by Hahnemann, he came back to New York, and soon after his return placed himself under my care. His general health at this time had become much impaired, doubtless through the long-continued influence of the local disease. The whole throat, which presented an atrophied and cavernous appearance, was studded with diseased follicles, some of which were greatly enlarged and vascular, or were filled with tuberculous matter; whilst others were broken down and destroyed by ulcerations. The uvula was elongated, and the epiglottis, which could be seen above the back of the tongue, was erect and œdematous, and its circumference was serrated with ulcerations. On examining the epiglottis with the finger, an extensive and deep ulcer could be felt at its base, on the laryngeal surface of this cartilage; whilst the pain in the larynx, the soreness experienced on pressure over the thyroid cartilage, together with the constant hoarseness and irritation in the laryngeal cavity, showed conclusively that the ulcerations had extended to the vocal ligaments. The patient complained of a dull pain, and a sensation of great weakness under the sternum; there were present also, a cough, emaciation, erratic pains in the chest, and other constitutional symptoms that indicated the presence of tubercles in the

lungs; but, notwithstanding many of the rational symptoms of phthisis were present, a careful examination of the chest was made, without detecting any structural lesions in the pulmonary organs. Bronchial irritation, however, existed to a considerable extent." (pp. 69-71.)

We observe here that the plan of treatment adopted by Dr. Green was so eminently successful in this case, that his patient was ultimately restored to robust health. We could add also, if our space would permit, another case presenting all the rational signs of ulceration of the larynx, with the voice reduced to a rough whisper, in which the repeated application of a strong solution of crystallized nitrate of silver (forty grains to an ounce of distilled water) to the cavity of the larynx, combined with other treatment, was perfectly successful in restoring the patient to health and the full performance of his duties as a clergyman. Another example of a merchant is given, in whom the voice had been no louder than a whisper for fourteen months, and who was treated by the nitrate with the same success: but we are anticipating our subject.

In the examples of follicular disease above detailed, no difficulty of deglutition was experienced; in some, however, this is a prominent and urgent symptom. A married lady, aged 32 years, exhibited some of the earlier signs of phthisis, to which she had a hereditary predisposition, but from which, however, she recovered. Some few months afterwards—

"She began to experience a slight uneasiness in her throat, accompanied by soreness, and a constant desire to clear the passage; symptoms which were soon followed by hoarseness, a slight cough, increased tenderness in the laryngeal region, and a difficulty in swallowing. So urgent, indeed, had this latter symptom become, that for several weeks preceding her visit to New York, she had found it very difficult to swallow her food, unless taken in a liquid form, or made soft by careful mastication. I saw her on the 3d of September, and found her pale, feeble, and emaciated; pulse languid, voice hoarse and raucous, and at times reduced to a whisper; cough frequent, and accompanied by a slight expectoration of viscid mucus. She complained of a difficulty of deglutition; a sense of smarting in the gullet, and soreness whenever pressure was made over the thyroid cartilage. On inspecting the throat, the enlarged cavity of the posterior fauces was found covered with diseased follicular glands; some of them in a state of ulceration, and others were filled with a puriform matter. About one third of the epiglottis was in sight; its circumference and lingual face were extensively ulcerated, which accounted for the pain and difficulty which the patient experienced in deglutition; the uvula was considerably elongated, and the curtain of the palate was pale, relaxed, and œdematous." (pp. 87-8.)

Follicular disease of the tonsils. In some patients the tonsils are the principal seat of the disease. In the following example of a merchant, aged 25, a prominent homœopathic physician of New York had treated the patient for three or four months, and had assured him three days only before he consulted Dr. Green that every vestige of disease was removed from his throat!

"The whole outward or rational symptoms manifested by this patient are those which indicate an advanced stage of pulmonary phthisis. He is pale and greatly emaciated; countenance presents an anxious and haggard expression; he has copious night-sweats, dyspnœa, and a constant harassing cough, which is attended by a free expectoration of muco-purulent matter; the voice is thick and hoarse, and deglutition is performed with much difficulty. The dyspnœa is greatly increased whenever the patient lies down, and for many weeks no other individual has been able to sleep in the same room with him, on account of the laboured and

stertorous breathing which is constantly present during sleep. The throat, on inspection, is found to be apparently entirely filled up with two enormously large and ulcerated tonsils (see Plate V), between which the uvula, which is also hypertrophied, appears wedged in, like the keystone of an arch. No portion of the pharyngeal membrane can be seen, as the morbid mass completely blocks up the view. How respiration can be carried on, or deglutition performed, with the throat in this state, it is difficult to imagine; and still more difficult is it to comprehend how a learned physician could have pronounced such a throat to be in a healthy condition! On exploring the chest no morbid signs are detected, except such as indicate the presence of some degree of bronchial irritation. But there is soreness in the region of the larynx, and pain, which is increased by pressure over the thyroid cartilage." (pp. 92-4.)

In this case the tonsils were of course removed, and the operation laid the pharyngeal follicles open to view in a hypertrophied and ulcerated condition, to which the solution of nitrate of silver was applied. Ultimately the patient was restored to perfect health. Dr. Green remarks:

"In the preceding case, the pharyngo-laryngeal affection may, in some respects, be considered as having been complicated with bronchial disease; but the inflammation of the bronchial vessels, which undoubtedly had existed to a considerable extent in the earlier stages of the disease, was not present when the patient came under my care. The effect, however, of the inflammation was still remaining, and this consisted in a dilatation of some of the bronchial tubes. This alteration of structure has led to a great error in diagnosis; for, misled by the external or rational signs—the emaciation and dyspnœa, the night-sweats, and the free muco-purulent expectoration of the patient—several physicians, both in this city and Boston, who had examined the case, had mistaken the bronchial dilatations for tubercular cavities, and had pronounced it to be one of confirmed phthisis. That it was not tuberculous disease, time has proved, by establishing the permanency of the cure. It is now over two years since the patient was under treatment, during this period he has passed through two severe winters, in which he was constantly engaged in business; and yet he has been, and still remains in excellent health." (pp. 95-6.)

Disease of the *follicles of the uvula*, together with hypertrophy and other morbid changes, is noted by Dr. Green as being seldom wanting in a large majority of cases like those detailed. The influence of elongation of the uvula on the pulmonary mucous membrane has been long known to the discriminating practitioner. The irritation it excites induces, after a period more or less prolonged, many of the rational signs of phthisis. The following case illustrates the morbid effects that may be produced by it in the apparent absence of all other causes:

"In 1841, E. B., aged 26, a merchant of this city, suffered severely from follicular laryngitis. During this year he left the north, and spent a part of the inclement season in one of the southern States. But his disease continuing to increase, he came back to New York, and after his return, placed himself under my care. At this time there was debility, loss of flesh, with cough and hoarseness, and a constant irritation about the throat: an examination of which revealed an extensively diseased condition of the follicles of the fauces and of those of the pharyngeal membrane. His uvula was moderately elongated, but not to that extent that seemed to require excision. Under the use of those topical and general remedies which are comprised in the plan of treatment that was employed in many of the preceding cases, this patient was restored to a good degree of health; the hoarseness and soreness of his throat disappeared, the mucous membrane assumed a healthy appearance, and he regained strength and flesh, so that he was enabled again to attend to his business. His cough, how-

ever, never left him, but, on the contrary, notwithstanding the employment of many of the ordinary remedies for its removal, continued to increase, and was attended, after a while, with a free expectoration of muco-purulent matter. The occurrence, at length, of erratic pains about the chest, with debility and other unfavorable symptoms, led the patient and his friends to apprehend the presence of confirmed pulmonic disease. At this crisis, after having resorted to various remedial measures without benefit, I proposed the excision of the patient's uvula, which, although but moderately elongated, was evidently a source of irritation. To this the patient acceded; the uvula was removed, and from that hour the cough ceased, the pain in the chest, and every other indication of thoracic disease which had been present, soon passed away; and from that hour to the present the patient has been free from all pulmonic disease." (pp. 103-4.)

The complication of follicular disease of the pharyngo-laryngeal membrane with laryngitis and bronchitis is frequent, as might *a priori* be expected. There is at first an expectoration of transparent, adhesive mucus. As the disease advances this discharge increases in quantity, and is characterized by the presence of opaque or albuminous matter, commingled with the more transparent liquid mucus. Emaciation, debility, night-sweats, and even slight hemoptysis may also ultimately supervene. In such cases, Dr. Green has succeeded in restoring his patient to a fair degree of health and strength.

The complication of follicular disease with phthisis pulmonalis is, according to Dr. Green, of no unfrequent occurrence, and renders all the symptoms more harassing and distressing; its cure ought therefore to be attempted:

"When coexisting with tubercular phthisis, the cough, the dyspnœa, and all the characteristic symptoms of the latter disease are greatly aggravated, and the affection passes through its stages and reaches its fatal termination much sooner ordinarily than when its progress is unattended with follicular disease of the pharyngo-laryngeal membrane. Although but little can be expected from any attempt made to prevent the final *dénouement* in these cases where disorganization of the lungs has occurred, yet, in many instances, the harassing cough, the difficulty of deglutition, and the dyspnœa which are often present in this combination of disease, are signally relieved, and the sufferings of the patient greatly mitigated, by the employment of topical medication upon the diseased laryngeal surface. So marked, indeed, has this relief been in some cases which have fallen under my observation, even when tubercular cavities have been present, as to awaken in the minds of the patients and their friends strong hopes of their final recovery. In other instances, and these have been not a few, where follicular laryngitis has preceded thoracic disease, I have seen the symptoms of incipient pulmonary affection rapidly disappear after the removal by topical medication of the primary follicular disease." (pp. 119-20.)

Dr. Green details an interesting case of phthisis, in which the treatment was singularly successful in mitigating the disease. Cauterization of the fauces, glottis, and cavity of the larynx was continued every day, on each alternate day, for several weeks; counter-irritation over the superior portion of the spinal column was kept up, and along with the alterative remedies prescribed at the commencement of the treatment, the patient was put upon the use of general tonics. Under this plan of treatment the improvement of his health was marked and rapid. He was able to swallow solid food without pain or inconvenience, his strength increased, his voice was improved, he had regained several pounds of flesh, whilst every evidence of ulceration of the throat had disappeared; and all this

mitigation of the disease took place notwithstanding the symptoms which have been enumerated,—in connexion with the morbid physical signs which auscultation revealed—indicated the presence of extensive tuberculous disease of the lungs. These latter indications increasing, he died of pulmonary phthisis.

Malignant follicular disease of the œsophagus is noticed by Dr. Green, and two cases related, but they present nothing of special interest.

The pathological anatomy of follicular disease is next discussed by Dr. Green; we do not find much addition to our existing information. Hypertrophy may occur, with or without induration of the follicles. When the mass of glands which are aggregated in the tonsils suffer from long-continued inflammation they become indurated. The induration is not generally of a malignant character, but depends, Dr. Green observes, upon the presence of a deposition of fibrin, which during the process of inflammation has been lodged in the cavity of the follicle or in the interstitial substance with which it is surrounded. The matter thus deposited very readily becomes vascular, is supplied with blood-vessels, and at length becomes organized; but this process takes place to a limited extent, for when excised the enlarged tonsil seems to possess but little sensibility.

Calcareous deposit sometimes occurs, and we believe it is much less rare than many suppose. Patients will cough or exscreate little calculi, which have evidently come from the tonsils.

The secretion of these muciparous glands will become morbid, and, like that in other mucous membranes in a state of inflammation, will assume an acrid and irritating quality, and thus the investing membrane is not only stimulated, but the disease is extended to adjoining follicles. Dr. Green remarks also, that hemorrhages will take place from them, and in illustration, mentions the case of a young gentleman, who for a long period has suffered under follicular disease of the throat, and who, without manifesting any symptoms of phthisis, has had repeated hemorrhages from the pharyngeal mucous membrane. The spot indeed can be distinctly seen from whence the blood exudes, and yet no abrasion of the surface, nor the rupture of any vessel can be detected. One or two similar examples have come under our own notice in the persons of inveterate smokers.

Dr. Green states that the secretion of a fluid, possessing all the sensible and chemical properties of pus, is the frequent result of disease of the pharyngo-tracheal follicles. When the disease in these glands has passed on to the stage of ulceration, the purulent secretion is marked and abundant, and its source is apparent; but it sometimes occurs where no structural lesion, either of the follicles or of the lining membrane, can be detected. Generally, however, when purulent secretion takes place in follicular inflammation, the glands are found to be more or less in an ulcerated condition; in many instances they may be seen in the posterior fauces and on the pharyngeal membrane, some of them ulcerated, others distended with purulent matter. The fluid, likewise, which is poured out from the follicles of the tonsils, when these glands are hypertrophied and inflamed, is not unfrequently of a purulent nature.

The deposit of tubercular matter in the substance or on the surface of the larynx, epiglottis, or trachea is denied by M. Louis. Dr. Green combats this doctrine, and quotes Andral, Carswell, Williams, and Hope, in

support of his own observation, that "in enlarged mucous follicles of the upper air-passages, and imbedded in the tissues of these parts, were morbid deposits, presenting all the physical characters of true tuberculous matter." He thinks that Dr. Carswell's supposition that tuberculous matter may often be secreted upon the free surface of the membranes of these parts, but that not being entangled or confined in any mucous crypt, it is removed as soon as it is formed, reconciles the conflicting statements on the subject.

Ulceration of the follicular glands is a frequent termination of long-continued irritation. After the irritation has persisted for some time, the engorged follicle presents a small ash-coloured point, which is surrounded by an inflamed base, and has red and slightly-elevated edges. In follicular disease, these ulcers, which ordinarily spread slowly, are generally first observed about the arches of the palate and on the back of the pharynx; they next attack the laryngeal face of the epiglottis, and the epiglottic glands situated at the base of this cartilage, and spreading by continuity, they in some instances invade the mucous follicles in the ventricles, and around the chordæ vocales. Indeed there is no part of the larynx and trachea that may not be the seat of ulceration. In their early stages, ulcerations of the mucous glandulæ are small and superficial; continuing for a long time, not only are the glands destroyed, but the mucous, the sub-cellular tissues, and even the cartilages themselves may become involved in the ulcerative process.

Thickening of the mucous membrane of the pharynx, &c., is an early change in the progress of follicular disease, but eventually an opposite state of things takes place, for not only are the surrounding engorged membranes unloaded, and their increased thickness removed, but the sub-cellular tissues and the pharyngeal muscles become atrophied; in part, probably, from the increased absorption which has been set up; and we then have, on inspection, those enlarged or cavernous throats, so frequently observable in long-continued follicular disease, and to which allusion has more than once been made.

Causes of follicular disease. These are either predisposing or exciting. Dr. Green notices first as a predisposing cause, a hereditary tendency to the disease. That this exists in families is, he observes, a well-established fact.

"At the present time, I have under treatment three brothers, clergymen, who have been compelled to relinquish their official public duties on account of follicular disease of the throat; and whose mother, now over eighty years of age is labouring under the same affection. In another instance, coming also under my observation, four members of the same family, with one of the parents, were the subjects of follicular disease; and among my notes a large number of cases are recorded, where two and three members of the same families have been treated for this affection." (pp. 158-9.)

A strumous diathesis, and all those influences which induce a cachectic state of the system, predispose to the affection, and among these influences no one is more prominent than impure air, such an atmosphere, for example, as clergymen, teachers, lecturers, and other public speakers, are frequently compelled to breathe in crowded and ill-ventilated churches, lecture-rooms, &c. Severe and protracted labour, conjoined with anxiety about temporalities, is a very frequent cause of the disease amongst cler-

gymen, especially "that numerous class who, settled in the towns and villages of the country, are compelled to sustain themselves and their families upon salaries which, with the practising of a most rigid economy, are barely adequate to supply them with the necessaries of life."

The male sex is more predisposed to follicular disease than the female. Louis has already established numerically, that a much greater proportion of males suffer from ulceration of the trachea, &c., in phthisis, than females. Of one hundred and ninety subjects carefully examined by him, one half of the males, but only one fourth of the females, presented ulcerations of the trachea; a similar but rather less proportionate difference was observed with reference to ulcerations of the larynx and epiglottis. Louis found that there was a greater difference in the frequency of ulcerations of the bronchi in the two sexes. Forty-seven phthisical subjects were examined by him; nineteen were women, of whom five had bronchial ulcers, or a proportion more than one fourth; whereas, seventeen of thirty men were thus affected, or more than one half, so that Louis's data do not bear out Dr. Green's observations, that the upper portion specially of the respiratory tube is more affected in men than women; on the contrary, we find the proportionate preponderance of males affected by ulcerations of the respiratory mucous membrane diminishes from the bronchi upwards, it being least with regard to the larynx and epiglottis. There are various reasons which might be assigned for this preponderance. The respiratory system is more highly developed in males; is more exposed to the ordinary causes of inflammation; tobacco-smoking in particular, a habit almost exclusively masculine, is more likely to induce in males pulmonary disease of the kind under consideration, by the irritation excited in the pharyngo-laryngeal tissues. The vocal organs are also called into unequal or prolonged exercise by the avocations of life more generally in the male sex than in the female.

The age of the largest number of patients treated for follicular laryngitis by Dr. Green was from 25 to 35; in very few was the disease manifested at an earlier period.

Among the exciting causes, Dr. Green enumerates an attack of influenza or of eruptive fever, but particularly the habitual use of tobacco:

"As an exciting cause, the use of tobacco, in my experience, has proved a powerful agent in the production of follicular disease of the throat. Acting as a stimulant, directly and constantly, upon the mucous follicles of the fauces and throat, and greatly increasing, as it does, the secretion of these glands, its employment, as we should conclude, *a priori*, must have a direct tendency to develop the disease, especially if a predisposition to the affection exists; hence it has occurred to me to notice, that of a great number of cases of throat-ail, which during the last year or two have come under my observation, a large proportion of them have taken place in individuals who had been, or were at the time, in the habitual use of tobacco. My attention has been called more particularly to this subject, from having noticed, several years ago, some observations on the use of tobacco in laryngeal and bronchial affections, by an eminent surgeon of this city. After having alluded to the almost universal use of tobacco in the countries of Northern Europe, he observes—'In one very fatal and distressing form of disease, to wit, laryngeal phthisis and bronchitis among public speakers, the fact is very clearly established, that the moderate habit of smoking, by the drain it accomplishes, and its anodyne qualities, has been eminently useful, at least as a preventive, of that peculiar malady so frequent in the United States, especially among

the clergy.'* From this opinion of my distinguished countryman and friend I am compelled to differ entirely by the statistical facts which I have obtained on this subject. Not only has the use of tobacco, in any and all its forms, proved in my experience an exciting cause of laryngeal disease, but where its employment has been persisted in during the treatment of any case, I have found it impossible to restore such to perfect health." (pp. 175-7.)

Dyspepsia has been mentioned as a cause of the disease, but Dr. Green is disposed to consider it in many cases rather as an effect; and that it is induced by the vitiated secretion from the follicles carried into the stomach with the food and drinks of the patient.

The regular use of the voice is not to be considered as an exciting cause, as Dr. Green found few barristers or auctioneers to be affected, but he thinks that an irregular or sudden and violent use of the vocal organs will bring it on, especially when a predisposition exists. Thus clergymen suffer rather than barristers, because their duties lead them to prolonged public speaking on one day of the week only, while during the remaining six the vocal organs are nearly quiescent.

Symptoms of follicular disease. These our readers will have already become acquainted with from the details of the cases quoted above. Perhaps, however, it will be useful to recapitulate them.

In some cases the disease will have made considerable progress before the attention of the sufferer is directed to the throat, but—

"Ordinarily, however, soon after the mucous glandulæ have taken on a morbid action, there is perceived in the region of the fauces an increased mucous secretion, and an uneasy sensation in the gullet or upper part of the throat is observed, attended by a frequent desire to swallow, as if some object sticking in the passage might be removed by the act of deglutition; or, more generally, repeated attempts are made by hawking to clear the throat, and allay the irritation; all which difficulties are considerably augmented by every continued effort made to read aloud, to sing, or to speak as in ordinary conversation. If the secretion from the mucous follicles of the throat be examined at this period, it will be found to be altered in its character, being adhesive, and, in some instances, of an alkaline quality, and proving to be, by its effect on the mucous membrane, of an irritating nature." (pp. 178-9.)

Alterations in the tone of the voice now become perceptible, particularly if the individual be accustomed to sing or speak in public, and is more perceptible in the evening than in the morning.

"On inspecting the throat, the fauces and the posterior wall of the pharynx will appear redder than natural, and the mucous membrane covering these parts will be deprived of its epithelium, injected, and studded over with enlarged mucous follicles. Sometimes, if the disease is recent, these glands will appear quite minute, and will be distinctly apparent only when the pharyngeal cavity is exposed to a full light. In other instances they will have attained a size sufficient to give a rough or granular appearance to the whole surface of the fauces, while the viscid tenacious mucus which is poured out by these follicles in their morbid state may be seen coating the membrane, or appearing in patches, or marking its surface with white or yellowish-white striæ. In some cases several of the enlarged and morbid cryptæ will become confluent, and, uniting, form angry-looking tubercles, of the size of a split pea, which may be seen on the posterior wall of the pharynx. In others, again, a deposition of textural matter takes place, and the follicle becomes indurated and permanently enlarged; or it may be distended with pus, or with a morbid secretion, which

* Travels in Europe and the East. By Valentine Mott, M.D. pp. 83-4.

will exhibit all the physical properties of tuberculous matter. If the affection has continued for some time, we shall frequently find some of the diseased follicles in an ulcerated state; these are generally first observed about the palatine arch, the posterior wall of the pharynx, and along the border and on the laryngeal face of the epiglottis. In the first stage these ulcers are small and superficial, appearing in the form of ash-coloured patches, surrounded by an inflamed and slightly elevated base. Continuing, they at length destroy the mucous follicles, and sometimes involve not only the mucous, but the sub-cellular, tissues in their progress. Accompanying the above symptoms, there is often found œdema and elongation of the uvula, and in many instances hypertrophy of the tonsils." (pp. 179-81.)

In the incipient stage of follicular laryngitis, of the uncomplicated form, there is seldom much cough present. The irritation that is felt in the larynx, and which is caused by the increased and vitiated secretion from the diseased follicles, is generally relieved for the moment by hawking in this stage of the affection. As the disease advances, however, and the glandulæ of the larynx and trachea become involved in the morbid action, a cough will steal on, which, from being slight at first, is at length severe, and in most cases is attended by a free tenacious expectoration. In two examples under our care, and in which we had no reason to suspect laryngeal ulceration, the cough was incessant, harassing the patient both night and day, and was by far the most prominent symptom. Nor was the expectoration in one of these cases "free:" indeed the nature and amount will depend entirely upon the extent of the bronchial irritation excited; so that the diagnosis between the earlier stages of tuberculous disease and a follicular affection, founded by Dr. Green upon the circumstance that, in the latter, there is a free tenacious expectoration, while, in the former, it is only a trifling amount of transparent frothy fluid, is somewhat apocryphal. We are inclined to think that those gray firm pellets of consolidated mucus, occasionally expectorated by individuals otherwise healthy, indicate the first stage of follicular disease. We fully coincide, however, in the subjoined opinion:

"In another respect, these two diseases are essentially different. That peculiar mental condition incident to pulmonary disease,—by which the spirits of the patient are buoyed up, and hope often continues bright to the last,—is well known. The reverse of this obtains in follicular laryngeal disease. In this latter affection *mental depression* is, to some extent, so universally present, particularly where the affection has been protracted, that I have been led almost to consider it a characteristic of the disease." (pp. 183-4.)

After the disease has extended downwards, and ulceration within the larynx is established, the cough becomes much more severe, and the voice more altered. The tone of the voice is in some degree a guide to the seat of the irritation: if the ulcerations are confined to the follicles about the tonsils, the veil of the palate, and the pharyngeal membrane, the timbre of the voice is not ordinarily much changed; incomplete disphony sometimes exists, or, in other words, the sounds are merely obscured, or imperfectly articulated. But let the ulcerations extend below the epiglottis, and the hoarseness is greatly increased, the voice loses its power, and should the mucous glands within the ventricles and around the vocal chords become involved in the morbid alteration, it is reduced to a state of complete aphonia, and a harsh whisper, which is merely an articulation of the ordinary respiration, alone remains.

There is one symptom, which we think Dr. Green does not notice, but which the practitioner will find a frequent accompaniment of this follicular disease, and that is, a loud snoring during sleep, far louder than the ordinary snore of the comfortable sleeper. This will be more particularly heard in hypertrophy of the tonsils or uvula, and in thickening of the pharyngo-laryngeal membrane.

When the epiglottis, and particularly its inferior border, is extensively ulcerated, great difficulty of deglutition, with pain, and sometimes dyspnœa, is experienced. There is an expectoration of an opaque secretion, which seems to come from the very top of the throat, and this frequently increased after eating, and is sometimes tinged with blood; or small masses of dark, almost coagulated blood, will be mingled with the sputum. These ulcers often escape observation, and keep up irritation after the other portions of the fauces are restored to their normal condition. To detect them, it is necessary to draw the whole mass of the tongue forward with a broad curved spatula, which we shall more particularly describe under the head of treatment. Dr. Green attaches considerable importance to the characters and situation of these ulcers as aiding the diagnosis, with reference to the condition of the inner surface of the larynx, although no very specific information is given to his readers on this important point.

"If the follicles, situated along the border and on the laryngeal face of the epiglottis, become ulcerated, I have observed that this organ, which in its normal state is slightly crescentic, loses this form, and appears flattened like the tongue; it is, moreover, enlarged and thickened, and its border may be seen frequently serrated by the erosions. When that cluster of follicles, which constitutes the epiglottic gland, becomes the seat of ulceration, the epiglottis will assume nearly an erect form, and be found incurvated, or its crescentic shape considerably increased; and when this lesion has extended to the numerous glandulæ of the ventricles, and to those around the chordæ vocales, the above alteration of the form of the epiglottis will be still greater; its lateral edges will then be found rolled in towards each other, so that the organ will present nearly a tubular form, with its convexity towards the dorsum of the tongue.....Another aspect, indicative of organic changes, which the epiglottis presents, is that lesion which Dr. Stokes calls the leaf-like expansion of the epiglottis. In one instance observed by this writer, the epiglottis was thinned and singularly elongated, and its form so altered as to present the shape of a battledore, the narrow extremity being next the glottis. In this instance, the lesions of the epiglottis coincided with double perforating ulcers of the ventricles." (pp. 188-90.)

Treatment of follicular disease. This is divided into two parts, topical and general.

With regard to the topical treatment of pulmonary, pharyngeal, or tonsillar disease little need be said; inhalation, insufflation, lotions, and the application of the solid nitrate of silver to the tonsils, uvula, and pharynx, are the well-known means adopted for this end. The application of nitrate of silver to the cavity of the larynx is not, however, to be classed amongst these ordinary methods; and the practice of it by Dr. Green seems to have been received with such incredulity in the United States, that Dr. Green has thought it necessary to multiply evidence as to the fact that he has introduced a strong solution of nitrate of silver within that cavity. Trousseau and Belloc are supposed, by Dr. Green, to have been the first to prescribe and employ topical medication in chronic laryngeal disease. They found a solution of the nitrate of silver, in the

proportion of two drachms, or sometimes, four drachms, to an ounce of distilled water, to be the most efficacious and harmless application. Two methods were adopted by them: the one was to saturate a small sponge attached to a bent rod of whalebone, and to manipulate so that the solution be expressed into the larynx; the other was to use a small silver syringe, with a tube suitably adapted for effecting the same object. Dr. Green, however, several years before the appearance of Messrs. Trousseau and Belloc's book, had instituted experiments, and came to a similar conclusion. Without wishing to disparage in the slightest degree the labours of our Gallic or American brethren, we subjoin a case which shows that above thirty years ago, our own countryman, Sir Charles Bell, successfully adopted the method of treatment so fully illustrated (and we think established too) by Dr. Green:—the italics are our own.

"Third case of disease of the larynx (from 'Surgical Observations, being a Quarterly Report of cases in Surgery;' by Charles Bell. London, 1816, p. 34). *Fanny Murray*, æt. 25, Regent's Ward. A few days ago Dr. Southey sent this girl, then an out-patient, to be examined, and as a case deserving to be watched. I immediately recognised the symptoms in the case of Mellon. She spoke in a whisper, scarcely audible, and with great effort; when desiring to be heard, she made an effort like a cough; *I put my finger into the glottis, and felt it rough.*

"*April* 18*th.* I was requested by Dr. Southey to visit his patient. She had been brought into the hospital very unwillingly, and is in imminent danger of suffocation. Her sense of danger has led her to the hospital. Since Christmas she has been ill; she was at that time attacked with cold and sore-throat, and from the beginning she could speak only in a whisper. Her voice has never returned, and at present her whispers are scarcely audible. She coughs in a very singular manner; she says it is an inward cough; it is the exertion of coughing without the sound. This I imagine to be produced by the want of action in the slit of the glottis, by which the air should be somewhat impeded, and thereby more forcibly sent through the glottis. She has pain at the pit of her stomach and in the loins, which I attribute to the exertion and fatigue of coughing. For these three nights she has not been able to lie down in bed. She expectorates a good deal: it is mucus and pus. Pulse 63, br. 42 in the minute. Her breathing has a harsh, sawing sound.

"*Evening of the* 18*th.* The hospital attendants becoming alarmed at the condition of this woman, I was sent for at eleven o'clock. She was sitting up in bed, breathing with much difficulty; but her countenance was of a red colour, the violence of the fit had subsided, and the blueness had been succeeded by redness and fulness. Dr. Southey came in. We wished her to swallow: she tried a little broth; much of it went into the windpipe, and she had a great struggle in recovering. We concluded that the epiglottis was eaten away by ulceration. I proposed mercurial fumigation, but my colleague did not expect benefit from it, as the patient had been three weeks under salivation without the slightest advantage. Having ascertained, by *putting my finger over the root of the tongue into the glottis, that it was rough and irregular with ulceration, I proposed to touch the surface with the argentum nitratum.* It was considered hazardous, but something was necessary, and I was confident that the application would allay irritation. I made a small pad of lint, and attached it to the ring of a catheter wire, and bent the wire so as to pass over the root of the tongue and epiglottis; *I dipped the lint in a solution of twenty grains of the caustic to half an ounce of water, and touched the glottis with it in this manner.* With the fingers of my left hand I pressed down the tongue, and stretched the forefinger over the epiglottis; then, directing the wire along my finger, *I removed the point of the finger from the glottis and introduced the pad of lint into the opening, and pressed it with my finger.* On withdrawing the lint, instead of coughing, she

began to speak more audibly than usual, and had neither cough nor spasm from this rough operation. I repeated the application four times, and her breathing was sensibly better when I left her.

"19*th*. This morning, at the visit, I found her considerably better. I touched the chink of the glottis again with the some solutions. I recommended issues to be made on the sides of the throat.

"20*th*. The issues not made. Last night it was reported to me that she was not so well, but this morning I find her remarkably better. I again touched the glottis with the solution of caustic, notwithstanding she complains of soreness of mouth from the former applications; it had the same good effect, improving her breathing as formerly. A pill of belladonna is ordered, and she is to smoke the stramonium: she is to put her feet and legs in the nitro-muriatic bath twice a day.

"22*d*. She has had the acid bath to her feet several times, and has been twice in the slipper-bath, with the acid more diluted, and the fluid reaching her loins. She speaks with more force, coughs less. Still she does not swallow better.

"26*th*. At Dr. Southey's request I again touched the glottis with the caustic solution. *I applied it as strong as it could be made*. The girl is frightened, but better.

"29*th*. *The girl is remarkably better;* she has taken the bath twice a day; her gums are sore, swollen, and of a dark red colour. Her countenance is much amended; she breathes without noise, and can swallow better than she did.

"*May* 4*th*. She can swallow better; she breathes easily, but she is full of complaints and uneasiness. Her mouth and throat are much inflamed, the gums dark coloured and spongy. She is to take the nitro-muriatic bath every second night.

"5*th*. The bath to be altogether omitted. Dr. Scott visited this patient; he never saw the mouth so sore from the operation of this remedy.

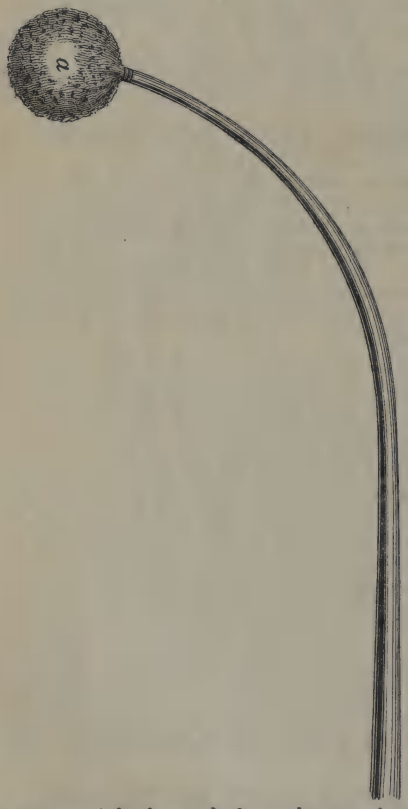
"30*th*. I examined this girl yesterday, and find her almost well. She says that she has not been so well for years as she now finds herself. Her countenance is indeed that of health. In swallowing, she takes five or six draughts in rapid succession; but when she swallows leisurely a single mouthful, as I made her do to-day, she does it without coughing. Some weeks after this Report, Fanny Murray placed herself in the way of the surgeon's round, and gave me her thanks in a very eloquent manner, to the amusement of the pupils; and next day she took an unceremonious leave of the hospital."

We learn from Dr. Watson's 'Lectures on the Practice of Physic,' that this was not the only occasion on which Sir Charles Bell applied the solution of the nitrate successfully. A man under Dr. Watson's care, in the Middlesex Hospital, had acute laryngeal disease, for which the operation of laryngotomy was successfully performed by Sir Charles. About a week after the opening in the larynx had closed up, the patient began to wheeze, speak in a hoarse voice, and in a day or two had a paroxysm of extreme dyspnœa. Dr. Watson got Sir Charles to examine the interior of the throat, and they agreed that it would be proper to "swab" the epiglottis and upper part of the air-passages with a strong solution of lunar caustic. Bell applied the sponge with little difficulty, and the next day the breathing was improved, and the hoarseness almost gone. (Lond. Med. Gaz. vol. xxviii, p. 777, Aug. 1841; also Lectures, vol. i, p. 809, first ed., 1843.) Dr. Watson explains, that the naval term, "swabbing," originated with the late Mr. Vance, who was for many years a surgeon in the navy, and who used the application a good deal in his practice. We subjoin Dr. Watson's own words:

"It is said that a little practice will enable a person to pass his finger into a patient's throat, and to familiarize his sense of touch with the ordinary condition

of the upper part of the respiratory apparatus, so as to be able to detect swelling, or irregularity, or thickening about the chink of the glottis. And great advantage is said to have been obtained from applying remedies directly to the diseased or irritable part. This practice was much followed by the late Mr. Vance, who had been for many years a naval surgeon, and he called it, in naval phrase, *swabbing* the affected organ. A small piece of sponge, secured with a string, or fastened to the end of the finger of a glove, is dipped in a strong solution of nitrate of silver, and then carried down into the throat, as far as the spasmodic state of the muscles which the attempt induces will permit, and pressed downwards against the superior surface of the larynx. I believe other stimulating applications are sometimes employed in the place of the nitrate of silver. Now, of this method of cure I do not know much, except by report. I have heard that many cases of chronic hoarseness and cough have speedily been cured by it." (Loc. cit., p. 810.)

We repeat, that these cases are not quoted to disparage Dr. Green's researches, but rather as confirmatory of them. The whole subject is undoubtedly his own, by the special attention he has devoted to it. We will now subjoin his method of applying the nitrate. The salt he uses is not the fused, but the crystallized.



"*Method of applying the solution.* In the treatment of laryngeal disease by the direct application of the nitrate of silver to the diseased surface, I have employed ordinarily a solution of this substance, of the strength of from two to four scruples of the nitrate to an ounce of distilled water. When, however, there are found extensive ulcerations of the epiglottis, or about the opening of the larynx—ulcerations which it is desirable to arrest at once—I have not hesitated to apply directly, to the diseased parts, a solution of double the strength of the last named. But one or two applications only of a medicine of this power should be made at one time; ordinarily, however extensive the lesions may be, it will not be necessary to employ a solution of greater strength than one composed of four scruples of the salt to an ounce of water. On the other hand, it has been found, that one of less strength than of from forty to fifty grains of the nitrate to an ounce of fluid will have but little effect upon a diseased mucous surface, where ulcerations exist

"In cases in which it becomes necessary to cauterize the interior of the laryngeal cavity, the aperture of the glottis should not be passed at once; the part should be *educated*, by applying the solution daily, for several days, to the faucial and pharyngeal region, to the epiglottis, and about the opening of the glottis. Proceeding in this

manner, that exquisite sensibility which belongs to the lips of the glottis, is in a good degree overcome, and the instrument may then be passed into the larynx, without producing half the amount of that irritation which its introduction below the epiglottis would have awakened at first.

"The instrument which I have always employed for making direct medicinal applications into the cavity of the larynx is one composed of whalebone, about ten inches in length (with or without the handle, as represented in the plate), curved at one end, to which is securely attached a small round piece of fine sponge. [The wood-cut in the margin (p. 500) is an exact fac-simile from Dr. Green's plate, and is the full size of the instrument.]

"The extent to which the rod is to be bent must be varied according to circumstances, for the opening of the glottis is situated much deeper in some throats than in others; but the curve which I have found suited to the greatest number of cases, is one which will form the arc of one quarter of a circle whose diameter is four inches. (See Woodcut, i.)

"The instrument being prepared, and the patient's mouth opened wide and his tongue depressed, the sponge is dipped into the solution to be applied, and, being carried over the top of the epiglottis, and on the laryngeal face of this cartilage, is suddenly pressed downwards and forwards, through the aperture of the glottis, into the laryngeal cavity. This operation is followed by a momentary spasm of the glottis, by which the fluid is discharged from the sponge, and is brought into immediate contact with the diseased surface.

"Every physician who has been present when this operation has been performed (and a large number have witnessed it from time to time) has manifested much surprise on observing how little irritation has been produced by the introduction of the sponge. If the patient, on opening his mouth, take a full inspiration, and then be directed to breathe gently out, at the moment in which the sponge is introduced, the irritation caused by the application will be much less than when this caution is not observed. The fact, indeed, has been fully established by repeated experiments, that the introduction into the larynx of a sponge, saturated with a solution of the crystals of nitrate of silver, of the strength of forty, fifty, or even sixty grains of the salt to the ounce of water, does not produce, ordinarily, as much disturbance as is caused by the accidental imbibition into this cavity of a few drops of tea, or even of pure water!

"In the topical treatment of the follicular disease, it will be found that all larynges cannot be entered with the same facility. Indeed, in some instances, where œdema of the epiglottis and of the arytenoid cartilages has existed, I have found it very difficult in making the first attempt to pass the sponge of the probang through the aperture of the glottis." (pp. 199-202.)

"Nothing will so speedily enlarge the opening of the glottis, when it has been contracted by œdema, as a few applications of the nitrate to the lips of the glottis and the laryngeal face of the epiglottis." (pp. 204-5.)

The use of the solution should be continued for some time: this is practically of great importance, as a relapse will otherwise soon take place. It should be applied at first every other day (and even oftener) for two or three weeks, and then twice a week until the surface assumes a healthy appearance. If the tubercles on the pharynx appear angry or confluent, the solid nitrate may be applied.

Dr. Green justly observes that the muciparous glands of the posterior nares are not unfrequently involved, and an unhealthy mucus is almost incessantly dropping down into the throat. The argentine solution may be applied by a small rod of whalebone, bent at the end into a right angle instead of into a curve.

There is one particular which Dr. Green has omitted to state in the above quotation, and that is, that a spatula must be made expressly for

the purpose: a spoon or an ordinary spatula will not serve. It should have a broad blade, 8 inches long and $1\frac{1}{4}$ broad, $4\frac{1}{2}$ inches should be curved at a right angle to the remaining portion, which is to be fixed into a handle 5 inches in length. The blade should be electrotyped with silver, as when made of naked steel its formidable appearance at first sight alarms the patient. Fixed in a handle perpendicular to the curve, the latter depresses the tongue while the hand of the operator is situate below the chin of the patient, and leaves ample room for the application of the solution. The point of the spatula should be carried well backward, so as to touch the root of the tongue, and thus enable the operator not only to depress the tongue, but also to drag it forward. It is by this means that the epiglottis can be brought into view in a very large majority of cases.

Concurrently with this topical application, other local means are necessary. Thus, when the uvula is elongated, partial or total excision is necessary. For this purpose Dr Green uses curved scissors and a pair of long and slender forceps having finely serrated blades. In hypertrophy of the tonsils, Dr. Green urges that excision is the only effectual means, if fibrinous deposit have already taken place. Even homœopathy, Dr. Green observes, seems powerless in these cases. The operation itself is almost painless.

"Hypertrophy and induration of the tonsils occur frequently in young persons and children, independently of follicular disease of the throat. In some instances, the affection appears to be congenital, or is hereditary; in others, it is the result of repeated attacks of chronic inflammation of the tonsillary glands. When the hypertrophy is accompanied by induration, whether this condition coexists with follicular disease, or is the effect of chronic tonsillitis, excision of the enlarged gland is almost the only method of treatment by which permanent and effectual relief can be obtained. This fact ought to be better understood by the profession than it seems to be, for the practice of painting these morbid growths with the tincture of iodine, or of cauterizing them with the solid nitrate, is still continued, and patients are daily being subjected to this annoying and useless practice, often month after month, with the apparent expectation on the part of their attendants that enlarged and indurated tonsils may be discussed by these applications!" (pp. 210-1.)

Dr. Green does not confine the use of the nitrate of silver to the treatment of chronic cases: he applied it to the fauces, pharynx, and larynx, of the strength of 45 grains to the ounce, in a case of acute laryngitis, in which suffocation was imminent. For a few minutes, the difficulty of breathing and the cough were increased by the application; a large amount of viscid ropy mucus was discharged; and along with it a small quantity of blood. In the course of half an hour after the application, the symptoms had improved. The laryngeal cough subsided, the respiration became less laborious, and the patient soon after obtained some sleep. In short, from this hour Dr. Green had no farther trouble from the case. Occasional doses of expectorant medicines were administered during the day. On the following evening, a slight increase of the cough and irritation came on, but these soon subsided, and were followed by a quiet night's rest.

In a case of ulceration of the epiglottis and opening of the larynx in a child, consequent on swallowing sulphuric acid, the remedy was used with the greatest success.

The application of the solution to the bronchi is also advocated and practised by Dr. Green.

"As stated in a foregoing chapter, follicular laryngeal disease is often found coexisting with chronic inflammation of the lining bronchial membrane. The presence of this complication does not contraindicate the topical employment of the nitrate of silver. On the other hand, in making the applications into the laryngeal cavity, a still more free use of the solution should be employed, in order that some part of the fluid may find its way into the bronchial divisions. Among the patients who have come under my care during the last eighteen months, are a number of intelligent physicians, who have been sufferers from laryngeal and bronchial disease. Several of them have informed me repeatedly that, after having a free application of the solution into the larynx, they have felt distinctly the fluid extending down the bronchial tubes. Often, in these cases, no taste of the medicine would be observed until matter, by coughing, was expectorated from the air-passages, when the peculiar flavour of the nitrate of silver—a most acrid bitter—would be perceived; and this would continue to be observed whenever the individual expectorated for many hours after the operation." (pp. 225-6.)

While advocating the argentine solution as an effective topical application, Dr. Green does not appear to have neglected counter-irritants to the spine and throat, and local bleedings. Blisters, leeches, &c., are amongst the remedies of this kind.

We have already referred to the utility and safety of the argentine solution, as a topical application in those cases in which laryngeal disease complicates phthisis pulmonalis. From the experience of twenty-five such cases treated by Dr. Green during the year 1845, he is enabled to speak in strong terms of its value in mitigating the sufferings and prolonging the lives of phthisical patients. The subjoined is a remarkable example of the value of the treatment: it is the case of a gentleman who went from Connecticut to New York:

"Some months before coming to the city he lost his voice entirely; but he was hoarse, and had had a cough, with laryngeal inflammation, several years before this occurrence. He had also had several attacks of hemoptysis; was emaciated, and so feeble when he arrived, that he was not able to walk the distance of a block without aid. He was suffering from a most severe paroxysmal cough; had great dyspnœa, and a free expectoration of purulent matter. The throat was studded with granulations or enlarged follicles, and it was with much difficulty that he could articulate above a whisper. A dullness on percussion over the right lung, with an absence of the respiratory murmur, and pain and stricture about the chest, with the above rational symptoms, marked the case as one of confirmed phthisis; and such it was admitted to be, after a critical exploration of the chest, by several experienced medical friends, among whom was one of the physicians of the New York Hospital, who is esteemed, and justly so, as a most accomplished auscultator.

"Applications of a concentrated solution of the nitrate were made to the throat and into the larynx of this patient for about two weeks. At the end of this time, his cough and dyspnœa were so much relieved, and his strength increased, that from not being able to walk any distance without aid, as was the case when he first arrived, he on the twelfth day of treatment went on foot down to the boat in which he came to the city and back again, a distance of more than three miles, without suffering any inconvenience. After remaining several weeks under treatment, he returned to Connecticut with augmented strength, and with his cough greatly relieved. Nearly three months after he left the city his sister called on me in New York, and informed me that her brother had been able after his return to superintend his affairs, which had not been the case for nearly two years before; and in this favorable condition he remained when last heard from, which was more than a year after he left New York." (pp. 231-3.)

General treatment of follicular disease. On looking over the details of the cases treated by Dr. Green, we find that the iodide of potassium holds the first rank. This is conjoined with the sesquioxide of iron, and various sedatives and expectorants, to alleviate the cough. Belladonna, conium, hyoscyamus, and the salts of morphia are among the former class; of the latter are the sanguinaria canadensis, or blood-root, hydrochlorate of ammonia, and the oil of Gualtherica. The sanguinaria is described by Dr. Green as a stimulating expectorant and slightly narcotic; and, when combined with opiates, a valuable remedy in allaying the cough and irritation, in some forms of follicular inflammation, complicated with bronchial or pulmonic disease. The tincture is made by macerating four ounces of the root in two pints of alcohol. From fifteen to twenty minims may be given thrice a day, with five or seven minims of laudanum, and a sixth of a minim of ol. gaulthericæ.

We observe that Dr. Green prescribes *one grain* of hydrochlorate of ammonia thrice a day, in combination with squill, digitalis, and opium. We are of opinion that the salt might just as well have been left out. The Germans ordinarily give not less than ten grains for a dose, and although we think this dose unnecessarily large, not less than from three to five grains should be taken.

Mercury in full, but generally in alterative, doses is recommended by Dr. Green. In those cases, he observes, where the general glandular system is involved in the morbid action, or even when some forms of pulmonic lesions coexist with the follicular disease, he has found the bichloride of mercury the most efficient, and, altogether, the best preparation of this mineral.

Exposure to a malarious atmosphere is recommended by Dr. Green in cases of phthisis pulmonalis, and he refers to our review of M. Baudin's publication, as well as to his own experience and that of others, for confirmation of his opinions as to the therapeutic efficacy of a malarious climate. He states no facts, however, that would lead the reader to suppose it could be of benefit in cases of *follicular* disease, nor does he recommend a residence in a marshy district for their cure.

Plates exhibiting the pathological anatomy of follicular disease, and the appearance of the throat in various stages and forms of the affection are appended. There is also a drawing of the instruments employed by the author for making medicinal applications into the laryngeal cavity, for excision of the enlarged tonsils, and for truncating the elongated uvula.

Having thus given an ample analysis of Dr. Green's work, it remains for us to propound briefly a critical estimate of its value. We think that the author has not only made a most valuable addition to practical or empirical medicine, but that the results of his method of treatment will lead to important changes in the prophylaxis and cure of pulmonary phthisis. It would appear from various testifying documents, which the author has collected together in an Appendix, that his statements as to the practicability and safety of topical medication in laryngeal disease was met by some of his countrymen by a sneering incredulity. There can be no doubt, however, that this part of the question is set entirely at rest; nor does the previous publication of the methods used by Bell, Vance, and Trousseau and Belloc, detract at all from the merit due to Dr. Green, for his persevering and successful attempts to render the treatment of some forms

of pulmonary disease more effectual and certain. We have adopted this method of treatment recommended by him, and can corroborate his statements as to its great value. Cases of pulmonary affection have in our hands been brought to a satisfactory termination, which we are quite sure, under the treatment ordinarily adopted, would have terminated fatally: and we remember individuals, whose cases terminated fatally, who (we feel equally certain) need not have died, at least of that disease which cut them off. This much is due to Dr. Green.

With regard to the influence of Dr. Green's views, on the prophylaxis and cure of pulmonary phthisis and chronic bronchitis, we would observe that their merit, in this respect, is that of being suggestive. It is of importance to know more precisely what agents determine the deposit of tubercular matter in the lungs; or when deposited, what agents call the latent morbific deposit into activity. The lungs and thorax may be considered as analogous to certain hollow viscera, as the bladder, rectum, uterus, &c. We know that in these there is a point of the mucous membrane at the entrance into the cavity which is endowed with special sensitive power, and the stimulation of which excites expulsive efforts. The laryngo-pharyngeal membrane is endowed with this peculiar sensibility, and as covering the inlet into the stomach and lungs is subservient to the expulsive efforts of retching, vomiting, and coughing; and these efforts must be considered as analogous to tenesmus and dysuria in dysentery and calculus vesicæ.

We find, however, that it is not the muscular apparatus only of these hollow organs that is implicated; the mucous membrane covering their interior is also affected when the sensitive portion of that tissue is irritated, and concurrently with hypertrophy of the muscular fibre there is increased secretion from the mucous membrane, presenting all the different grades of morbid change from simple mucus to a discharge having a purulent character. It becomes then of importance to investigate more accurately than has hitherto been done, the pathological relations of these sensitive inlets to the mucous surfaces with which they are in such intimate connexion, and more especially the pathological relations of the pharyngo-laryngeal membrane to the pulmonary structures, including both the external and internal. If upon inquiry it be found, that a morbid condition of the muciparous glands of that membrane, or of the membrane itself, be a frequent determining or exciting cause of tubercular deposit and of those various structural changes in the pulmonary mucous membrane, generalised under the term *chronic bronchitis*, the physiology and pathology of those glands well demand a close investigation. We are inclined to think that the condition of the abdominal viscera will be found to have an important influence on them. There is a close relation between the two extremities of the alimentary canal: this is shown in those cases of phthisis pulmonalis complicated with fistula in ano, and of which we do not remember an example that was not complicated with pharyngo-laryngeal disorganization. In cases of phthisis complicated with a gouty diathesis, and also with hepatic disease, we have frequently observed a similar complication; and our late experience leads us to think, that in such cases the symptoms might be much ameliorated, life much prolonged, and perhaps the more fatal pulmonary affection prevented by a timely attention to the state of the respiratory inlets.

The predisposing causes of pulmonary disease may also be eventually

better elucidated than hitherto. The practice of smoking tobacco, especially in the form of cigars, cannot fail to set up a morbid action in the pharynx and larynx, and so induce insidious chronic inflammation of the pulmonary membrane; and we can fully corroborate Dr. Green's statements on this head. The low degree of sensibility in the fauces generally, renders it necessary in all cases not to trust to the declaration of the smoker, that he feels no pain or uneasiness in the throat. The practitioner must inspect it, and in many examples he will find follicular disease in its first and second stages, although no complaint has been made by the patient. Nor should the quantity smoked be a guide to his judgment in determining the amount of mischief, as there is the widest difference between individuals in the amount of tolerance of the "weed;" a single cigar in one individual will produce as bad effects as half a dozen in another. We cannot, however, here enter more fully into the suggestions we have thrown out. The scientific reader will readily see that the etiological and pathological relations of the throat are very numerous, and that it is for him to work out practical results by daily observation.

We ought not to close this article, without suggesting to Dr. Green the propriety of correcting the proof-sheets of any future volume he may publish, for we think it is impossible he can have done that necessary duty on the present occasion. The grossest errors abound, in punctuation, in the spelling of professional terms, and in the latinity of the formulæ. The Latin of medical rescripts is already ridiculously *doggish*, even when correctly written; but what shall we say to such a confusion of genders and cases as in the following:

"R. Hydrarg. chlo. corrosiv. gr. xii. Solve in aq. distil. q. s. adde Micæ Panis Alba, Saccharis Alba āā q. s. ut fit pilulæ numero ccxl."

Or this:

R. Tinct. Sanguinaria ℥iss.
Tinct. Opii. ℥ss.
Ol. Gaultheria gtt. x.
Misce; cujus capiat guttas viginti vel triginti, ter die.

Our brethren in the United States are so closely allied to us by community of origin, of manners, and of language, that we have a family jealousy of any aberration from those minutiæ of composition, which render a very attractive subject the more attractive, and the absence of which diminishes the merits of the best book.

Art. XVI.

On Tumours of the Uterus and its Appendages. (*Jacksonian Prize Dissertation.*) By Thomas Stafford Lee, m.r.c.s.e., Fellow of the Medico-Chirurgical Society, &c. &c.—*London*, 1847. 8vo, pp. 262.

Mr. Lee divides the subject of his work into three divisions. Firstly, he considers tumours of the uterus; comprising tumours of the walls of the uterus, polypoid tumours, cauliflower excrescence of the uterus, and malignant tumours of that organ. 2. Tumours of the ovary; comprising cystic tumours of the ovary, and the malignant tumours of that organ. 3. Tumours of the vagina and external organs of generation.

By the term tumour of the uterus, Mr. Lee wishes to describe all those

morbid growths which, being at the same time attached to its substance, project into its cavity or from its walls, and divides them into the "benign" and malignant. The former (the non-malignant) he describes also under the term fibrous tumour.

The symptoms of fibrous tumour of the uterus are seldom referable to the viscus itself. There is irritability of the bladder and rectum, the urine cannot be long retained, or there sometimes may be entire retention; the fæces are flattened, and, from a weight in the pelvis, the patient is led to take advice. They are often extremely chronic, and will remain dormant for many years. Virgins are more liable to suffer from them than married women. Of 74 preparations in five museums of the metropolis, examined by Mr. Lee, in 18, or one fourth, the tumour occupied the fundus externally; in another fourth it was situate on the posterior wall; and in another fourth (19 cases) it was found distending the cavity. In four examples, the anterior wall of the uterus was the seat of the morbid growth; in four more the posterior part of the cervix; but in no instance was its anterior portion affected. They are of a cellulo-fibrous structure, and pass into the cartilaginous and osseous transformations. Sometimes the bone is so hard, as to be able to receive a fine polish. When the tumours project into the uterine cavity, and undergo this degeneration, they may become detached and expelled.

Fibrous tumours are insensible, and have but few blood-vessels; it is from the cellular cyst around them that the blood proceeds in those uterine hemorrhages which arise when the tumour protrudes into the cavity of the viscus.

Mr. Lee thus describes the symptoms of fibrous tumours of the uterus:

"Sometimes this disease may exist and only a little leucorrhœa be present, and the morbid product not ascertained until after death; at others, there is a sense of burning heat in the pelvis, running down the thighs, irritability and uneasiness about the rectum; the stomach is disordered, and it is said there is great venereal excitement. The mind becomes affected; there is melancholy, and all the symptoms of hysteria: very frequently there are expulsive pains—and, when present, are considered of great diagnostic value by M. Lisfranc. If you question further, you will find that the functions of the bladder are interfered with; and this perhaps was the first symptom which drew the attention to the womb: a hard tumour can be frequently felt above the pelvis, œdema of the legs is present, and there is a deep-seated weight in the pelvis." (pp. 11-2.)

The symptoms will vary according to the situation of the tumour:

"Those tumours which are placed under the peritoneal surface can be distinctly felt by the hand on external abdominal examination: they may be either smooth or lobulated, and generally, though not always, have a fixed character; but when they possess a pedicle, and it is long, they can be moved about the cavity of the abdomen like a diseased ovarium. If their growth is rapid, they are accompanied with indurations of the uterine tissue around their seat; you then have great pain on pressure, with symptoms of general disturbance, more or less fever, with quick pulse, thirst, and derangement of the digestive system. These symptoms are so frequent when the tumour occupies this position as almost to be pathognomonic of the disease. 2dly. If the tumours are placed in the posterior or lateral parts of the uterus, the early symptoms are more distressing; you have then great pain from pressure on the pelvic nerves, the veins in the abdomen become pressed upon, œdema and varicose veins show themselves, more particularly on one side, sometimes on both; piles are produced; the evacuations of

the rectum are interfered with, the fæces are flattened, and constipation, frequently the most obstinate and troublesome symptom, is present, and produces inconveniences we can hardly obviate. 3dly. If the tumours are placed in the anterior walls of the uterus, the functions of the bladder become more particularly implicated: the urine can be retained only for a short time; there is almost constant desire to make water, although only a small quantity is evacuated. At other times retention of urine occurs, which is relieved by change of posture or the catheter. Piles are a very frequent accompaniment of tumours of the uterus. 4thly. If attached to the fundus, it may cause retroversion of the womb (Clarke), and prevent the evacuation of fæces and urine at the same time. Dr. Lever relates a case of this sort, 'where the obstruction had been so great, that the bladder, ureters, and pelvis of the kidneys were enormously distended, while the bowels were loaded with masses of hard, feculent matter.'

"2. When the tumours are placed in the walls of the uterus these symptoms are much more obscure, until they obtain consideration from their bulk. In many instances, the only symptoms referable to the uterus are a weight in the seat of that organ, with occasionally a burning sensation, accompanied by an increased mucous discharge: this may remain for years without producing any further inconvenience, until some cause or other produces irritation; it then increases and produces all the symptoms of pressure on the neighbouring organs, which have been already referred to. When in this position, they may even become osseous without attracting attention.

"3. Tumours projecting into the cavity of the uterus are much more serious in their nature, and produce more immediate effects than those just described. Of these there are two varieties—1st, those which retain their character as tumours, and project into the cavity of the uterus; 2dly, those that lose that character, project so far as to obtain a peduncular attachment to the womb, and in fact become polypoid tumours. These latter, with their symptoms, will be noticed when speaking of polypi.

"When the tumour is submucous, and only projects into the cavity, and not so far as to become a polypoid tumour, the first symptoms usually observed are those connected with derangement of the function of menstruation: the patient finds that the periodical discharge is increased in its quantity, lasts longer than usual, and is followed by a more or less white discharge. This may or may not call attention to the uterus: but soon the symptoms are aggravated, the quantity of discharge is not only increased, but contains clots, a considerable portion of blood is lost at each period, and these occur more frequently, until at last the constitution begins to suffer." (pp. 12-14.)

Not only do hemorrhages take place at the menstrual period, when the tumour projects, but the patient is also harassed by expulsive pains. In addition to these general symptoms, there are those to be ascertained by manual investigation. By this it will be necessary to determine the condition of the os and cervix uteri, the weight, with the mobility or immobility of the uterus, and whether the uterus is connected to any mass previously felt above the pubis. Mr. Lee speaks highly of the uterine sound used by Dr. Simpson, of Edinburgh, in the diagnosis of uterine disease:

"This instrument has the appearance of a small male catheter, with a bulb at one extremity, and fixed in a handle at the other; it is marked by an elevation at $2\frac{1}{2}$ inches from the extremity, that being the natural length of the cavity of the healthy uterus, and is then graduated with inches, in order to ascertain the length of the cavity of a diseased one. When the instrument is passed to its full extent into a healthy uterus, you can elevate the fundus above the pubis, turn it from side to side, and also throw it down upon the rectum; these movements showing the healthiness of the organ: therefore, if they can be made while a tumour

exists in the abdomen, it proves that the uterus itself is unconnected with it. In speaking of this instrument, Professor Simpson says, 'But if a tumour is connected to the walls of the uterus, the sound will appear as if it passed into the mass, which will move with all the movements of the instrument, and the uterus itself will be found intimately attached to it; whereas, supposing the tumour unconnected with the uterus, we should be able to perceive that the uterus is moveable and free, and could be thrown down towards the rectum, and easily detached from the tumour.'" (p. 16.)

Mr. Lee points out the necessity and mode of distinguishing fibrous tumours from indurations depending upon an inflammatory state of the uterus, from pregnancy, from ovarian disease, from uterine polypi, and from tumours situate in or upon other viscera of the pelvis or abdomen. Perhaps the most important point in the diagnosis is the distinction between a morbid growth and pregnancy. Mr. Lee observes:

"In all cases of enlargement of the womb, the breasts are the first organs to sympathise with its development, and this is more especially seen in the young and those who have never borne children; so that whether the cause of the distension be the retention of the catamenia or tumour of the uterus, the mammæ become developed and enlarge to a certain extent, as if Nature were providing nutrition for the young. But in pregnancy the nipple and surrounding parts undergo certain changes which are peculiar to that state, and although not infallible, still are very conclusive. These changes consist of an œdematous appearance of the areola and nipple, and an enlargement of the follicles. Some or all of these changes, as far as my experience goes (and I have taken notes of more than one hundred patients), are always present; in two only of the hundred was their neither areola nor follicle, but the œdematous state of the nipple existed to indicate pregnancy. But some of these signs not only belong to pregnancy, but also to disease, and consequently ought to put us on our guard. I have noted carefully the appearances of the breasts in ten cases of fibrous tumours, and I have almost invariably found the following characters. That at the commencement of the disease, or when it had produced such irritation as to cause the patient to apply for medical advice, I have found that the breasts have, in the great majority of cases, been enlarged and tumid; that the areola has in eight cases out of ten been enlarged and darkened; that in the same number the follicles have been more or less numerous, in some they have been remarkably distinct; but that in only one case out of ten was there any moisture or œdema present, and this was a suspicious one, as the patient never came back after the first consultation. All these cases were under observation a considerable time, except the last, and therefore we are positive that in none pregnancy existed. In one case also, the patient obtained by squeezing the nipple a whitish fluid resembling milk.

"The absence of these signs about the nipple are of the greatest importance in our diagnosis; but other points also deserve notice. In pregnancy, the womb has a peculiar elastic, doughy feel, perfectly regular on its surface, and ovoid in its shape. The fœtal heart can be heard beating about the umbilicus, and the placental murmur is usually heard in the left iliac region. But in disease the tumour is irregular, and of stony hardness; no placental murmur nor fœtal heart is heard. The beatings of the aorta may be observed with distinctness, but it can be easily distinguished from the fœtal heart, as it is a single sound, and synchronous with the pulse. In tumours of the uterus the catamenia are present, and usually increased, whilst in pregnancy they are absent; and if any spurious discharge takes place it is scanty." (pp. 20-1.)

Treatment of fibrous tumours. In a great majority of cases they are quiescent, and require no notice; when, however, an inflammatory pro-

cess is set up around the morbid growth, local depletion and the antiphlogistic treatment in general are indicated. Mr. Lee recommends the application of leeches to the neck or body of the womb, and a hip-bath immediately after the leeches come away. He recommends as many as six to be applied once or twice a week.

"During the intervals of the leechings, mercury or iodine should be applied to the womb itself, either in its pure state or made more consistent with wax. The ointment used at the Red Lion Hospital for Women, is mixed with one part of the Ung. hyd. fort., one part of Cera flava, and one part of lard. This is rolled up in the form of a ball, and introduced into the vagina every night, as high up as possible, in order that it may envelope the os and cervix of the uterus; this remains for twenty-four hours, when it has generally disappeared; it may then be repeated.

"I can strongly recommend this plan of local depletion with the application of mercurial ointment; of course it requires judgment to adapt it to the various cases which come under notice, but in all cases where it is judiciously applied it improves the patient..... Dr. Rigby has carried this plan still further; not only does he envelope the os and cervix uteri with the strong mercury ointment, but he dissolves the ointment, and after having drawn it up into a catheter, injects it into the cavity of the womb. He tells me that this is a great additional advantage, and that he has some cases under his care where, in one case in particular, the tumour which formerly was one large, hard, solid mass, is now separating into distinct parts and becoming gradually less." (pp. 25-6)

Iodine is, of course, one of the remedies recommended by Mr. Lee, following in the wake of Dr. Ashwell and Dr. Walshe. In very many cases, nothing can be done except to palliate the irritation; if pregnancy take place, the danger is great, and the induction of premature labour becomes necessary, as recommended by Dr. Ashwell.

The second chapter is devoted to the consideration of polypi of the uterus; Mr. Lee discusses them as polypi and polypoid tumours. These do not differ in structure from the preceding class of tumours. The most dangerous symptom connected with them is the profuse hemorrhage to which even small polypi will give rise. Mr. Lee raises the question whence the blood proceeds, and concludes that the bleeding may be attributed to the very vascular state of the mucous membrane at the insertion of the polypus into the uterus; that the veins of the part are the principal sources of bleeding; and when the mucous membrane is absorbed the vascular network which envelops these growths may add materially to the result. Even when the mucous membrane is uninjured, this envelope may materially increase its vascularity.

The soft polypi of the uterus, or the polypi proper, are divided into six species, comprising—1, vesicular polypi; 2, polypi from the enlargement of the nabothian glands; 3, fibro-cellular polypi; 4, cellulo-vascular; 5, mucous polypi; 6, the channelled polypi of the cervix. The symptoms marking these morbid changes closely resemble those which characterise the polypoid tumours, but Mr. Lee remarks, that the white mucous discharges are greater, while the expulsive pains are less, than in polypoid tumours; hemorrhages are very frequent in these polypi, and is the principal symptom to which we ought to direct our attention. Its source is not only from the plexiform arrangement of the veins of the mucous membrane at the base of the tumour, as we find to be the case in

the former disease, but also from their own proper vessels, principally veins. The treatment consists in their removal either by excision or ligature, as medicines are useless. In the first instance, however, it will be necessary to repress the profuse hemorrhage to which they give rise, and which indeed will follow excision. Some excellent practical remarks illustrate the chirurgical treatment of polypi.

The cauliflower excrescence of the os uteri comes next under discussion. The following is the result of Mr. Lee's microscopic investigations into its composition in a case which came under his notice:

"Under the microscope these lobules were found to be covered individually by epithelial scales resembling those of the mucous membrane; and each was composed of nucleated cells, with here and there a blood-vessel ramifying on it, but the tumour was not apparently vascular. The edge of the lobules, with epithelial scales, appeared as if impacted one upon another; beneath which, from its circumference, where the cells were much compressed to its centre, cells became gradually developed. There was no appearance of fibrous tissue, nor any of the caudate cells indicating cancer. This then was the result of a careful examination of a part of this tumour removed during life by Dr. Richard Quain and myself. The following is a description of a portion examined in the same way after death: when a piece of the tumour, the only remains of which were in small detached clusters, was taken and placed in water, it appeared to be made up of a number of villi, apparently attached to a central substance of more firm consistence. It was composed of nucleated cells of large size, some circular, some oval, and others elongated oval; these contained a quantity of granular matter and a well-defined nucleus, which appeared to contain a cavity filled with a quantity of granular matter. The two together had the appearance of a cell within a cell, or a compound cell. These cells were connected by fine filaments like cellular filaments. From this examination we conclude that the tumour is composed entirely of cells, and that these are covered by an epithelial membrane; also that it was of simple structure and not malignant." (p. 84.)

It is well known that, in cases of this kind, there has been observed an immense discharge of a watery fluid. Mr. Lee investigated this also:

"It was of a brownish colour, tenacious to the touch, and of a faint odour; it had the appearance, when in large quantities, of saliva coloured. Under the microscope, we found that it was composed of an immense number of nucleated cells, principally of an elongated oval form, containing some granular matter, and each cell was provided with a distinct nucleus; a quantity of granular matter was seen floating in all directions in a thin fluid, which contained a number of epithelial scales. These appearances go far to establish the opinion proposed by Dr. Anderson, that the discharge is dependent on the effusion of cells from the blood-vessels, and thus its great exhausting power is explained." (p. 87.)

Nothing is added by our author to the pathology or treatment of encephaloid, or scirrhous tumours of the uterus.

The second part of Mr. Lee's work treats of diseases of the ovaries; and the first chapter is devoted to ovarian dropsy. In our review of Dr. Clay's pamphlet, (vol. XVI, p. 587) we occupied a considerable space with a consideration of the capital operation of ovariotomy. Four years have brought with them additional experience, and in this chapter of Mr. Lee's we have the whole subject of ovarian disease brought under review, and the treatment by extirpation of the sac, as well as by other means, fully investigated.

Mr. Lee collected 140 cases of ovarian dropsy, and placed their characteristics in a tabular form; some he observed for himself, the details of others are from books.

As regards the effect of marriage, it appears that of 136 examples, 88 were married, and 11 were widows, so that only 27·2 per cent. were single. As regards age, only 3 were those of females under twenty years; but between twenty and forty, there were 82 examples, 26 between forty and fifty, and 19 between fifty and sixty. Mr. Lee has omitted to state what ratio these numbers bear to the number of females living at the respective ages, an important point in estimating the *influence* of age.

With regard to other causes, Mr. Lee deduces from a rather meagre table, that the most frequent is parturition, that the sudden suppression of the menses is next in frequency, the excitement of marriage follows as a third, and he believes that disappointed affection is one of the most fertile causes in those that are unmarried.

The right ovary is more usually affected than the left, for of 93 cases, while only in 8, or 8·6 per cent. both ovaries were diseased, in 50, or 58·8 per cent. of the remainder, the right was affected exclusively.

Mr. Lee considers the pathology of ovarian dropsy under the following heads: 1. The simple cyst attached to the ovary or broad ligaments of the uterus. 2. Enlargement of the graafian vesicles. 3. Cysts unconnected with the ovary, and which are formed in various parts of the abdomen, usually mistaken for ovarian dropsy. And lastly, 4, multilocular cysts. We shall not follow him through the pathological anatomy of these various structures, nor through his consideration of their contents, and their diagnosis, but pass at once to their treatment.

In the early stages, when the enlargement of the ovary arises from inflammatory action, the enlarged organ can be felt distinctly between the vagina and rectum, as a tumour very painful on pressure. Under these circumstances, the antiphlogistic treatment is indicated:

"Local bloodletting is very important; this is to be effected, not as it is usually prescribed, viz., by cupping the loins, or by application of leeches to the vulva, but by the actual application of leeches to the tumour itself through the rectum. The bowel is to be washed out by a copious enema; then, by placing the leeches in a long tube, the upper end of which is perforated by a number of holes, the mucous membrane bulges through these perforations on introducing it into the bowel, and the leeches usually fix themselves to it, as is necessary in introducing this instrument; and when it arrives at the diseased ovary it produces pain, and ought not to be pushed further. Large quantities of blood may be taken in this way, and the application ought to be repeated every fourth day. This local depletion is to be further assisted by calomel, and opium, and salines Blisters and leeches ought to be applied to the tumour, when above the pubis, when pain and uneasiness are felt." (p. 155.)

When the tumour has so far increased as to occupy the cavity of the abdomen, the antiphlogistic treatment may still be useful, combined with counter-irritation in various ways. Iodine is of course mentioned as a powerful remedy, and its beneficial influence seems to be due to the suppuration of the cyst which follows its use. This result cannot be attained, however, without dangerous constitutional disturbance. Moderate bandaging, &c., is not estimated highly by Mr. Lee; and it appears, from his inquiries, that some of the cases published by Mr. Isaac B. Brown as being success-

fully treated by his method (one principal point of which is the careful application of a tight flannel bandage) were not so successful as his statements would imply; that, in fact, his conclusions were somewhat premature.

The operation of tapping is fully considered by Mr. Lee, and is shown statistically to be an operation much more dangerous than is generally supposed. After mentioning minor causes of this, Mr. Lee observes:

"The greatest danger to be apprehended after tapping, is the inflammation of the cyst itself, or sometimes of the peritoneum of the abdomen. This is almost the inevitable termination, some time or other, of the lives of the patients who are subjected to tapping; according to my experience, the cyst itself is the part most usually inflamed. In some cases a portion of the ovarian fluid escapes, and acts as a foreign body on the peritoneum; in others, the internal lining of the cyst takes upon itself an inflammatory action, or the trocar may have punctured a mass of cysts, and thus produced inflammation. The effects of the inflammation, however produced, are alarming; all the symptoms of active fever are found; there is great pain in the abdomen, which becomes tense and very tympanitic, vomiting ensues, and rapid exhaustion takes place, followed by death."

Mr. Lee has collected a number of cases where the duration of the disease, after the first tapping, was accurately recorded, and finds that more than one half of those who died did so within four months, and nearly half of these were only tapped once: almost all the deaths after the first operation were attributed to the inflammation of the sac or peritoneum.

Combining the results of his own tabulation and of Mr. Southam's, it would appear that the mortality after tapping is much greater than is generally believed: 24 out of 57 deaths died after the *first* tapping, and within eight months after the operation; 20 of the 24 died within one month, and 12 of these 20 within one week. From these and other observations, Mr. Lee concludes that, taken at its best, tapping is a very dangerous means of palliating ovarian dropsy; that when it is had recourse to, it will have to be frequently repeated; that the relief afforded between each operation will become gradually less, and the dangers consequently greater. The operation ought only to be performed under one of two circumstances: either early, when the cyst is unilocular, or when the ovarian tumour is producing fatal pressure upon vital organs. In no case, except under the latter circumstances, ought a multilocular cyst to be punctured, because the relief given is so trifling, and the dangers of tapping are so much increased in this form of the disease.

The radical cure by excision of the sac is elaborately considered by Mr. Lee. Previously to ascertaining the results of the operation, he undertakes the task of determining the usual course of the ovarian disease, and the results of the ordinary treatment. The dangers of the operation of tapping have been stated already. Under the ordinary treatment, the duration of the disease was found to be as follows: 26 per cent. lived one year; 19 per cent. two years; 13 per cent. three years; 8 per cent. four years; 2·3 per cent. five years. In fact, half the cases terminate by death in two years. Mr. Lee has tabulated 118 cases treated by excision of the sac (in the Appendix a valuable and elaborate table is given of all the known operations from 1809 to 1846, comprising the leading points in each case), and discusses the various details of the operation with great acumen and

judgment. We subjoin his general conclusions, premising that of 114 operations 40 died, being a mortality of 1 in 3.

"That of these 114 operations, in 24, or rather less than one in five, the operation was obliged to be abandoned, either from extent of adhesions, from the tumour being an uterine or omental one, or from there being no tumour at all; proving most indisputably the difficulties of the diagnosis.

"That in the 90 cases where the tumour was removed, nearly one died to three recoveries.

"That the diagnosis of ovarian tumours is very obscure as regards adhesions, and the character of the tumour; that adhesions existed in 46 of the 81 where the fact is mentioned, and in 6 there was no tumour.

"That where adhesions existed, the mortality was greater, being one death in $2\frac{1}{2}$, whereas, the mortality was one in three where they were absent.

"That the disease may be complicated with organic disease of other viscera.

"That the principal recorded causes of death, where it took place soon after the operation, are hemorrhage and peritonitis; but the cases are much too few to be depended upon.

"When death takes place in consequence of the operation, it is very rapid. Of 30 patients, where the time is mentioned, 14 died within 36 hours, and 25 within a week.

"That the character of the disease is of importance with regard to its mortality. In the extraction of hard tumours of the ovary, the mortality was more than 1 in 2. Of the 16, 9 were cured, 7 died, and in 5 the tumour was not removed. Whereas, where the tumour was composed partly of fluid and partly of solid matter, viz. in 65 cases, 44 were cured, 21 died, and in 14 the tumour was not extracted, making the mortality less than one in three: so that encysted dropsy is much more favorable to the operation than hard tumours of that organ." (p. 210.)

Mr. Lee thinks that, in the majority of cases, the operation of ovariotomy is "decidedly unjustifiable;" but that in others it "is *very* justifiable," namely, in those cases of encysted tumour which have enlarged to such an extent as to demand active interference, or when an unilocular cyst is becoming multilocular. In such examples, with a correct diagnosis, general good health, and freedom of the tumour from adhesions, the operation ought to be performed.

Malignant tumour of the ovary, tumours of the vagina and of the external organs of generation are then considered. The latter class of organs can scarcely be considered as "appendages to the uterus," and are only admissible as rendering the work a complete monograph on tumours of the sexual organs of the female. As such we estimate it; and we think it deserves a place in the library of the practitioner, both as a practical treatise and as a work of reference. It is in truth as valuable for the results of thoughtful reading contained in its pages, as for new researches. We have often thought that a very complete series of monographs might be written, if the results of observation and treatment, scattered through our vast periodical literature in the form of "cases," were classified so as to afford general deductions. We ought to add, that original "cases" illustrate several of the diseases treated of in the volume before us.

Art. XVII.

Proceedings of the World's Temperance Convention, held in London, August 4th, 1846, and following Days. With the Papers laid before the Convention, Letters read, Statistics, and General Information presented, &c. &c.—London, 1846. 8vo, pp. 140.

There are many reasons why we deem it incumbent upon our brethren of the medical profession to take an active part in the investigation which is now being carried on by a large and not unimportant section of the public in this country and elsewhere, with regard to the effects of the habitual use of alcoholic drinks, and the possibility of effectually maintaining the "mens sana in corpore sano" without recourse to them. The fearful array of social and individual evils which may be traced to the *abuse* of fermented liquors, should lead every reflecting mind to consider how far the *use* of them is desirable or necessary; and this inquiry is peculiarly incumbent upon those who assume to themselves the right of guiding the public in all that concerns the welfare of the bodily fabric, whether in health or disease. Their influence for good or evil in this matter can scarcely be too highly estimated. If they are able, after careful consideration of the evidence on each side, to give their sanction to the statements of the advocates of the Total Abstinence cause, that sanction ought not to be withheld; since its weight in the scale of social order and morality demands the open and unqualified expression of it, unrestrained by any fear of ridicule or loss of the world's approval. That they would *knowingly* place their influence in the opposite scale, cannot for a moment be admitted; but there is too much reason to fear that, either from actual ignorance of what the experience of multitudes of all ranks and conditions has now demonstrated, or from a natural tendency to persistence in that sort of *laissez-faire* system which it is so easy to practise and (in this matter especially) so agreeable to their patients, the generality of medical men are at present lending their sanction to a system of most pernicious error. Having long since made up our own minds on this subject, we have determined not to forego this opportunity—the last in our power—of recording our earnest convictions in regard to it; in the hope of leading our readers, if not at once to view the matter in the light in which *we* see it after many years of observation and personal experience, at any rate to inquire and observe for themselves, and to pause before they again recommend or sanction practices which, though comparatively innocent in themselves, aid in perpetuating the direst evils with which our country is infected.

There are, it must be admitted, few cases in which the wish so readily becomes the father to the thought, or in which the feelings are so apt to bias the judgment, as in the consideration of the real utility of fermented liquors. The prevalent ideas of social enjoyment and good fellowship are so intimately associated with the circulation of the bottle or the discussion of a bowl of punch,—most of us have such vivid recollections of the thirst and fatigue of travel alleviated by a glass of good ale at a road-side inn,—

and many a medical man's notions of quiet and legitimate enjoyment after a trying day of professional labour are so closely connected with a comforting tumbler of toddy or brandy-and-water by his fireside,—that it is commonly thought impossible to imagine that the animal spirits can be exuberant without the excitement derived from alcohol, or that the wearied body and mind can derive their needed refreshment from beverages so poor as tea, coffee, or cocoa. When we are regulating the diet of a patient, moreover, how difficult it is to prescribe a rigid abstinence to those who earnestly petition for *only* a glass or two of wine, or a tumbler of beer, per day, as a necessary means of sustaining their fainting strength, or of imparting to them (and this is perhaps the most insidious plea of all) the power of digesting their proper food. And how pleasant it is to preserve the confidence of our patient by thus chiming-in with his humour, rather than by rashly propounding what he may regard as unreasonable crotchets to excite his doubts as to our own sanity. The medical profession in this country, however, is beginning to be awakened from this pleasant *insouciance* by the pressure from without; and to find it necessary to place itself in the midst of the current of human progress, which might otherwise sweep past it and leave its *dicta* among the despised relics of an immoveable conservatism. Some hundreds of medical men of all grades and degrees, in every part of the British empire, from the court physicians and leading metropolitan surgeons who are conversant with the wants of the upper ranks of society, to the humble country practitioner who is familiar with the requirements of the artizan in his workshop and the labourer in the field, have given their sanction (as we shall presently see) to the statement that the maintenance of health is perfectly compatible with entire abstinence from fermented liquors; and that such abstinence, if general, would incalculably promote the improvement of the social condition of mankind. The medical adviser may now shelter himself, therefore, under this high authority; and need no longer be considered a madman, or even an enthusiast, for denying what it has been supposed that the common sense of mankind unmistakeably teaches. The difficulty, however, is to carry this doctrine into practice; and nothing but such a degree of moral courage as can rise superior to temporary ridicule can give success. But our profession is surely one of the last in which that moral courage should be found wanting; for the demands upon it are varied and continual. And in this particular case, it may be remarked, the difficulty is constantly lessening, from the spread of more correct information on the subject; and we have, in fact, known instances in which medical men have *lost* credit with their patients, through urging upon them as necessary those stimulants which their own convictions told them that they were better without.

We need not descant at any length upon the evils of intemperance. The experience of every medical practitioner must have brought its terrible results frequently before his eyes. But whilst thus familiar with its consequences as regards *individuals*, few but those who have expressly inquired into the subject have any idea of the extent of the *social* evils resulting from it, or of the degree in which they press upon every member of the community. On this account we shall preface our inquiry with a few passages from a short paper in the pamphlet whose title we have

placed at the head of the present article. This paper, entitled "Intemperance the Great Cause of Crime," consists almost entirely of extracts from recent public addresses of our judges, and from the written statements of magistrates, gaolers, and police-superintendents, whose position furnished them with the means of gaining the fullest information on the subject. The whole of it is pregnant with the deepest and most fearful meaning; and nothing but our limited space prevents us from placing it before our readers in its unabridged condition. We beg their earnest consideration of the following statements.

"Judge Wightman stated in his address to the grand jury at Liverpool, in August 1846, that 'he found from a perusal of the depositions, that one unfailing cause of *four fifths* of these crimes was, as it was in every other calendar, the besetting sin of drunkenness.'

"Judge Alderson, when addressing the grand jury in 1844, at the York assizes, said—'Another thing he would advert to was, that a great proportion of the crimes to be brought forward for their consideration arose from the vice of drunkenness alone; indeed, if they took away from the calendar all those cases with which drunkenness has any connexion, they would make the large calendar a very small one.'

"Judge Erskine declared at the Salisbury assizes in 1844, when sentencing a *gentleman* to six months' hard labour, for a crime committed through strong drink, that ninety-nine cases out of every hundred were from the same cause. Judge Coleridge likewise stated at the Oxford assizes, that he never knew a case brought before him that was not directly or indirectly connected with intoxicating liquors. And Judge Patteson at the Norwich assizes said to the grand jury, 'If it were not for this drinking, you and I would have nothing to do.' One of the judges stated some time ago at the circuit-court in Glasgow, that 'more than eighty criminals had been tried and sentenced to punishment; and that, with scarcely a single exception, the *whole* of the crimes had been committed under the influence of intoxicating liquors. From the evidence that appeared before him as a judge, it seemed that every evil in Glasgow began and ended in whiskey.'" (Proceedings, p. 123.)

So that, according to the testimony of witnesses whose competency and truthfulness no one can call in question, *four fifths* of the entire amount of crimes is the *very least proportion* we can assign to those which are committed under the direct or indirect influence of intoxicating liquors. Let us now call witnesses of another but not less unimpeachable class,—the chaplains of gaols.

"In a late Report of the prisons of Glasgow, an account is given of 3907 individuals, most of whom were committed for crimes respecting which sentence of transportation might be awarded; and, respecting these the Rev. George Scott, chaplain, thus writes: 'Though a number of causes are specified, drunkenness is the most prolific source of most of the crimes in Glasgow. Of the many thousands annually imprisoned, I think it would not be possible to find one hundred sober criminals in any one year. Even the youngest learn this ruinous vice, and when they live by stealing, swallow astonishing quantities of whiskey.' The accuracy of Mr. Scott's observations is corroborated by the new chaplain, in his Report of Glasgow Prisons for 1845. 'To the ruinous habit of drunkenness,' says he, 'may be traced either directly or indirectly the offences of *at least three fourths* of those that come to prison, *females* as well as males. Of this I am convinced, even from their own statements as well as from other circumstances.'" (p. 125.)

The chaplain of the Stirling prison states: "So far as my experience

has at present gone, I think that drunkenness is the main cause of crime;" and the Rev. John Clay, the experienced and devoted chaplain of the North Lancashire gaol at Preston, gives similar testimony. "Persons," he says, "who in hard times are led into criminality by destitution, are in better times led into it by drunkenness." To the same effect is the evidence of Mr. J. Smith, governor of the Edinburgh prison. The number of commitments for disorderly conduct arising out of drunkenness, during the year ending June 1844, was 3325; and of those for other offences, the number during the same period was 2385. "I do not hesitate to say," adds Mr. Smith, "that it is my firm belief that, but for drunkenness and the evil and ruinous consequences which follow in its train, there would not have been one fifth part of the number of commitments during the period." The following is Mr. Logan's general summary of similar information obtained from other quarters:

"We collected the following information in July 1844, when visiting prisons in the west and south of Scotland; and the reader will bear in mind that the majority had been committed for theft, and several were about to be removed to our penal colonies. At Greenock, the governor stated that out of 461 prisoners, 297 might be said to have committed their crimes under the influence of drink. At Kilmarnock, Captain Blane believed that he was under the mark, in stating that *four fifths* of the crime there was caused by intoxicating liquors. In Dumfries, the governor was 'warranted in stating that nineteen out of every twenty brought before him were so in consequence of drinking;' and when conversing with *thirty* prisoners out of the total number (forty-two) *twenty-nine* acknowledged that strong drink had been the cause of their imprisonment; and the sitting magistrate stated to the clerk of the police-court, that very morning, that were it not for intemperance, the premises might be shut up for ever. At Ayr, the governor 'had no hesitation in saying that thirty-nine cases out of forty were the fruits of intemperance,' and added, 'if you think proper to visit the prisoners you will find that my statement is pretty correct;' well, we visited each cell, and conversed with every unfortunate inmate; and out of *seventy-three* prisoners there, no less than *seventy* acknowledged that had it not been for these accursed drinking customs, they never would have occupied the lonely cell of a prison. Similar statements were made to us when visiting the prisons of Paisley, Stirling, Hamilton, Dumbarton, Airdrie, and Kircudbright; and what is true of Scotland is to a very great extent the same in England and Ireland..... These facts have all been fully corroborated by the testimony of the respective governors of Millbank Penitentiary and Newgate, London; Wakefield House of Correction; Manchester New Bailey; Newgate and the Female Prison, Dublin; and having visited these prisons and conversed with criminals in each of them (with the exception of Millbank, where it is not allowed), we found that their statements respecting the cause of crime were quite in keeping with those referred to in Scotland." (p. 126.)

We need scarcely, we think, adduce any more evidence in proof of the position that *intemperance is the chief cause of crime.* How fearful then is the responsibility of those who by any means, direct or indirect, encourage this baneful propensity!

To show the dreadful extent to which intemperance prevails, we shall quote from the preceding paper, and from an essay in the same publication, on the Statistics of Temperance and Intemperance, by Mr. T. Beggs, some facts relative to the present condition of the city of Glasgow. On the authority of Sheriff Alison it is stated that in the year 1840 there

were in Glasgow, amongst about 30,000 inhabited houses, no fewer than 3010 appropriated to the sale of intoxicating drinks; every tenth house being devoted to the sale of spirits—a proportion unexampled, it is believed, in any other part of the globe. The same gentleman declares that he believes that 30,000 persons, or one tenth of the whole population, go to bed drunk every Saturday night. It appears from an inspection of the registers of the police-station, that not fewer than 25,000 commitments are annually made, on account of drunkenness and disorderly conduct in the streets; and these commitments include no fewer than 10,000 females. A large proportion of the parties so committed are discharged early in the morning and are not brought before the police magistrate, not above a quarter of them being entered upon the records of his office. The annual consumption of ardent spirits in Glasgow is estimated by Sheriff Alison at six gallons per head; making an aggregate of nearly 1,800,000 gallons yearly. The value of this, at the retail price of 15*s.* per gallon is £1,350,000. Now what is the consequence of this as to the health and social condition of the city? "Glasgow," says Dr. Cowan, in his 'Vital Statistics of Glasgow,' "exhibits a frightful state of mortality, unequalled perhaps by any city in Great Britain. The prevalence of fever presents obstacles to the promotion of social improvement among the lower classes, and is productive of an amount of human misery credible to those only who have witnessed it." The returns furnished by Dr. Davidson from the Glasgow Fever Hospital enable us to form some estimate of the influence of intemperance in keeping up this fever, and in aggravating its rate of mortality. Of 249 males admitted during the year ending November 1, 1839, just one half are recorded as "temperate," that is, as never indulging in strong drink to the extent of inebriety; whilst of the remainder, 51 are classed as "a little intemperate," that is, as now and then drinking to intoxication; whilst 73 were "habitually intemperate," drinking ardent spirits whenever they could get them. Of 164 females, 76 or less than half were "temperate," 8 "a little intemperate," and 80 "habitually intemperate." It is evident from these data, that the cases of fever amongst the intemperate part of the working classes must bear a much larger proportion to their number, than among the comparatively sober who constitute (it is to be hoped) the bulk of the community; and that upon the former, therefore, the maintenance and propagation of the disease chiefly depends. The result is still more striking, when the rates of mortality are examined in these three classes respectively. Out of the 201 temperate patients only 28 died, or 1 in 7·2; whilst out of the 212 more or less intemperate, the number that died was 47 or 1 in 4·5. In Dr. Craigie's table of the deaths in 31 cases of fever that occurred in the Edinburgh Royal Infirmary, there were stated to be 15 irregular or dissipated, and only 2 regular; the habits of the remaining 14 are not stated, but they were probably of the "little intemperate" class. This comparison is made, be it remembered, not between drinkers and abstainers, but between different classes of drinkers. The expense in which the city of Glasgow is involved by the fever is estimated at £46,000 per annum; a sum enormous in itself, but like a drop in a bucket compared with that which is squandered in the purchase of ardent spirits.

We shall not pursue this painful inquiry any further; having said enough, we trust, to demonstrate the importance of the subject, to which we would now invite the serious and candid attention of our readers.

A brief historical notice of the origin and progress of the Total Abstinence Movement may not be without its value, in showing how far experience has replied by anticipation to some of the objections which would occur to almost every one who has not given his express attention to the inquiry. It is well known that individuals have risen up from time to time, in all ages, to proclaim the virtues of *aqua pura* as the beverage most conducive to health of body and vigour of mind; and our readers need scarcely be reminded of the cases of Cornaro and Benjamin Franklin, were it not for the remarkable degree in which the strong practical sense of the latter anticipated the conclusions more recently drawn from scientific investigation. "On my entrance into a London printing-house," says Franklin in his 'Autobiography,' "I worked at first as a pressman, conceiving that I had need of bodily exercise to which I had been accustomed in America, where the printers work alternately as compositors and at the press. I drank nothing but water. The other workmen, to the number of about fifty, were great drinkers of beer. I carried occasionally a large form of letters in each hand up and down stairs, while the rest employed both hands to carry one. They were surprised to see, by this and many other examples, that the 'American aquatic,' as they used to call me, was stronger than those who drank porter. My fellow-pressman drank every day a pint with bread and cheese for breakfast, one between breakfast and dinner, one at dinner, one again about six o'clock in the afternoon, and another after he had finished his day's work. This custom appeared to me abominable; but he had need, he said, of all this beer, in order to acquire strength for his work. I endeavoured to convince him that the bodily strength furnished by the beer could only be in proportion to the solid part of the barley dissolved in the water, of which the beer was composed,—that there was a larger quantity of flour in a penny loaf,—and that consequently if he ate this loaf, and drank a pint of water with it, he would derive more strength from it than from a pint of beer." The pious and enthusiastic Wesley, and the ingenious and benevolent Dr. Beddoes, devoted no small amount of labour and reasoning to an attempt to awaken the public mind to the injurious effects of the prevalent use, moderate and immoderate, of fermented liquors; and the latter, amongst other tracts on the subject, published an excellent one in 1808, entitled 'Good Advice for the Husbandman in Harvest,' from which we shall presently make an extract.

Besides these well-known examples, there have always been many who have practised, in a quiet, unostentatious manner, an habitual abstinence from all fermented liquors; and amongst these might be named some who have been remarkable for the amount of mental and bodily exertion which they have been able to sustain. Still it has been the current opinion, sanctioned by the general voice and the usual practice of the medical profession, that the *moderate* employment of fermented liquors of good quality is beneficial, or at any rate innocuous in a great majority of instances; and that where the demands upon the bodily strength are peculiarly constant and severe, efficient aid in meeting them is derived

from their use. The cases in which the contrary result has been apparent have been set down as idiosyncrasies, which can afford no rule for general guidance; and those who have ventured to oppose the public *dictum* on this subject have been usually considered at the best as amiable enthusiasts, whose principles, though true as regarded themselves, are not at all adapted for popular practice. But without the guidance of science, and against rather than with the authority of doctors, *the people* have begun to find out for themselves that those well-meaning but impracticable enthusiasts really spoke the truth; and that what has been commonly regarded as universal experience on this matter is nothing better than "a mockery, a delusion, and a snare," having no better foundation than the notion which prevailed until the beginning of the last century, that foul air was beneficial to the sick.

The following is a brief sketch of the origin and progress of the great Temperance Movement.

Association for the purpose of promoting temperance was first conceived and put into execution in the United States of North America, where the inhabitants are far advanced in the knowledge that "union is power," and in the practical skill necessary for applying coalition to a variety of purposes both political and philanthropical. Combinations against drunkenness have existed in America from a very early period. At first they merely insisted on general moderation in the use of strong drinks. Afterwards their rules and regulations became more stringent; and abstinence from ardent spirits at least formed part of the confederate agreement and pledge. About 1815 or 1820, regular societies were formed on this basis in America, and began to extend themselves widely. The method of abstinence from everything intoxicative was at a later period introduced there from Great Britain.

Early in the year 1828 Mr. John Dunlop (a name worthy to be placed by that of Father Mathew, among the benefactors of mankind) began to agitate the subject in Scotland, and took various plans of doing so; such as collecting statistics and proofs of national intemperance; demonstrating the good effects which had followed association against inebriation in America; exposing the evils of the system of compulsory drinking, an usage peculiar to this country; also by travelling about and conversing with influential individuals and philanthropists throughout the country; writing in newspapers, composing and disseminating tracts, lecturing publicly in large towns, &c. Notwithstanding Mr. Dunlop's enthusiastic and energetic proceedings, he did not succeed in gaining any regular disciples to the cause till about the latter end of 1829 and beginning of 1830, at which period the Greenock, Glasgow, Edinburgh, Dundee, Paisley, and other societies were instituted in Scotland; the Bradford Society in Yorkshire, by Mr. Forbes; and the Old British and Foreign Temperance Society in London, by Mr. Collins of Glasgow. Mr. Dunlop had proposed that all wines should be abandoned as well as spirits, but only partially succeeded in that point.

In the summer of 1829 Dr. Edgar of Belfast, without knowledge of Mr. Dunlop's proceedings, set on foot the Irish temperance movement, assisted by Mr. George Carr; and it is believed that the first European temperance society was established by the latter at New Ross about June

or July 1829; Mr. Dunlop's first societies not having been established till October of that year. Thus the Irish and British movements were separate and independent in their commencements.

Things continued on the original footing for some years, with only in general a pledge against the use of ardent spirits. But it soon became evident to reflecting persons engaged in the cause, that half measures would not suit the urgency of the case, and that a prodigious and nearly universal national evil must be met with correspondent strength of remedy. It was remarked, too, that all drunkards who were really reformed by joining the societies, not merely conformed to the actual rules, but uniformly abstained from using anything intoxicative. Many individuals connected with the societies accordingly made it their practice to abstain totally, and in 1832 it is believed that the Paisley institution made total abstinence a part of their regulations.

Teetotalism, or total abstinence from all intoxicating drinks, was not, however, fairly established generally till 1834; when the seven men of Preston, assisted by Mr. Pollard of Manchester, and others, started societies fairly and exclusively on the teetotal principle. The published lectures of Mr. Livesey of Preston constitute an era on this subject; and the labours and lectures of a gentleman in the iron trade at Liverpool (whose name we forget at the moment) were extremely useful at this period.

Shortly after this the New British and Foreign Temperance Society was established at London on teetotal principles, under the management of Messrs. Janson, Oxley, Meredith, Green, and others; and very soon almost all the original temperance societies in Great Britain adopted the rule of total abstinence. The doctrine was pushed over into Ireland by the Liverpool gentleman we have mentioned, to be brought into the most brilliant success in due time afterwards by Father Mathew.

About 1836 the teetotal element was established in the American temperance societies, which, it is believed, are now universally conducted on this principle.

The total abstinence reform has extended itself successfully into Canada, New Brunswick, India, the West Indies, and our other colonies. It has also made its way into the South Sea Islands, and elsewhere among the half-cultivated races; and has been partially adopted in Sweden and other parts of the North of Europe.

The following are the terms of the certificate to which we have referred, as having received such numerous signatures of medical practitioners, including those of many of our most distinguished physicians and surgeons:

"We the undersigned are of opinion—

"1. That a very large proportion of human misery, including poverty, disease, and crime, is induced by the use of alcoholic or fermented liquors as beverages.

"2. That the most perfect health is compatible with total abstinence from all such intoxicating beverages, whether in the form of ardent spirits, or as wine, beer, ale, porter, cider, &c. &c.

"3. That persons accustomed to such drinks may, with perfect safety, discontinue them entirely, either at once, or gradually after a short time.

"4. That total and universal abstinence from alcoholic liquors and intoxicating beverages of all sorts would greatly contribute to the health, the prosperity, the morality, and the happiness of the human race."

Let us say, however, *in limine*, that whilst taking upon ourselves the earnest advocacy of these doctrines, we by no means wish to identify ourselves with all that has been written and uttered by the disciples of the Total Abstinence system. Too often their intemperance has passed from their cups to their language; the finger of pharisaical scorn has been pointed at the "moderate drinkers," whose consciences have not yet told them that there is any harm in the temperate use of fermented liquors; and even those who agree with them in their leading principles, and who join with them in their practice, but who hesitate at sanctioning all that ignorant enthusiasts think fit to assert, have been stigmatised as enemies rather than as friends to the great cause of emancipation. Now we most fully recognise the importance of earnest and awakening appeals to those who are sunk in the lethargic slavery of one of the most brutalising of all sensual indulgences; but we are certain that exaggeration never ultimately serves the interests of truth. No words *can* depict too strongly the evils of intemperance. No appeals *can* be too urgent or awakening to the blunted feelings of those who are ruining themselves both for time and eternity by an habitual indulgence in this overpowering propensity; but surely there is plenty of matter for the advocates of abstinence, without going out of their way to condemn those who maintain that fermented liquors are the gifts of God, to be *used* in moderation but not *abused*. We are quite sure that the manner in which their public proceedings have been conducted has kept many aloof, who would have been most valuable and influential advocates of this great cause of social and individual reformation. The fact we believe to be, that a large proportion of the intemperate denunciations and rash statements to which we allude, have been put forth by men who have themselves felt all the tyranny of this dreadful slavery; and (as we have been informed by some most competent observers) they feel, on their emancipation from it, a sort of excitement that is almost uncontrollable, urging them to bear public testimony to the evils from which they have escaped, and infusing into that testimony a strength that makes it operate powerfully on the minds of those whom they desire to awaken, whilst it leads them (with the want of discrimination natural to men of imperfect education) to express the most unmitigated reprobation of those more especially who profess themselves friends of temperance, but who do not feel called upon to preach or to practise total abstinence.

Now we are quite content to brave their condemnation for the sake of what we consider to be truth; and feeling satisfied, as we just now said, that the interests of truth cannot be served by exaggeration, we think it right fearlessly to state that we cannot, with them, affirm that we consider alcohol in all its forms to be nothing else than a poison. We cannot conscientiously go the length of denying that under any circumstances, whether of health or disease, the administration of alcohol can be justified. We believe that if the whole world could be *really temperate* in the use of fermented liquors, there would be no need of Total Abstinence societies. But we advocate their principles, because sad experience has shown that a large proportion of mankind *cannot* be temperate in the use of fermented liquors, and that nothing short of total abstinence can prevent the continuance, in the rising generation, of the terrible evils which we have at present to deplore;—because experience has

further shown than the reformation of those who are already habitually intemperate cannot be accomplished by any means short of entire abstinence from fermented liquors;—and because experience has also proved that this reformation cannot be carried to its required extent without the moral influence of the educated classes. Such influence can only be afforded by *example*. There is no case in which its superiority over mere precept is more decided and obvious than in this. "I practise total abstinence myself," is worth a thousand exhortations; and the miserable failure of all the advocates who cannot employ this argument should lead all those whose position calls upon them to exert their influence (and who are there who do not possess *some* means of thus doing good?) to a serious consideration of the claims which their duty to society should set up in opposition to their individual feelings of taste or comfort.

Without setting ourselves up as apologists for the managers of Total Abstinence Societies, we may remark that this, like other great movements, has been mainly brought about by the agency of individuals, whose very enthusiasm necessarily renders them somewhat one-sided in their views. It is for the cool-judging philosopher to place these views in their true light, and thus to guide mankind in forming a just appreciation of them; but such a movement might be retarded for centuries, or might never take place at all, if there were no one-sided enthusiasts in the world, and it were left to the philosophers to set it going. Now the leaders of the Total Abstinence movement have conscientiously felt that a charge has been laid, as it were, upon their shoulders, to rid the country of intemperance; and they can fairly plead the enormity of the disease, and the difficulty of completely eradicating it, as an apology for the severity of their cure. There are so many apertures, they affirm (and with justice), by which men contrive to escape from the Abstinence principle,—creeping out, like cunning foxes, in search of the object of their craving,—that every hole must be stopped up, every apology for a recourse to alcoholic liquors cut off. We admit the danger, and the necessity for the utmost caution in the avoidance of it. But still we do not think that those of the professed advocates of Total Abstinence, who deny the possibility of *any* benefit from the use of alcohol, have taken up a defensible ground; and the argument for the stringency of the pledge should rather be based, in our estimation, upon the risk of abuse, which the slightest violation of it has been found by experience to involve, than upon those asserted "poisonous" properties, which it assuredly does not possess in a degree nearly so strong as many of our most valued medicines. Dissenting as we thus do from much that has been uttered from the Teetotal press and platform during the last ten or twelve years, we yet must in justice admit, that when so large a number of parties are concerned (and these chiefly of very limited education), as in the present case, it is somewhat unreasonable to expect perfect wisdom in every mouth, or that zeal in so good a cause should not sometimes get the better of discretion. And the leaders of the movement may fearlessly ask, what assocation of such a size,—political, ecclesiastical, or philanthropic,—could bear to be tried by a severe test on this point?

In the exercise of our own duty as cool-judging critics, we now propose to inquire in the first place into the present state of our know-

ledge as to the physiological action of alcohol on the human body; next, to consider how far the results of the comparative experience of those who make habitual but moderate use of fermented liquors, and of those who entirely abstain from them, under a variety of circumstances, warrants the assertion that total abstinence is invariably (or nearly so) compatible with perfect health, or is even more favorable to health than habitual but moderate indulgence; and finally, to endeavour to deduce from these data such conclusions with regard to the therapeutic use of alcohol, as may cause its employment by medical men to be attended with the greatest possible amount of good and the least admixture of evil.

Our knowledge of the physiological action of alcohol, though far from being sufficiently complete to afford a specific determination of its hygienic or therapeutic value, is yet quite sufficient to guide us in the inquiry; and we shall accordingly state briefly the points which may be regarded as in our apprehension most satisfactorily made out. We believe that no physiologist of repute would now be found to maintain any other doctrine in regard to the materials of the albuminous tissues of the animal body, than that propounded a few years since by Mulder and Liebig; namely, that they are derived exclusively from those alimentary substances whose constitution is similar to their own; so that the non-azotized compounds cannot enter into the composition of more than a very small part of the animal fabric. This doctrine, when first put forth, was received with a degree of hesitation and distrust proportioned to its novel and startling character; but the testimony in its favour has been gradually though quietly accumulating, so that it now commands very general if not universal assent. By the term "albuminous," we mean to designate all those tissues which can be formed at the expense of albuminous matter; and this category includes the gelatinous and horny tissues, as well as those which possess a composition more nearly allied to that of albumen; for we know that the former, as well as the latter, must be generated from albumen during the incubation of the egg, as well as during after-life, when neither gelatine nor horny matter exists in the food. The only tissues in the animal body of which albumen does *not* form the principal basis, are the adipose and the nervous. In *both* these it is probable that the membranous *walls* of the cells and tubes have (like similar membrane elsewhere,) an albuminous composition; the *contents* of these cells and tubes being of a non-azotized character in the latter case as in the former. For it has been pointed out by Valentin (Lehrbuch der Physiologie, Band I, p. 174) that although the substance of the brain and nerves appears to yield an azotized fatty acid when analysed *en masse*, the supposed composition of this acid (which is quite an exception to all chemical probability) may be accounted for by regarding it as a *mixture* of albuminous matter and ordinary fat, which is exactly what might be anticipated on anatomical grounds.

All our present physiological knowledge, then, leads to the decided conclusion that alcohol cannot become the pabulum for the renovation of the *muscular* substance, which process can only be effected by the assimilation of albuminous materials in the food; and that the habitual use of alcohol, therefore, cannot add anything to the muscular vigour. And this conclusion receives most striking confirmation from the well-known fact,

that, in the preparation of the body for feats of strength, the most experienced *trainers* either forbid the use of fermented liquors altogether, or allow but a very small quantity to be taken; their trust being placed in a highly nutritious diet, active muscular exertion, and the occasional use of purgatives which purify the blood of the products of decomposition or draw off superfluous alimentary materials.

That alcohol has some peculiar relation to *nervous* matter, would appear from its power of stimulating the nervous system to increased action; but this power, although coincident with a certain relation in their chemical composition, could not be predicated from the latter, since ordinary fat, which has no such stimulant effect, has a closer chemical relation to nervous substance than is possessed by alcohol. Whether alcohol is capable, by any transformation, of being converted into nervous matter, is a question which we have at present no data to determine; but there can be no doubt that this tissue may be formed equally well from other ingredients of food, which have not like it a stimulant effect. It cannot, therefore, be a *necessary* pabulum to the nervous system; and its peculiar virtues as an habitual article of diet, if such there be, must be looked for in its stimulating qualities.

But, it may be maintained, although alcohol is not requisite or useful as a pabulum for the tissues, it is most efficient as a combustible material, serving to keep up the heat of the body in extreme cold, and to defend it against the effects of vicissitudes of temperature,—in common language, "to keep the cold out." Now this at first sight appears a very cogent argument for its use under certain circumstances, if not for its regular employment; but when its effects are more closely examined, it will be found that neither physiological science nor the results of experience sanction such a proceeding. The maintenance of the animal heat chiefly depends, as all our readers must be aware, upon the formation of carbonic acid and water by the oxygenation of hydro-carbon contained (probably in various forms) in the blood. Now the ingestion of alcohol, so far from promoting, *checks* the oxygenating process; as was shown long since by the result of the experiments of Dr. Prout, who invariably found the quantity of exhaled carbonic acid to exhibit a marked decrease after the ingestion of alcoholic drinks, other circumstances remaining the same. Subsequent experimenters upon the respiratory process have met with the same results; and they are confirmed by the fact ascertained by Bouchardat, that when alcohol is introduced into the system in excess, the blood in the arteries presents the aspect of venous blood, showing that it has not undergone the proper oxygenating process. Now although we may not understand the reason of this (although it seems to be referable to the well-known power of alcohol to prevent or retard chemical changes in organic substances), the fact is of the utmost importance.

The inference to which we are thus conducted by physiological reasoning, instead of being negatived by general experience (as it is commonly supposed to be), is fully confirmed by it. The Esquimaux, Greenlanders, and other inhabitants of the coldest regions of the globe, effectually maintain their animal heat by the large consumption of fatty matter; and whatever may be the *temporary* effect of an alcoholic draught, we believe that all arctic and antarctic voyagers agree that *continued* resistance to cold

is most effectually maintained without alcohol, or at any rate with a much smaller quantity of it than is commonly thought necessary. A very striking proof of this is afforded by the arrangements recently made for the overland arctic expedition, on which the best authorities have of course been consulted by Government. In the programme of these arrangements it is expressly stated, that *no fermented liquors* are to be used by the parties who proceed upon it. We have heard many of the now almost extinct race of stage-coachmen, who had been induced to give up their former habit of imbibing a glass of ale or of brandy-and-water at every stage, and to substitute an occasional cup of hot coffee and a rasher of toasted bacon, speak most decidedly in favour of the superior efficacy of the latter system; and we doubt if any man who had the resolution to adopt it, ever returned to his habits except from the love of liquor. We are strongly inclined to the belief that much of the reputed warming effect of alcohol is due to the hot liquid with which it is usually combined when used for that purpose. A tumbler of hot brandy-and-water, of whiskey-toddy, or negus, is doubtless a very comfortable beverage when imbibed on a cold night on the top of a coach; but our own experience, and that of many others who have tried the experiment, warrants the belief that a cup of hot tea, coffee, or cocoa will have quite as much warming influence, whilst a *cold* alcoholic drink will be nearly if not quite ineffectual. The only cases in which we conceive that alcohol in any form can be more useful than other compounds of hydro-carbon as a heat-producing substance, are those in which all the combustible material of the body has been used up during the progress of a fever or other exhausting disease, and in which the state of the digestive system prevents the reception of any other kind of pabulum into the circulating current. To these we shall hereafter more particularly refer.

Before leaving the question of the heat-producing powers of alcohol, we should advert to an explanation which has been offered, of the diminution in the amount of carbonic acid exhaled after its use; since this explanation, if correct, might vitiate the theoretical part of our argument, though it could not affect the results of experience. It has been stated by no less an authority than Liebig, that this diminution of carbonic acid is owing to the comparatively small proportion of *carbon* in alcohol; the heating power of that substance being chiefly due to the *hydrogen* it contains, which is exhaled from the lungs in the form of the vapour of water, so that, whilst the alcohol is being carried off from the blood, the proportion of carbonic acid to watery vapour, in the products of the combustive process, will be unduly low. This may possibly be true; but it is not the whole truth. There are other substances, as Dr. Prout has shown, whose ingestion is followed by similar results, to which no such explanation is applicable; of this nature is strong tea, especially green tea. And moreover, the experience of Dr. Prout would lead to the decided conclusion, that the presence of alcohol in the blood prevents the extrication of matters whose retention is injurious to it, and for whose removal the respiratory process is the appropriate means. For whilst the diminution in the amount of carbonic acid exhaled continues as long as the effects of the alcohol are perceptible to the individual who has swallowed it, these effects no sooner pass off (which they did in Dr.

Prout's individual case, with frequent yawnings and a sensation as if he had just awoke from sleep), than the amount of carbonic acid exhaled *rises much above* the natural standard,—thus giving, it would seem, unequivocal evidence of the previous abnormal retention of carbon in the system, of which it is only able to free itself after the alcohol has been burned off. And this view is further confirmed by the fact which experience has forced upon men who are so far most unwilling adherents to the abstinent system,—that alcoholic liquors ingested *during* the performance of severe labour, in very hot situations, cause a very rapid and decided failure of the strength; so that men who drink largely of such liquors in the intervals of their work, are obliged to abstain from them whilst their labour is in progress. The physiologist well knows that the quantity of hydro-carbon carried off by the lungs diminishes as the external temperature rises; and one of the reasons for the oppressive influence of a continued exposure to great heat is probably to be found in the obstruction which it presents to the extrication of that amount of carbonic acid, whose removal is necessary for the depuration of the blood. Now, if alcoholic liquors be ingested in this state of the system, and interpose (as we have endeavoured to show that they do) a still further obstacle to this process, the result would be precisely what experience demonstrates,—namely, the flagging of the powers of the system, from the imperfect purification of the blood.—Thus, put the subject in what light we may, theory and practice here go hand in hand in guiding us to the conclusion that alcohol is not more efficacious than other pabula for the combustive process, except in certain disordered states of the system, to which we shall hereafter refer; and that its habitual use cannot be defended on the ground of the necessity for supporting the heat of the body by its means during exposure to very severe cold, whilst it is positively injurious when the surrounding temperature is high. We shall presently adduce other evidence upon this latter point, from the experience of those who have resided in tropical climates.

It appears, then, that the physiological influence of alcohol upon the system, under all ordinary circumstances, cannot be attributed to anything else than its *stimulant* character; and it is almost a self-evident corollary from this proposition, that its *habitual* use even in moderate quantities can exert no beneficial effects. For the healthy fabric should be quite capable of maintaining itself in vigour upon a proper diet and with a due quantum of sleep, exercise, &c., without any adventitious assistance; and if it be not, assistance should be sought from alterations in diet or regimen, or from remedies which tend to promote the regular play of its functions, rather than from stimulants, which may produce in some of these a temporary excitement, but which thus tend to destroy the balance of the whole. The very nature of a stimulant is to produce a subsequent depression, and to lose its force by frequent repetition. The depression is proportional to the temporary excitement; and the loss is thus at least equivalent to the gain. And when a stimulus loses its effect as such by frequent repetition, it is still felt as being necessary to bring the system up to par, an increased dose being required to elevate it higher. Thus, as is well known, those who habitually employ fermented liquors for the sake of their *stimulating* effects, are led on from small beginnings to most

fearful endings; and the habit, growing by what it feeds on, becomes a necessity. No pretext is more commonly given out as an apology for the habitual use of fermented liquors, than the aid which a moderate employment of them is thought to afford to the digestive process. But we maintain that, where a man duly observes the laws of health, the appetite will always desire the amount of food which the system needs, and the stomach will be able to digest it. If health is to be measured by the capacity for eating, then the habitual moderate use of fermented liquors may be conducive to it; but if the increase in this capacity which they produce be of no service to the economy at large, they cannot have any other than an injurious effect, by leading us to overtask the powers of our digestive apparatus. Thus, as Liebig has very well pointed out, the residents in warm climates who take stimulants before their meals, in order to make up for the deficiency of appetite, act upon a most unphysiological and ultimately injurious system; forgetting or being ignorant that the real demand for food is much less when the surrounding temperature is high, and that the diminished appetite really indicates the diminished wants of the system. In a large proportion of the cases in which the habitual employment of fermented liquors has really a show of utility, we are quite certain that a copious use of cold water externally, and the substitution of it for more stimulating beverages, will be found in the end to be the most wholesome practice, tending (as large experience has shown that it does) both to improve the appetite and to invigorate the digestive powers.

We do not go so far as to maintain that *no* exceptions are to be made to this rule; but we are satisfied that these exceptions are much fewer than is commonly supposed; and that they are to be made rather in cases where some temporary disturbing cause is acting upon the system, than in those in which there *seems* to be an habitual want of assistance.

In like manner, we believe that the nervous system can derive no benefit from the habitual use of fermented liquors; since, in a healthy state of body, it ought to be equal to the work it is called upon to perform; and, if over-tasked, it must be renovated by repose. Doubtless it may be stimulated to increased temporary activity by the use of alcohol; but this activity can never be long sustained; and in the state of subsequent depression, the body is more than usually liable to the influence of morbific causes. There is no part of our frame which requires nicer management, or which is more rapidly acted upon by influences from without or from within, than the nervous system. The regular employment of it, if well directed, and carefully supported by attention to everything that promotes the general health, may be carried to a marvellous extent; and yet, in some peculiarly susceptible constitutions, the least indisposition gives rise to a feeling of nervous depression, which might seem to demand the use of stimulants for its removal. In the majority of cases, however, this feeling of depression is the result of habitual inattention to the laws of health; and, although it may be temporarily removed by alcohol, yet the evil is only palliated for a time; and the very means employed lays the foundation for a future increase of the feeling of depression, requiring an increase of the stimulus for its removal. We may appeal to universal experience in support of our doubts, whether those who have had frequent recourse to alcoholic stimulants for the removal of

nervous depression, arising from previous exhaustion by over-work, or from disorder of some other function of the body, have been able to stop at any one point; or whether they have not, to produce the same effect upon their feelings, been obliged to increase the dose, the more frequently it has been repeated. This we conceive to be the great and palpable distinction between the effects of a stimulant which excites, and a pabulum which supports, the system. The former needs to be increased in proportion to the frequency with which it is employed. The demand for the latter varies merely in accordance with the amount of renovation to be effected.

But it is often asserted, that although stimulants may be dispensed with in temperate climates, the habitual use of them is necessary to aid the system in resisting the enervating influence of extreme heat. Let us see how far the results of experience, when carefully sifted, bear out this doctrine. In the first place, we presume, it will be readily admitted by our readers, that the effects of *excess* in the use of fermented liquors are far more injurious in hot than in temperate climates. A very considerable proportion of the mortality in the stations which have the reputation of being the most unhealthy, is so directly traceable to such excess, that its continuance can scarcely be accounted for, except on the principle of "a short life and a merry one." Some years since, whilst ourselves stationed in the West Indies, we conversed with a gentleman resident in Tobago, who informed us that the average annual mortality amongst the Europeans of that island was *one in three*. Upon inquiry into the habits of the residents, we found that intemperance prevailed amongst them to a most fearful extent; few getting up in the morning without their glass of sangaree (wine and water), and the strength of their beverage being gradually increased during the day, until it arrived at neat brandy at night. He further spoke of it as no uncommon occurrence for a party of friends who had met at a drinking bout, to be summoned within two or three days to the funeral of one or two of their number. Our informant was himself apparently quite indisposed to recognise between these occurrences any relation of cause and effect; and was obviously under the belief, that if it were not for the protecting influence of good wine and brandy, his life would be worth a yet shorter purchase. Our readers will probably form a different conclusion. We have on various occasions sought for information from those who had best preserved their health during a long residence in tropical climates, as to their habits in regard to the use of alcoholic liquors, and have almost invariably found that they had practised extreme moderation, if not total abstinence. All medical men who have practised in India agree in attributing the large proportion of cases of severe disease which present themselves among Europeans in that country to the immoderate indulgence in fermented liquors. A statistical proof of it is afforded by the fact quoted by us in a former Number of this Journal (January, 1841), in regard to the experience of the British army in Bengal, in which temperance societies (on the old plan of abstinence from distilled spirits only) had been established a few years previously. We must refer to our former abstract for a fuller statement of the results of the disuse of ardent spirits and the diminished consumption of other fermented liquors; and shall only here state that the returns drawn up by the Inspector-General for the first six months of 1838 show

that the average daily per centage of sick belonging to the Temperance Society (about one third of the whole strength) was only $3\frac{2}{3}$, whilst the daily per centage amongst the remainder was $10\frac{1}{5}$. Even this result does not give the most favorable view of the case; for many men joined the Temperance Society whose constitutions had been ruined by previous dissipation, and several such were habitual tenants of the hospital until invalided. Since that time, the Total Abstinence principle has been introduced among Europeans in India and other tropical countries; and, we are assured, with the most favorable results. There has been no want of satisfactory medical testimony in its favour. Indeed, all our best writers on tropical diseases are most explicit on this point. And we may here give the evidence recently given by Mr. Gardner, now superintendent of the Botanic Gardens in Ceylon, a well-educated surgeon, who spent several years of most active exertion in Brazil, and who penetrated into that country further than any other scientific European. During three years' travelling in that climate, under constant fatigue and exposure to vicissitudes of weather and irregularity of living, his only beverage, besides water, was tea, of which he had laid in a large stock previously to his departure from Pernambuco. He was told when he arrived at Brazil, that he would find it necessary to mix either wine or brandy with the water which he drank; but a very short experience told him, not only that they are unnecessary, but that they are decidedly hurtful to those whose occupations lead them much into the sun. "Whoever drinks stimulating liquors," he says, "and travels day after day in the sun, will certainly suffer from headache; and in countries where miasmata prevail, he will be far more likely to be attacked by the diseases which are there endemic."

Now this testimony, from those who *have tried* the experiment of total abstinence in tropical climates, and who have watched its results in others, must surely be regarded as of greater weight than any vague notion to the contrary, however prevalent such notion may be; more especially as it corresponds exactly with what might be predicated upon scientific grounds. For, as we have already shown, the introduction of alcohol into the blood obstructs its depuration by the respiratory process; more especially when the surrounding temperature is high, and the natural exhalation of carbonic acid is consequently diminished. Hence the system is subjected to the injurious influences of an imperfectly decarbonized and aerated blood; and the liver is called upon to do what the lungs are prevented from effecting, —the foundation being thus laid, in the habitual stimulation of the liver to undue functional activity, of inflammatory disease in that organ.

The testimony of those who are exposed to *vicissitudes* of climate is perhaps even more valuable than that of those who have to sustain continued heat or severe cold; and under this aspect we regard the evidence of intelligent seamen as of peculiar importance, in addition to the force it derives from the well-known attachment of their class to spirituous liquors. That such regard the total abstinence principle as at any rate a *safe* one, may be inferred from the circumstance that it is now carried into practice in a very considerable part of the merchant service in this country, and in a still larger proportion of American vessels; and that the adoption of this plan is not known to occasion any difficulty in obtaining crews for the "temperance ships," when a fair compensation is made in the superior

quality of the provisions and allowances, or in the rate of wages, as an equivalent for the "stoppage of the grog;" in fact, such ships are often in positive request. And it is not a little worthy of note, that lower rates of insurance are frequently taken upon "temperance ships," than upon those in which the usual allowance of spirits is continued; it being well known that a large proportion of losses at sea are due to the intemperance of officers and men. We consider that an immense improvement was made in the victualling of the navy, when the allowance of grog was diminished, and coffee, cocoa, &c. were substituted; and we trust that the day is not far distant when the total abstinence principle may be recognised as worthy of government support in the army and navy, instead of being (as at present) checked or discouraged by the strong temptations to indulgence which are placed so completely in the way of the men, as to require great moral courage on their parts to resist them habitually. That the moral condition of sailors is more likely to be raised by the universal extension of the abstinence system amongst them, than by any other single measure of improvement, is unhesitatingly declared by all who have had experience of the superior conduct of the sailors on board the "temperance ships;" and we feel assured that the "cat" may be discarded when the grog is thrown overboard; at least two thirds of the offences now punished by flogging having their origin directly or indirectly in alcoholic excitement.

That there are peculiar difficulties attending the complete withdrawal of the allowance of spirits in the naval service, we freely admit; and it is well that these difficulties should be openly stated, in order that they may be fairly met, and, so far as possible, counteracted. We have requested a distinguished medical officer attached to the late Antarctic expedition, to place us in possession of his opinions on this point; and we are sure that those who are acquainted with the scientific reputation of Dr. Joseph Dalton Hooker will consider the declared results of his experience under such trying circumstances as highly important. In reply to our question, whether the habitual use of fermented liquors may be *safely* dispensed with on board-ship, he thus writes:

"I should say clearly so, and with benefit, too: *provided the water be good.* For the comfort of the men the water should be palatable; and this is far from being universally practicable. The *officer* can vary his viands and drink so much, that to have no spirits is no loss to him; but there is no substitute for grog to the sailor. Beer is too bulky; lemonade soon palls when daily used, and would not agree with all. Cold tea is not palatable to every one, even if recommendable; and the sailor gets hot tea once a day as it is, which in hot weather is almost once too often. You must not judge of the navy by the merchant service. In the latter, the sailor joins for immediate profit, and is willing to go through the voyage with bad water and no spirits, for it only lasts a few months or years; and, in joining an abstinence ship, he does not forswear grog for ever, and has opportunities of varying his beverage with his ship. The good navy sailor, on the other hand, ships for forty years (at least such are the men we want and prize); and for five, or even seven years' commission at a stretch, in a very hot climate, where the water is bad, perhaps, he has but one diet, and no prospect of its being altered.

"Perhaps the gravest objection to abolishing fermented liquors in the navy allowance is, that you cannot do so with the officers. They are allowed to buy and lay-in private and mess-stock, and the service allows them stowage. It is not so with the sailor. He is prohibited from laying in a sea-stock, both because no room is allowed him to store it, and because he could not afford it, or be

trusted if he could. In the army, where there is little or no communication between officers and men, and no intimacy, this would not tell so heavily as on board-ship, where every one has a great fellow-feeling with his shipmate, and where partiality in the treatment of any class, with regard to the withholding of what each in his station is accustomed to on shore, could not fail to produce a very strong feeling. These are, however, secondary considerations. *Allowing the water to be palatable*, I have no hesitation in saying that the habitual use of the spirit may be *beneficially dispensed with*, as far as the health of the crew is concerned."

Two points in the foregoing extract are particularly worthy of note: first, the importance assigned to the goodness of the water; and secondly, the stress laid upon the example of the officers. It is well known that the substitution of iron tanks for wooden casks has been of the greatest benefit in improving the quality of the water on board-ship, as well as in saving stowage-room; but much still remains to be done. If the accounts which we have recently heard, of the success of the application of electricity to the decomposition of those minute quantities of organic matter in water that has been long kept, to which its taint is due,—an application which has been recently made by the well-known electrician Mr. Andrew Crosse, and for which he has taken steps to secure a patent—should prove correct, a great boon will have been conferred on our naval service, which will render it much easier, we hope, to extend to it the still greater boon of the total abstinence reformation. With regard to the influence of the example of the higher classes, we see that it is far stronger on board-ship than on shore, either for evil or for good. Let the officer once determine to forego his moderate allowance of wine, spirits, or malt liquor, and the seaman will be easily induced to follow his example. The medical officers of the navy have it in their power to set on foot a reformation, the glory of which shall far surpass that of the greatest victories which history records; for let *them* begin and persevere, without fear of ridicule or obloquy, and we feel assured that they will make certain progress, though it may be slow.

In reply to our second query, whether the abstinence in cold climates is attended with positive benefit, Dr. Hooker writes:

"I do think that the use of spirits in cold weather is generally prejudicial. I speak from my own experience. *It is very pleasant.* The glass of grog warms the mouth, the throat, and the abdomen; and this, when one is wet and cold, with no fire, and just before turning into damp blankets, is very enticing; but it never did me one atom of good: the extremities are not warmed by it; and when a continuance of exertion or endurance is called for, the spirit does harm, for then you are colder or more fatigued a quarter or half an hour after it than you would have been without it. Several of the men on board our ship, and amongst them some of the best, never touched grog during one or more of the antarctic cruises. They were not one whit the worse for their abstinence, but enjoyed the same perfect health that all the crew did throughout the four years' voyage. Many of our men laid in large stocks of coffee, and when practicable had it made for them after the watch on deck. These men, I believe, would willingly have given up their spirits in exchange for coffee; but we could not ensure them the latter on the requisite occasions. To the southward of the antarctic circle, or of lat. 50°, you may say, it blew a gale three days out of five; there was always a heavy swell running; the whole ship and bedding were damp from condensation, where not so from shipping seas; the atmosphere of the lower deck (with hatches battened down) such that you could not see from one mess-table to another; and this for days

together. There is neither standing, sitting, nor lying in comfort. All hands, officers and men, up and ready; the one watch on deck, the two others on the *qui vive* for any emergency. In cruising amongst the ice, the ship is perhaps put about every half-hour; and we have been for sixteen hours in this state. Every time we go on deck we are drenched with cold salt water, which sometimes freezes as it falls; and when you go below, there is really nothing to do but 'lick your paws,' as the men say. Nothing hot can be got."

Certainly a more uncomfortable situation, short of positive danger, can scarcely be imagined. Let us see what Dr. Hooker says of the use of spirits on these occasions, in answer to our third query—whether there exist *any* circumstances, which in his opinion render the *occasional* use of alcoholic liquors beneficial.

"This is perhaps as extreme an instance as I could bring forward of the demand for spirits. Now I do not believe that to 'splice the main-brace' half a dozen times, or even more, in this sixteen hours, would do any good in the way of giving strength; but to refuse the men some grog would be a great hardship. I have seen grog given, half a gill at a time, thrice (I think) under such circumstances, with no perceptible harm; but I do not suppose it did any good; and more would, I am sure, have done mischief. The fact of giving it did good in one way;—it made the men joyful, not from excitation, but as we all rejoice on cutting the Christmas pudding; and I quite believe that under that continued exertion the bad effects were dissipated. But this is a very different thing from doing any real physical good. I can well suppose the effect to have been, though inappreciably, the contrary. Of one thing I am sure, and that is, that no one was more ready for a repetition of the exertion from taking the stimulus; the intervening time was more pleasantly and comfortably passed. It may be a question whether, granting the spirits to have done some good towards exhilarating, when no modern appliances could be available, it would be desirable to withdraw it on such occasions. It is a choice of evils perhaps.

"I know of only one occasion on which the spirits appeared indispensable; and that was, when a little more exertion at the crowning of a mighty and long continued effort was demanded. Thus the ship, when sailing in the pack-ice, is sometimes beset, or falls to leeward into the lee-ice. This takes two or three minutes; but if there is much wind, it takes many hours to get her out. Not being in command, the sails are of no use; and the ice prevents her moving in any way but with it to leeward. Under these circumstances, the only way to get her out is by fastening ropes from the ship to the larger masses of ice, and warping her out by main force against the wind. Now I have seen every officer and man in the ship straining at the capstan for hours together, through snow and sleet, with the perspiration running down our faces and bodies like water. Towards the end of such a struggle, at the mighty crowning effort, I have seen a little grog work wonders. I could not have drunk hot coffee without stopping to cool; nor if I had, do I think it would have supplied the temporary amount of strength which was called for *on the spot* under circumstances like this.—These, however, are extreme cases, which do not affect the sailor in his ordinary condition, and which any ship might be well prepared for."

Fully agreeing with Dr. Hooker, that we know of nothing which, under such trying circumstances, could be advantageously substituted for the alcholic stimulus, we may add the remark that, where the habitual use of it is relinquished, a much smaller amount of it will suffice to produce the required stimulation, than when a large allowance is daily imbibed. Every medical practitioner must be aware of the necessity of regulating the quantity he administers for any particular object by the usual habits of his patient; a single glass of wine doing that with one, which an entire

bottle would scarcely effect with another more seasoned vessel. We must not omit Dr. Hooker's conclusion.

"The great practical difficulty on board-ship is, that you have no available substitute for bad water but good grog, as the sailor is at present situated. I cannot, however, but think that, with more attention to the comforts of the sailor, his own love of liquor would diminish; and that he might be weaned from it by the officers, though the depriving him of it by the Government would be a dangerous experiment."

We shall now pursue our inquiry through other occupations and habits of life; and in proof that the severest muscular labour, continued through long periods of time, and under circumstances of the most trying character, is perfectly compatible with total abstinence from alcoholic liquors, we shall present our readers with a few selections from a large body of testimony which we have obtained from sources worthy of complete reliance.

A gentleman residing at Uxbridge thus writes:

"In the year 1841, I obtained the amount of bricks made in our neighbourhood by our largest maker; and the result in favour of the teetotallers was very satisfactory. Out of upwards of 23 millions of bricks made, the average per man made by the beer-drinkers in the season was 760,269; whilst the average for the teetotallers was 795,400—which is 35,131 in favour of the latter. The highest number made by a beer-drinker was 880,000; the highest number made by a teetotaller was 890,000: leaving 10,000 in favour of the teetotaller. The lowest number made by a beer-drinker was 659,000; the lowest number made by a teetotaller was 746,000: leaving 87,000 in favour of the teetotaller. Satisfactory as the account appears, I believe it would have been much more so, if the teetotallers could have obtained the whole gang of abstainers, as they were very frequently hindered by the drinking of some of the gang; and when the order is thus broken the work cannot go on."

Brick-making, we believe, is commonly accounted one of the most laborious of out-door employments; at any rate it is one which involves exposure to all the vicissitudes of weather, and therefore it may be taken as a fair sample of severe labour under trying circumstances. Respecting the above return, we think it should be especially remarked that it does not record the result of a trial made expressly for the purpose, with full advantages on both sides, and continued for a short time, during which the desire for victory might be supposed to lend an adventitious aid; but that it shows the actual amount of work done during an entire season by bodies of men working on both systems, but not pitted against each other.

The following statement by Mr. William Fairbairn, an eminent machine-maker, of Manchester, at the head of a firm employing between one and two thousand workmen, will be found in the Sanitary Report for 1840.

"I strictly prohibit on my works the use of beer or fermented liquors of any sort, or of tobacco. I enforce the prohibition of fermented liquors so strongly, that if I found any man transgressing the rule in that respect, I would instantly discharge him without allowing him time to put on his coat. In those foundries in which there is drinking throughout the works all day long, it is observed of the men employed as workmen that they do not work so well; their perceptions are clouded, and they are stupefied and heavy. I have provided water for the use of the men in every department of the works. In summer-time the men engaged in the strongest work, such as the strikers to the heavy forges, drink water very

copiously. In general the men who drink water are really more active and do more work, and are more healthy, than the workmen who drink fermented liquors.

"I observed on a late journey to Constantinople, that the boatmen or rowers to the caïques, who are perhaps the first rowers in the world, drink nothing but water; and they drink that profusely during the hot months of the summer. The boatmen and water-carriers of Constantinople are decidedly, in my opinion, the finest men in Europe as regards their physical development, and they are all water-drinkers; they may take a little sherbet, but in other respects are what we should call in this country teetotallers." (p. 252.)

The following is the published testimony of Mr. Josiah Hunt, a well-known agriculturist in Gloucestershire, as to the efficient performance of harvest-work on the abstinence system. His experiment is further valuable as showing the positive advantage gained by the substitution of articles of solid food for alcoholic liquors of equal cost—a point of great economic importance to the labouring classes. After mentioning the terms on which his work had been done in former years (namely 8*s*. 6*d*. per acre, and an allowance of three gallons of cider, or an additional payment of 3*s*. per acre), he continues:

"I let 80 acres of grass to mow, harvest, and stack, to four of those who did the like last summer, with three others, at 8*s*. 6*d*. an acre in money; and instead of 3*s*. an acre for drink, an equal sum to be expended in the purchase of unintoxicating drink and food; on condition that neither of them should taste any fermented liquor during the progress of the work. Three of the men had signed the pledge in the previous winter; the other four did so about a fortnight after they began to work.

"They commenced on the 10th of June, and finished on the 26th of the next month; which was longer by two weeks than they would have been if the weather had proved fine. The whole of the work, without the least exception, was performed more to my satisfaction than ever was the case before. During the progress of it, they gave abundant proof that they were equal to as much work as any seven men in the neighbourhood; and also to as much as they themselves had been equal to at any time whilst taking intoxicating drinks. They were not picked men; four of them, about the respective ages of 55, 41, 30, and 29, having worked for me for several years; the others, aged 41, 30, and 20, having been engaged at various times in the spring, without any intention of retaining them during the summer; and that they were not of more than average strength may be inferred from the fact that I was told before they began,—'We know very well how your experiment will end; for there are but two men out of the seven that can do a day's work; they will be knocked up before they have mowed two hours.' At the end of the first day's mowing it was, however, found that they had done more than any other men in the neighbourhood; and as they thus proceeded without being 'knocked up,' the tables were turned, and I was then told that they performed so well in consequence of their good living. How this was obtained, I propose presently to show; but before doing so, I must, in justice to the men, add, that their conduct during the summer has presented a striking contrast to much that I have witnessed in ale and cider-drinkers. I have not heard any improper expression escape either of them during the whole period, and their general behaviour has been very creditable.

"Instead of intoxicating drink, they used tea and cocoa, sweetened with sugar or treacle, and skim-milk. The following are the quantities used, with the cost, viz.: 2lbs. of tea, 22lbs. of cocoa, 31½lbs. of sugar, 4½lbs. of treacle, and 60 gallons of skim-milk; all of which cost £3 12*s*., instead of (as at the rate of the cider last year) £12. There thus remained £8 8*s*. to expend in food; and for one shilling more than this sum, or £8 9*s*., they were enabled to procure the following, viz.: one hundred-weight of beef, one hundred-weight of bacon, four sacks of

potatoes, and one sack of flour, with twenty pounds of suet for puddings; all of which 'good living,' be it remembered, was obtained out of the saving effected by the substitution of an unintoxicating drink for the intoxicating and expensive one of the previous summer." (Bristol Temperance Herald, Sept. 1841.)

As this, being the testimony of a single individual, might be thought open to question, we shall add a summary of the testimony of thirteen farmers and labourers in the neighbourhood of Bodmin, Cornwall, who have for some years been in the habit of prosecuting their harvest operations without any allowance of alcoholic liquors to the men engaged in them;—an equivalent of some other kind being of course given. The total number of acres of hay and corn harvested by them on this plan in 1846 was 1518; and if to this be added the quantity harvested by teetotallers who were mixed up with beer-drinkers, the total amount harvested on the abstinence principle by the farmers attending Bodmin market would not be short of 3000 acres. This, we think it will be allowed, is a scale of operations quite sufficient to afford satisfactory results. The testimony of those who employ none but total abstinence labourers is unanimous in favour of the system. "I feel assured," says one, "that work can be performed better on the teetotal principle; and that quite as much work can be done as on the use of alcoholic drinks. I am quite satisfied of total abstinence being more congenial to health, strength, and happiness in the harvest-field than the old drinking system, and am resolved as long as I remain a farmer to save all my hay and corn on that principle." "I am glad to inform you," says another, "that I have done my labour this harvest with comfort and contentment on the teetotal principle, as I have for the past *eight successive* harvests." "Our parish," writes a labourer, "is divided into small farms; and many of the farmers have their harvest-work done by men who have to go to the mine or stream-work, and do their day's work first, and then in the afternoon they go in the harvest-field; and most of them say they would rather have teetotal beverages than intoxicating drinks. I have passed through *nine* harvests on the teetotal principle, and three I have been on the cold-water system, and I find this is best of all. I have been at it for a month together, and I could always do my work to the satisfaction of my employers; and the men I have worked with have said that teetotalism is best." Another farmer says, "We have saved our hay and corn for eight or nine years to my satisfaction; the last two years the best of all. The last harvest has been passed with pleasure to the men (though not pledged teetotallers) and to myself. Not an oath nor an angry word has been heard. We have worked in times of necessity till 10 o'clock; and I never heard one say that he was tired. One of them cut an acre and a half of wheat after 2 o'clock; two acres a day per man was the average quantity cut; and they worked with such comfort to themselves, that they wish to go through another harvest on the same principle." "I have sent," writes another farmer, "the return of the hay and corn I have cut and saved this year; and I can say that I have done it much more comfortably on the teetotal principle than we ever did when we used malt liquor; the work-people have done their work well and with great spirit." "Without any brawl or anything uncomfortable," is the additional testimony of another. "As to the comfort of the plan," writes another, "I can say, the more I have of it, the better I like it. Never did I do my work so easily, nor enjoy my health so well as

I have since I abstained from all intoxicating drinks; and as to the work-people in the harvest-fields, all appeared to be pleased and satisfied; and some of them who were not teetotallers said, that they would sooner work on the teetotal plan than on the drinking system, if they could be attended to properly. My full conviction is, that if farmers would but put half the expense in solids and teetotal drinks for their men, that they put in beer and cider, their men would be better pleased, and their work be better done, and a great deal of sin would be prevented." Where an improved diet has been substituted, in the Bodmin district, for alcoholic drinks, it has been found that the labourers, like Mr. Hunt's, *increased* in weight during the severe labour of harvest, as much as 5lbs. per man on the average.

We think that we have now adduced sufficient testimony of the inutility, to say the least, of fermented liquors, as regards the maintenance of muscular strength in field labour. It is obvious that practice here fully bears out theory; and that the substitution of solid aliment containing the materials of muscular tissue, for a liquid which contains but little of these, and whose principal constituent is a heat-producing substance—never less wanted than when laborious exertion is being made under the summer sun—is attended with the very result which the physiologist would predict, namely, an increase in the amount of muscular substance, and consequently in muscular vigour. If we only go the length of admitting that they are unnecessary, the duty of doing our utmost to check their employment seems to us imperative; since it is the universal testimony of those who have fairly tried the abstinence system, that the temper and habits of workmen, who were previously "moderate drinkers," are in every way improved by it,—to say nothing of the avoidance of absolute intoxication with all its evils, which, although the most obvious, is not perhaps the most important result of the abstinence system—since for one *drunkard* there are scores who are injuring their bodies and souls, their families and their employers, and who are consequently in the end more or less burdensome to the public at large, by what is accounted amongst them but a *moderate* use of fermented liquors.

We shall next adduce evidence of the equal inability of alcoholic drinks to sustain the bodily powers in prolonged labour of other kinds; and we may first mention a very striking case which came within our own knowledge a few years since. A gentleman with whom we were then intimate, and who, though moderate in his own habits, was by no means a disciple of the total abstinence system, informed us that he had once had the command of a merchant vessel from New South Wales to England, which had sprung so bad a leak soon after passing the Cape of Good Hope, as to require the continued labour, not merely of the crew but of the officers and passengers, to keep her afloat by the use of the pumps during the remainder of her voyage, a period of nearly three months. At first, the men were greatly fatigued at the termination of their "spell" at the pumps; and after drinking their allowance of grog would "turn in" without taking a proper supply of nourishment. The consequence was, that their vigour was decidedly diminishing, and their feeling of fatigue of course increasing; as our physiological knowledge would lead us to expect. By our friend's direction, coffee and cocoa were substituted for the grog; a hot "mess" of these beverages being provided, with the bis-

cuit and meat, at the conclusion of every watch. The consequence was, that the men felt inclined for a good meal off the latter, their vigour returned, their fatigue diminished, and after twelve weeks of incessant and severe labour (with no interval longer than four hours) the ship was brought into port with all on board of her in as good condition as they had ever been in their lives.—When visiting Messrs. Boulton and Watt's celebrated factory at the Soho, Birmingham, some years since, we were much struck by the Herculean aspect of a particular workman, who was engaged in forging the steel dies (used in coining) into the massive blocks of iron in which they are imbedded. This, we were informed, was the most laborious occupation in the whole factory, requiring a most powerful arm to wield the heavy hammer whose blows were necessary to ensure the union of the two metals; and involving also constant exposure to a very high temperature. The day was sultry and oppressive; and the additional heat of the forge was, to our feelings, almost unbearable. But we stood awhile, watching this gigantic workman, the girth of whose chest seemed twice that of any ordinary subject, whilst, naked to the waistband, and with the perspiration streaming down his head and body, he dealt the rapid and skilful blows of his ponderous hammer upon the heated mass. At the first pause, we asked him (from mere curiosity, for teetotallism was then scarcely talked of) what liquor he drank; and he replied by pointing to a whole row of *ginger-beer* bottles behind him, the contents of one of which he imbibed every ten or fifteen minutes. He stated, upon further questioning, that he found it quite impossible to drink alcoholic liquors whilst at his work; their effect being to diminish his strength to such a degree as to render him unfit for it.

This case might be regarded as a solitary exception; but the fact is, we believe, borne out by general experience,—men who have to carry on laborious occupations at a high temperature, as in iron-foundries, gas-works, sugar-houses, &c., finding that the use of alcoholic liquors, whilst they are so employed, is decidedly prejudicial to them. Most such men, however, are in the habit of drinking a moderate amount of beer or other fermented liquors in the intervals of their work, and many more drink to excess; the idea that such liquors enable them to support their exertion being a very prevalent one among all classes. The matter was long ago put to the test, however, by Dr. Beddoes, who, under a conviction of the worse than useless character of fermented liquors for this purpose, went to the anchor-forge in Portsmouth dockyard, and selecting a dozen of the smiths, proposed to them that six of them should drink only water for one week, whilst the others took the usual allowance of beer. The men, convinced that such a system would not answer, refused to try the experiment, and were only induced to do so on the promise of a reward if they succeeded in beating the beer-drinkers. On the first day the two sets of men were very much alike; on the second, the water-drinkers complained less of fatigue than the others; the third day, the advantage was decidedly in favour of the abstainers; the fourth and fifth days it became still more so; and on the Saturday night, the water-drinkers declared that they never felt so fresh in their lives as they had felt during that week. This result may fairly be viewed with suspicion, on account of the strong inducement which this benevolent but not always judicious physician had placed before the water-drinkers to procure their trial of his system; and

it might also be objected that a week's experience was not enough to test it. There is ample evidence at the present time, however, contained in the various publications devoted to the Total Abstinence cause, that labour of the severest kind, and under exposure to the greatest vicissitudes of heat and cold, may be fully as well sustained without alcoholic drinks as with the most moderate and regulated employment of them. We shall not quote from these publications, however, because their statements may appear to bear the stamp of partiality. It is comparatively easy, it may be objected, to get up a body of evidence in favour of *any* system of quackery; but the *whole* truth must be known, before we can give assent to doctrines so completely opposed to the experience and common sense of mankind. We have already grappled with the latter part of the objection, and have shown that the experience of mankind at large is decidedly in favour of habitual abstinence from fermented liquors; and in regard to the particular question of evidence, we trust that our readers will give us some credit for discrimination when we state that we have ourselves collected and carefully examined a great variety of evidence from all parts of the kingdom, some of it furnished by unwilling and much more by indifferent witnesses. Among the documents which we have before us is a letter from a "moulder" in the Gorball's iron-foundry at Glasgow, containing the following statement: "I can assure you that temperance men can do more work and better work than those who use or indulge in spirituous liquors of any kind. I have joined the Total Abstinence Society *eleven years* ago; and from that day to this hour I have abandoned the use of spirituous drinks; and the happy result has been that I am better in health and abler for work than when I was indulging in the use of those delusive liquors." From Rotherham we have the testimony of 100 reformed drunkards, of various occupations; among them that of S. S., who has been a teetotaller now about seven years, and whose work is moulding iron plates for spades and shovels, which is, taking it throughout the day, one of the hottest and most laborious occupations known. We have received from Leeds the testimony of thirty-four men (and we are assured that many more might have been easily obtained), whose signatures are appended to the following statement: "We the undersigned, having practised the principles of total abstinence from all intoxicating liquors, for the several periods stated below, and having during that time been engaged at very laborious occupations, voluntarily testify that we are able to perform our toil with greater ease and satisfaction to ourselves (and we believe more to the satisfaction of our employers also) than when we drunk moderately of these liquors; our general health and circumstances have also been considerably improved." Of these men, twelve belonged to the class whose occupations are commonly regarded as peculiarly trying; seven of them being furnace-men at foundries and gas-works, two of them sawyers, one a whitesmith, one a glass-blower, and the last a railway night-guard. The duration of the periods of abstinence of these men ranged from one to ten years. The following is the experience of a wood-sawyer of Glasgow, whose very well-written letter now lies before us: "I have wrought at this laborious employment for twenty-six years in the city of Glasgow, fifteen years of which I was under the fatal delusion that these liquors were strengthening, and that my hard work required that I should use them for the purpose. I joined the Total Abstinence Society eleven years ago, and

from that day to this hour I have abandoned the use of these drinks; and the happy result has been, that I have been enabled to endure more fatigue, do my work better, and do more of it, than when I was indulging in the use of these delusive liquors." The following is another very striking testimony, given by a nail-maker at Glasgow: "I have been a teetotaller these five years; and though I previously believed that strong drink was necessary to aid me in my work, yet since I have become an abstainer, I find hard work easier, and long hours more readily to be endured. I am also one of the Glasgow Fire Brigade, and was once at a great fire at Mr. Thompson's mill for *seventy-three* hours in succession, with nothing but coffee and ginger-beer, and endured while all my comrades were beat and fell away." In the month of April of the present year, the Temperance Society of Leeds closed their monthly meetings for the season with a "working-man's demonstration," at which the representatives of twenty-one laborious occupations publicly testified to the compatibility of hard labour with perfect health on the Total Abstinence plan. The duration of their trial was in no instance less than three years, and in many instances extended to eleven; the shortest of these periods being, we should think, quite sufficient to test the value of the system.

We do not think it necessary to adduce any further evidence in support of our main position, that total abstinence from fermented liquors is consistent with the maintenance of the most perfect health, even under the constant demands created by labour of the severest kind, or by extremes of temperature; and that, on the whole, the abstinent system is preferable, on physical grounds alone, to the most moderate habitual use of them. The most powerful claim, however, which the Total Abstinence advocates have upon public attention, lies rather (to our apprehension at least) in the moral benefits which their system is calculated to produce; and it is with reference to these that we would earnestly recommend our readers to examine for themselves, whether a great deal that is commonly believed as to the *therapeutic* use of alcoholic liquors is not equally baseless with the notion of the necessity of their habitual use for the sustenance of the body in health. There can be no reasonable doubt that a great deal more wine &c. is employed as medicine than there is the least occasion for. It is so pleasant a remedy, that we have recourse to it on the slightest occasion. People prescribe it for themselves, because they think they understand its action sufficiently well to supersede the necessity of proper medical advice, and because it is so palatable and comforting a draught. Other medicines are usually nauseous to the taste, and our patients are glad enough to get rid of them when they have done their work; but this is too frequently continued long after the purpose which it is supposed to answer is no longer required. And there is abundance of melancholy proof, that a craving for fermented liquors, which has ultimately led to habits of the most degrading intemperance, has been not unfrequently created, even in most delicate, refined, and high-principled women, by the habitual use of them when introduced under the guise of medicine by the physician. The records of Total Abstinence Societies, moreover, show that in a very large number of cases in which drunkards supposed to be reformed have "broken out" or returned to their intemperate habits, the cause of the relapse has been the use of fermented liquors under medical direction, the mere taste of which has excited the craving that seemed

long subdued. Hence in some of the forms of "pledge" the promise is made to refrain from even the medicinal use of alcoholic liquors; which we regard as a most dangerous and unwarrantable proceeding, since there are cases (as we shall presently attempt to show) in which no other agents can have the same beneficial effect, and the difference may even be one of life or death. The proper course we apprehend to be, that those who take the total abstinence pledge should promise not to take alcoholic liquors, except when these are ordered by a qualified medical practitioner; and it is the obvious duty of the medical profession to refrain from ordering them, except where the indication of benefit to be derived from their use is of the plainest possible kind.

We believe that if the question of the therapeutic use of fermented liquors be placed in the same aspect as that on which we have on former occasions attempted to show that the action of almost all our remedies must be at present viewed,—namely, as quite open to that new kind of investigation which consists in the comparison, not of different methods of treatment one with another, but of the results of each method of treatment with the natural course of the disease,—a great deal of evil of various kinds will soon be done away with. At present, nothing in the annals of quackery can be more truly *empirical* than the mode in which fermented liquors are directed or permitted to be taken by a large proportion of medical practitioners. If their physiological action be really as grossly misunderstood as we deem it to be,—if their benefit can be looked for in little else than their stimulating effects, and the belief in their permanently-supporting character be really ill-founded,—if we are to distrust the grateful sensations which commonly follow immediately upon their use, and to look for evil in their more remote consequences (as the experience of the results of their habitual employment would lead us to do), —then it is obvious that a great change will be needed in our usual practice in this respect, in order to bring it into conformity with the mere corporeal requirements of our patients, to say nothing of its bearing upon their moral welfare. We shall not presume to attempt a full exposition of all the circumstances in which the therapeutic use of fermented liquors is indicated; but we shall endeavour to lay down a few general principles, based upon the data which we may derive from the phenomena of their physiological action, and from practical experience as to their habitual or occasional use in the state of health.

In the first place, then, we may lay it down as a general principle, that as alcohol cannot serve as a *pabulum* for the healthy tissues of the body, so it cannot give any *direct* support to the system in furnishing the materials of those morbid products, which frequently constitute a drain upon the system that may become most serious from its amount and continuance. But it will be said that ample experience has shown that the administration of fermented liquors, in cases of excessive purulent discharge (for example), is the only means of sustaining the feeble powers of the system; and we are not disposed to deny that benefit is derivable from them. But we believe that this benefit is to be looked for in their stimulating action upon the digestive apparatus, which enables it to prepare and introduce into the system such an amount of the nutriment that constitutes its real pabulum, as it would not otherwise be able to assimilate. We believe it will be found that if our *chief* trust be placed in fermented

liquors in such cases, failure is almost inevitable; and that the *power* of the system will depend, not upon the quantity of wine or porter that can be poured in without intoxicating effects, but upon the amount of solid nutriment which the patient can digest by their assistance. The quantity of alcohol given should therefore be carefully regulated by this indication; and it should be reduced in proportion as the demand for nutriment is lessened, and the tone of the stomach improves. There is another large class of cases with which practitioners in large towns are especially familiar, in which it is of the utmost importance to sustain the powers of the system for a time against some depressing influence, even though there be no considerable demand for material in the form of an extensive suppuration or the like. Such cases present themselves especially in ill-fed and intemperate subjects, especially among such as have been exposed to the additional depressing influences of bad ventilation and drainage. Almost every disorder in their frames has a tendency to assume the asthenic form; and it is of the greatest consequence, as in the instance already alluded to, to obtain the assimilation of nutritious matter. Here too, we believe that fermented liquors are indicated, not so much as general stimulants, but as exercising upon the digestive apparatus an influence which no other remedy with which we are acquainted can so forcibly exert. But for this purpose we apprehend that the quantity requisite is far smaller than that which is usually administered; and that great injury is often done by over-stimulating the stomach, and thereby positively weakening its power of supplying the real wants of the system.

It is, again, by their temporary stimulus to the digestive operations, that fermented liquors seem to be *occasionally* useful during pregnancy and lactation. We believe that in every case in which the appetite is good, and the general system healthy, the habitual use of these stimulants is positively injurious; and the regular administration of alcohol with the professed object of sustaining the strength under the demand occasioned by the copious flow of milk, is one of the grossest pieces of quackery that can be perpetrated by any practitioner, legal or illegal. For alcohol affords no single element of the secretion; and if the materials of the latter are introduced into the system as fast as they are drawn out of it, there is no exhaustion. In a healthy subject, and under a proper system of general management, this will be the case; and alcohol can do nothing but harm. But there are cases,—very few, however, in comparison with the whole,—in which the conditions of pregnancy and lactation produce an irritable state of the stomach, that prevents it from digesting or even receiving that food which the system really demands; and in some of these we have known the regular administration of a small quantity of alcoholic liquor more efficacious than any other remedy. In one instance of this kind that fell particularly under our notice, in which the mother was most anxious to avoid the assistance of fermented liquors, the lactation must have been early stopped, on account of the want of functional power in the stomach, and the very poor quality of the milk, had it not been that the administration of a single glass of wine or tumbler of porter per day was found to promote the digestive power to the requisite degree, and thus to produce a general invigoration of the system, which was speedily manifested in the improved condition of the child as well as of the mother.

The small allowance we have mentioned never required an increase, and was relinquished without difficulty soon after the weaning of the infant.

We believe, then, that cases are of no infrequent occurrence in which, under some *temporary* depressing influence, the powers of the digestive apparatus are not adequate to supply the demand upon it made by the system, and that recourse may in such cases be advantageously had to alcohol as an equally *temporary* stimulus. But it is worthy of consideration whether, when it is thus administered for purely medicinal purposes, it may not be desirable to give it in such a medicinal *form* as will render it not peculiarly palatable or inviting, in order that the patient may have no inducement to continue the use of it after the real demand has ceased to exist.

There is another class of cases in which it appears to us that alcohol may serve a most important purpose that no other substance can answer. We refer to those in which there is a positive deficiency of heat-producing materials in the system, and in which the digestive apparatus is for the time incapable of introducing such as are ordinarily most serviceable for this purpose. Such a condition is the result of many exhausting diseases, and more particularly of certain forms of fever, in which, without any particular local affection, the powers of the whole system are prostrated by the action of a poison introduced into the blood. Day after day the fatty matter of the body is used up in the respiratory process, and no food is taken in to replace it; and thus, as in cases of simple starvation, the patients die of *cold* unless some means be taken to sustain their heat. Now there is reason to believe that when alcoholic liquors are received into the stomach they are taken into the circulation, not by the lacteals, but by the more direct channel afforded by the permeable walls of the capillaries of the mucous membrane. Theory would teach us that through such a thin septum the alcoholic fluid, being thinner than the blood, would pass towards the latter by endosmose; and experiment fully confirms this view, since it was found by Sir B. Brodie, that alcohol in strong doses exerts its usual effects upon the system, even though the thoracic duct be tied; and MM. Bouchardat and Sandras have obtained evidence of its presence in the blood of the gastric veins. Thus, then, alcoholic fluids introduced into the stomach can be directly absorbed, without any of that preparation which the oleaginous or farinaceous materials of combustion require; and we can well understand, therefore, how, in the advanced stages of fever, when everything depends upon the power of sustaining life until the poison has been expelled from the system, alcohol should be a more powerful therapeutic agent than any other. A severe epidemic of the kind we allude to (the *synochus* of Cullen), which we witnessed some years ago, afforded us the opportunity of seeing the results of opposite modes of treatment in two sets of cases as nearly similar as might be; in neither were any very decided measures adopted during the early stages of the fever, for none seemed called for; but in one set the same expectant practice was continued to the end, whilst in the other the administration of wine and spirit was commenced as soon as the weakness of the pulse and the coldness of the extremities indicated the incipient failure of the circulating and calorifying powers. The quantity was increased as the necessities of the patient seemed to require; and we

remember one case in which a bottle of sherry and twelve ounces of whiskey every twenty-four hours was the allowance for some days. The result was that the mortality on the former system was at least three times as great as on the latter; the patients dying from simple exhaustion and cold, and no local lesion being detectible on post-mortem examination.

Now, in cases where alcohol is thus beneficial, there is an absence of anything like *stimulating* effects. The pulse is usually *lowered* in frequency, instead of being accelerated, and the brain is brought back to more regular action, instead of being disturbed. That a very large quantity of alcohol can be thus given without producing a stimulant effect (and the same is probably true of alcohol taken during exposure to very severe cold) is probably due to the fact of its being burned off almost as fast as it is taken into the circulating system, so that it never accumulates to such an extent as to act injuriously on the brain. We are acquainted with no case in which the beneficial influence of a particular remedy, when administered with caution and discrimination, is more obvious; and we would strongly urge upon those who *intemperately* (as we think) advocate the Total Abstinence cause, and who deny that alcohol *can ever* exert any beneficial influence on the human body, to consider whether so clear a case is not here made out, as to show that one exception at any rate must be made to their assertions.

These are the principal classes of cases in which the regular use of alcoholic fluids seems to us to be indicated. Of those in which their administration as stimulants is urgently called for, in order to sustain the flagging circulation when the heart's action is enfeebled by some violent shock to the general system,—such as concussion of the brain, a blow on the epigastrium producing concussion of the solar plexus, extensive burns or lacerations of the surface, severe and sudden hemorrhage, and the like, —we need say but little. Nothing can be more absurd than to say that, because alcohol is a poison, it can never be beneficial, since the same applies to every one of our most potent remedies; and those who declare that they had rather die than swallow a drop of this poison (and we have been assured that this threat has been acted on), seem to us to be as wilfully throwing away their lives as the suicide who takes a dose of prussic acid or blows his brains out with a pistol. In many of the cases we have mentioned, none but alcoholic stimulants will have the desired effect; and if that effect be not produced, death is inevitable. Those who have watched, as we have, by the bedside of children in a state of collapse from a severe burn, and have had the satisfaction of finding themselves able to sustain the circulation and the warmth of the body by the frequent administration of a spoonful or two of cordial, but have experienced the subsequent mortification of finding that, when they had given place to another less attentive nurse, the little patients have sunk after a brief intermission of the constant support which they require, can fully enter into our appreciation of the value of this class of remedies. But in all cases of this kind, it is of the utmost importance not to carry the stimulating plan too far, or the subsequent reaction may give us a fearful retribution for our incaution. And this is of course to be peculiarly borne in mind in cases of concussion of the brain; since the reaction which results from the injury alone is in many cases so difficult to combat, without the

addition of that which results from the injudicious use of stimulants. We are inclined to think that, in many cases of prostration, in which the cooling of the body is taking place rapidly, and offers an additional impediment to the restoration of the circulation, the freer application of external warmth, especially by means of the hot-air bath, is likely to prove a most useful adjunct to the stimulants exhibited internally.

The class of cases in which the habitual use of alcoholic stimulants is most commonly, and in our opinion most perniciously, recommended by practitioners of medicine, is that in which there is chronic disorder of the digestive apparatus, with its multiform consequences. This disorder, in at least nine cases out of ten, has its origin in inattention to the laws of health, as regards diet, regimen, exercise, physical or mental exertion, and the like; and we cannot reasonably look for its cure by the use of stimulants. For the action of these, in such states of the system, is precisely like the application of the whip or spur to the horse already tired, which produces a temporary improvement in his pace, and prompts him to get through his work the quicker, but which leaves him, when he has done it, more fatigued than if he had taken his own time. We do not in the least deny that by men who are undergoing the excessive "wear and tear" of incessant and anxious mental exertion, the work is accomplished with more feeling of ease at the time, and even with less immediately consequent fatigue, when alcoholic stimulants are moderately employed. And upon such a system we find men going on month after month, and even year after year, without any obvious injury. But the time almost inevitably comes, when the overtasked system gives way; and long and difficult is then the process of restoration from its disordered state, as every medical man well knows. Now we are confident that when the exertion of the nervous system is greater than can be borne without the assistance of alcohol, provided due attention be given to diet, fresh air, out-door exercise, and sleep, the excess produces a positive injury, which is sure to manifest itself at some time or other; the use of alcohol only warding it off for a time, and preventing it from being at once felt. It is in renovating the system after such a course of long-continued ill-treatment, that we regard the hydropathic treatment as peculiarly effectual. We may keep our patient in town at his usual occupations, practise all kinds of experiments upon his stomach, recommend fat bacon or lean chops, prescribe blue-pill and senna-draught, or quinine and calumbo, and ring the changes upon all the wines and malt-liquors which the cellar can furnish, in search of one that shall be free from directly injurious consequences; but we shall not effect a twentieth part of the benefit which our patient will derive from giving himself a complete holiday, betaking himself to some agreeable spot where there is sufficient to interest him, but nothing to excite; promoting a copious action of his skin by exercise, sweating, and free ablution; washing out his inside with occasional (but not excessive) draughts of cold water, and trusting to the natural call of appetite alone, in preference to artificial provocatives. Let those who decry hydropathy witness the results of this method, as we have done, in but a few cases, and they *must* come to the conclusion, unless blinded by prejudice or interest, that water is better than wine, and that a hearty miscellaneous meal swallowed, with a vigorous natural appetite, is more invigorating

than the carefully selected and delicately-prepared viands to which the dyspeptic subject is compelled to restrict himself, and which he can only digest with the aid of a glass of sherry, or a tumbler of bitter ale.

The insensibility to the effects of various morbific causes, which the use of alcoholic stimulants induces, and the toleration of them which it thus permits, is one of the most fertile sources of subsequent disease. As in the cases just adverted to, if we are prevented from feeling the *immediate* consequences of our improper course, we flatter ourselves that we are uninfluenced by them, and we give to our wine, our spirits, or our beer the credit of the escape. But this is far from being the case. The enemy is only baffled, not dispersed; and although he lies concealed for a time, he only waits until his onslaught can be more effectually made. Bad air, insufficient and unwholesome food, impure water, foulness of the skin and garments, and similar departures from the strict laws of health, *must* exert their influence on the system, all the alcohol in the world notwithstanding; and it is one of the greatest benefits of abstinence that, by making these evils less endurable, it prompts the sufferer to seek a remedy. Let our readers refer to the account of the former condition of the great tailors' workshops in London, (Sanitary Report, 1842, pp. 99 et seq.), where the heat and closeness were such that on the coldest nights of winter large thick tallow candles melted and fell over with the heat, and fresh hands from the country fainted away; and where gin was taken at seven o'clock in the morning to get the strength up for the day's work, and repeated three or four times in the subsequent ten hours: and then look at the consequences upon the health of the men, whose average age is not above thirty-two years, owing to the large mortality from consumption; whilst, at fifty, they are considered as superannuated. We have here an example that speaks strongly for itself. And applying this result to other cases, we think it will be admitted that when the *tolerance* of such nuisances as exist in the dwellings and workshops of our labouring population depends upon the use of fermented liquors, it is nothing else than an unmixed evil.

We might have added much upon other topics connected with the therapeutic use of alcohol; but our limited space compels us to leave it with only one observation. The whole medical art is based upon experience; and the value of any remedy can only be fairly tested by the omission of it in some of the cases in which it has been reputed to be most successful. Nothing can be stronger than the reputation which alcoholic stimulants have acquired, as affording efficient aid in the maintenance of the bodily strength under circumstances calculated to exhaust it; and yet the most unimpeachable testimony has shown the fallacy of this opinion, and has put "universal experience" quite in the wrong. So it has sometimes happened that medical men have assured staunch teetotallers that they would die unless they admitted alcohol into their system as a medicine; but the patients, being obstinate, did neither, thus falsifying the prediction in a very unexpected measure, and proving that the experience of doctors is not more infallible than that of the public.

We should gladly, also, have discussed the question, whether the sudden and complete disuse, or the slow and gradual diminution, of the allowance of fermented liquors, in cases where intemperate habits are to be reformed, is the least injurious to the constitution. But this, too, we must dismiss

with the brief observation that, considering the large number of habitual drunkards who have adopted the former course, the number of cases of delirium tremens that have occurred in consequence has been marvellously small. If the moral strength could be relied on to adopt the more gradual method, we should consider it as safer on the whole; but in those who have been subjected to the degrading influence of frequent intoxication, and have acquired that craving for liquor which must be regarded as constituting a diseased condition, every taste of the forbidden gratification occasions a fresh conflict with the better nature, to which it is most dangerous to subject it, and the shortest method is generally the safest.

We now commend this important subject to the best attention of our readers. We speak as unto wise men; and we ask for nothing but a candid and dispassionate hearing.

ART. XVIII.

1. *Contributions to Clinical Medicine. On Pericarditis. In Lancet from May* 17, 1845, *to October* 31, 1846. By JOHN TAYLOR, M.D., Physician to the Huddersfield Infirmary, late Professor of Clinical Medicine in University College, London, &c.
2. *On some of the Causes of Pericarditis, especially Acute Rheumatism and Bright's Disease of the Kidneys, &c. Medico-Chirurgical Transactions,* Vol. XXVIII, 1846. By JOHN TAYLOR, M.D., &c.

IN 1826, when M. Louis produced his well-known essay on Pericarditis, it was matter of notoriety that the diagnosis of that affection was utterly unestablished. Laennec had given up the problem in despair,—he had sometimes *divined* the existence of the disease, he had never *diagnosticated* it. The inquiries of M. Louis threw no small light on the mysteries of pericarditis, as they had done on those of so many other diseases (*nihil tetigit, quod non ornavit*), and multitudes of zealous observers have, during the last twenty years, added their contributions, trifling perhaps in the state of isolation, but in that of combination forming a very imposing mass of solid information. And how, and to what degree, have the certainty and facility of the diagnosis of the disease been advanced by these labours? Where, at the moment we write, stands pericarditis in the scale of disease, which men can during life measure in their extent and in their amounts with *certainty*, facility, and dispatch? Holds it a high place, a low place, or a variable place? Were we to answer from our own experience, we should echo the term *variable*,—and affirm that pericarditis is at the present hour a diagnostic anomaly, from the facility and perfection (partaking almost of the marvellous) with which it may in all its forms, offsets, and minutest modifications be diagnosticated in some instances, while in others (even where the disease is fully developed, and where it is looked for) it escapes detection altogether. In the present place it is enough to state this proposition; the opportunity for illustrating it will occur more than once in connexion with various facts which we shall presently be called upon to pass in review.

But if the historian of pericarditis may find himself (presuming him to be a specimen of that too rare character—an *honest* medical historian) loading his page perforce with numerous names — names of the first

noticers of this murmur and of that bulge, of this ache and that palpitation—he will unquestionably, on the other hand, discover that to the name of a single individual alone may legitimately be attached the honour of having thoroughly worked out the disease in all its phases and bearings—and that individual is Dr. Taylor. Dr. Taylor, having been for a considerable number of years closely engaged in clinical observation, found himself at length possessed of a mass of recorded cases of various maladies, and deemed the time had come when a profitable analysis of the whole might be made. He selected pericarditis as the disease to commence with, and after due consideration of the guise in which he might most profitably appear before the public, fixed upon the course of proceeding adopted in the contributions we are about to notice. He determined upon publishing separately all the cases of the disease which he had recorded, accompanied with comments upon their leading features,—the collection to be followed by a numerical analysis of the whole, adapted to exhibit the inferences fairly deducible therefrom in respect to the symptoms, diagnosis, causes, and other leading phenomena of the disease. Now there are various reasons which led Dr. Taylor (and others who, like ourselves, agree with him) to believe that this method of *publishing* the details in full of cases, which are to serve as the basis of general inferences, the only just and proper one—where it *can* be pursued. In the first place, conclusions drawn from the review of a number of cases, instead of being taken upon trust, are capable of being verified; and additional questions, such as never occurred to the author, may be answered by other persons who may subsequently engage in similar investigations. Secondly, the character of the cases will furnish a test of the confidence to be placed in the man who records them, and would teach us by their analysis: there will of necessity appear in their narratives evidence of his honesty or dishonesty, his fitness or unfitness to observe, &c. In the commentary on each individual case, there is, in the third place, an opportunity afforded of introducing observations on numerous subsidiary points, which could not so well be made the subjects of consideration in a general analysis. Many other sound reasons (and others yet further we could ourselves urge) may be found in Dr. Taylor's pages exhibitory of the importance of full detail in the published narratives of cases. But it is useless to dwell on these; the broad fact is this, that true practical medicine being confessedly capable of sure advancement by but one solitary means (slowly, ever, it is true), namely, by minute observation and numerical analysis, the public display of the elements of that analysis cannot be too constantly nor too completely effected.

The cases of pericarditis, which Dr. Taylor has published, are forty in number; all (with two exceptions) are accompanied with commentaries such as just alluded to, but no *analysis* of the whole has yet been printed by their author. Our task, therefore, in working out some of the results to which these cases lead, will be by no means a light one; but in the interest of our readers we cheerfully enter on the labour.

Friction-sound. It is a fact familiarly known that pericardial friction-sound may very closely simulate valvular murmur, and several examples of the difficulty of distinguishing the two things occur in Dr. Taylor's cases. Indeed Dr. Taylor admits that there are conditions under which

he has been unable to determine the true seat of the morbid sound; thus we find him in one instance saying: "On Nov. 12th and 25th, the characters of the sound at the base of the heart were such, that it might have arisen either from some slight remains of the pericarditis, or from a trifling amount of inflammation supervening in the aortic valves. If the cause last named were the true one, then it is worthy of remark that the sound was not (probably on account of its feeble intensity) propagated into the arteries of the neck." In another case, where the *character* of a "double, continuous, rough murmur" was considered insufficient to localize it in the pericardium, rather than in the endocardium, its pericardial origin was inferred from the facts that "it was heard extensively, and loudest at first where valvular sounds are seldom loudest (fifth left costal cartilage), and afterwards changed its point of greatest intensity to opposite the third rib. This fact agreed best with the notion that the pericardium was the seat of the sound, and seemed to indicate either adhesion or, more likely, greater serous effusion." Besides "the morbid sound was very loud over the third rib, and scarcely audible at the top of the sternum—a circumstance which could hardly occur in aortic-valve disease." The event proved the correctness of the diagnosis. But there was in this case another remarkable circumstance, "a single rough bellows-sound, synchronous with the pulse, audible along each side of the vertebral column in the greater part of its length, but loudest on the right side opposite the fifth and sixth dorsal spines." Dr. Taylor thinks that this sound originated in the pericardium and not in the aorta, for the following reasons: "1. It was loudest about the fifth and sixth vertebræ, and therefore opposite to the heart. 2. The great *transverse* extent of the sound, without any evidence of disease of the lung to conduct it. 3. Its diminution (on the 24th of the month) when the friction-sound in front diminished, and when there was evidence of increased serous effusion into the pericardium. 4. Its cessation with the friction-sound on the 30th." A difficulty on the face of this explanation is that this sound behind was *single*, while that in front was *double*. "Perhaps," suggests Dr. Taylor, "the single sound depended on a fibrinous exudation on the posterior surface of the pericardium, so disposed as to occasion a friction-sound chiefly with the ventricular systole; considered in itself, the sound resembled that occurring in disease of the aorta, much more than the rubbing sound of pericarditis; but independently of the considerations adduced above, it may be presumed that it would have been less likely to cease in the former than in the latter case."

The *creaking* quality sometimes assumed by pericardial friction-sound, has been commonly set down as significant of stretching of adhesions. Cases collected by Dr. Taylor exhibit the error of this notion. In one of these "the serous surface of the pericardium was everywhere covered with soft lymph, and there was no point of adhesion. The roughness of this lymph was so trifling, as to be barely visible, but it was most distinctly appreciable by the finger passed over it." In another instance, at the very time that friction-sound became *creaking* in character, there were noticed signs of a moderate amount of serous effusion into the pericardium. From this it would follow that *much pressure* of the opposed surfaces is not required for the generation of this particular variety of rubbing sound.

Dr. Taylor refers to but one example of the *crumpling* variety of this sound; it was here superficial, diffused nearly over the entire cardiac region, and lasted about twenty-one days.

The full development of friction-sound does not seem readily possible, unless both divisions of the pericardium are the seat of plastic exudation. A case appears among those set down by Dr. Taylor, in which no pericarditis was suspected during life, yet the "whole surface of the heart was covered with recent lymph," while the loose division of the pericardium was totally free from it. Now the heart had been examined during life, and a "trifling murmur with the first sound at the base" discovered. This was considered to be an obstructive aortic murmur; but the post-mortem examination proved that the aortic valves were healthy, and the author does not appear disposed to regard the murmur as anemic. Hence we would draw the inferences from this case that: 1st. Pericardial friction-sound (single and systolic) may be produced, but not to any great amount, by the rubbing of lymph limited to the cardiac division of the membrane, and this in the very situation (namely, at the origin of the great vessels) where motion exists in the least marked degree. 2d. Such friction-sound may be readily confounded with a systolic aortic murmur; means of distinction may readily be imagined, but as none were tested in the case, we will leave them all unstated. Now, in the inference which we have just drawn (No. 1), we are at variance somewhat with the spirit of Dr. Taylor's remarks on this point: his inclination is rather to insist on the source of fallacy arising from the almost total deficiency of friction-sound, when only one division of the pericardium is the seat of lymph-exudation. Perhaps this is the manner of viewing the point which is best calculated, generally speaking, to be practically useful; because it will teach the observer to abstain from a positive negation of the existence of pericarditis on the evidence of imperfect or (presumedly) deficient friction-sound. But, on the other hand, it is clear that the *ultimate truth* is such as we have stated it, and to *this* the clinical investigator should certainly point his aim.

Several of Dr. Taylor's cases prove the fact that a considerable amount of serous infusion does not necessarily prevent the occurrence of friction-sound at the same time. In one of these cases the rubbing sound was heard on the day of the patient's death, and eight ounces of serum were found in the pericardium.

In one instance friction-sound existed, disappeared, and returned again four days later. This disappearance of the friction-sound could not be ascribed to serous effusion, as no increase in extent of the heart's dullness could be detected. The return of it coexisted with an exacerbation of the rheumatism, under which the patient laboured at the same time.

Heart-sounds. The feebleness and deficiency of tone in the heart's sounds, well known to accompany serous effusion into the pericardium, are presumed to depend solely upon the deadening influence of the fluid enclosing the heart. But a case noticed by Dr. Taylor at least opens the question whether these alterations of the natural sounds may not depend, at all events in part, upon some coexistent condition. In this instance the alterations in question were noticed, though little or no serous effusion was present. It is suggested that "the first sound might be diminished by some modification in the contraction of the muscular fibres of the left

ventricle, consequent on the adjacent inflammation, or, at least, as much of the sound as depends upon muscular contraction; or both the sounds might exist, but be marked by the loud friction-sound."

Cardiac dullness. In various parts of his commentaries, Dr. Taylor insists very emphatically on the importance of percussion-dullness in the cardiac region as a sign of effusion. He observes very justly that its diagnostic importance is extensive, first, on account of the large number of cases in which it occurs; and secondly, because this sign is readily appreciable, and not unfrequently constitutes the only physical evidence of the disease to be found at the time of observation (that is, without waiting to watch the *progress* of the disease). But hypertrophy causes dullness also; how are the two kinds to be distinguished? Dr. Taylor replies that in the instance of pericardial effusion: "1, The dullness extends *upwards* to the level of the second rib or clavicle, and but little below the healthy limits *downwards;* 2, It extends further to the *right* side than that of hypertrophy of the *left* ventricle, and further to the *left* side than that of hypertrophy of the *right* ventricle; 3, It changes in its extent from day to day, and sometimes rapidly." These characteristics are more clearly and unmistakeably put by Dr. Taylor than by any other writer.

The dullness of condensed apex of the left lung, might be confounded with that of pericardial effusion. In a case illustrating this matter, the dullness was pronounced to be pulmonary, because, "although it extended up to the clavicle, yet it was *complete* only up to the third rib (as high as the heart reached); and *incomplete* above that point, as if the sign in these two situations depended upon two different causes; moreover, the transverse extent of the dullness was not so great as it ought to be in pericardial effusion of such amount as to reach the clavicle—its extension was less towards the right of the cardiac region (being more prolonged towards the left side, as in hypertrophy of the left ventricle), and it did not vary in extent from time to time." The diagnosis was confirmed by the fact that the dullness persisted after the cure of the pericarditis.

Two cases are referred to as proving the production of percussion-dullness over the upper and *back* part of the left side of the chest by pericardial effusion. In both of these, however, the left lung was in a condition to make it doubtful whether this might not have been the true cause of the dullness; at all events the narrations are not convincing.

Heart's impulse. The rule is, that where serous effusion exists, especially to any amount, in the pericardium, the heart's impulse becomes deficient in force. This rule would appear to suffer occasional exceptions; in one of the cases before us, in particular, it does not appear that any peculiarity was remarked in the impulse of the heart, notwithstanding the existence of copious serous effusion. Certainly no mention of diminution of impulse is made in the report of the case; but it would have been more satisfactory, had the natural state of the heart's impulse been made matter of affirmation. We do not ourselves remember to have seen effusion without modified impulse; and it is to be borne in mind that a positive opinion cannot be fairly acquired, unless when the patient has been examined both *before* and *after* the serous fluid has been accumulated. Dr. Taylor did not see the subject of the case in question (No. xix) until after the pericardial sac had undergone great distension with serum.

Undulatory movement. Much has been written and speculated con-

cerning "undulatory, or vermicular motion" accompanying the impulse in the cardiac region. Dr. Taylor's cases illustrate its occurrence in connexion with serous effusion, with enlargement of the heart, where no serous effusion exists, and with adhesion of the pericardium. Hence, they who maintain that such motion is specially significant of adhesion alone, err grossly: we have, within the last few days, observed one of the best specimens we have ever seen of this said vermiculation, in a case of pericarditis with serous effusion insufficient in quantity to materially alter the quality of the friction-sound. Much nonsense has indeed been put forth from high places concerning this matter; and we have more than once had evidence at the bed-side of one of its probable causes. People's minds are not made up as to what constitutes vermicular movement: some persons scarcely ever look at a cardiac region without fancying they see an example of it; others cannot perceive it where it absolutely and fully exists.

Tactile vibration. It is shown that vibratory tremor, when occurring at all, is of shorter duration than the accompanying friction-sound: a more powerful rubbing is required to render friction of membranes a tactile than an audible phenomenon. Tactile vibration is modified in amount, but not necessarily destroyed, by the occurrence of serous effusion. Where valvular disease coexists with pericarditis, and vibratile tremor is discovered, it may be difficult, on first view, to determine to which affection the tremor is due. But, if pericardial, it will accompany both movements of the heart, and will be coextensive with the friction-sound: temporary duration of the tremor will eventually prove the diagnosis. Vibratile tremor may exist in one and the same cardiac region, both of valvular and of pericardial origin; and it may be possible to distinguish them under favorable circumstances. Thus, in a case of Dr. Taylor's, a tremor existed at the apex in connexion with mitral regurgitation, and for some time was the only vibration to be felt. A month later the base became the seat of tremor in connexion with an attack of pericarditis; the friction-sound of this affection remaining after the new tremor had disappeared. But here the diagnosis was made through an *accident* of localization; suppose the pericardial tremor had arisen at the apex, how would it have been distinguished from the mitral, to which it was superadded? The distinction would have been impossible in the existing state of knowledge.

Cardiac bulging. Among the physical signs of pericarditis, revealed to observers in the essay of M. Louis, appeared bulging or vaulted form of the cardiac region. M. Louis ascribed it to the influence of serous effusion, believing that, previous to the occurrence of such effusion, it cannot exist. M. Gendrin has more recently attempted to show that the alteration of form in reality depends on paralysis (through adjacent inflammation) of the intercostal muscles, and may arise previous to effusion, and constitute one of the very earliest signs of pericarditis. We have looked carefully, in a certain number of instances, for evidence in support of M. Gendrin's view, and have as yet failed to discover it. On the other hand, too, it is certain that a good amount of effusion may exist without inducing any precordial bulging,—we know this from our own experience, and Dr. Taylor's Case xix gives additional demonstration of the fact. Further, hypertrophy of the heart is, according to our observation, a much more frequent cause of cardiac bulging than serous effusion. From all this, the inference follows that the absence of bulging is not to be looked

on as affecting the diagnosis of effusion, nor is its presence to be considered confirmatory of the existence of such effusion in a doubtful case, unless the bulk of the heart itself has been, within a short period, ascertainably natural.

Failure of diagnosis. The importance of the physical signs, which we have now, under Dr. Taylor's guidance, considered in certain of their characters, cannot be exaggerated. If this be esteemed too strong a statement, let us simply refer him, who thus judges it, to the fact that among Dr. Taylor's *forty* cases were *fifteen* at least in which the disease was revealed by these signs alone. On the other hand, precise and distinctive as these signs are, and easily as they are to be discovered, except under a concourse of peculiarly adverse circumstances, it is an unfortunate truth that pericarditis escapes detection occasionally during life; in *ten* of Dr. Taylor's forty *cases*, the existence of pericarditis was either not suspected, or, at least, not substantiated. Let us inquire into the reasons of this. The first general character traceable in these cases of failure of diagnosis is that they were all cases of *non-rheumatic* pericarditis,—hence of pericarditis arising under circumstances presumed to be less usual than certain others,—hence of pericarditis likely enough not to be inquired after or looked for by the physician. Was this non-inquiry after the disease, then, the invariable cause of its escaping detection? *A priori*, we should fear not (and the reason why we use the word *fear* is obvious), for Dr. Taylor frequently makes incidental reference to the fact that it is his habit to examine all the organs of individuals placed under his care. We should further fear not, because we must confess we have ourselves twice failed to detect the disease (under peculiar physical conditions of the individual, and under peculiar combinations of functional derangements of organs unconnected with the heart), though we had examined the cardiac region not uncarefully, and if not precisely under the impression of probably finding pericarditis, at least not under the stultifying influence of any preconceived idea of its absence. What, then, actually were the peculiarities of these ten cases of Dr. Taylor?

It appears, in the first place, that three of the ten were not seen by Dr. Taylor; the cases we have to account for, therefore stand as seven out of thirty-seven, and in all but one of these seven the cardiac region was examined more or less carefully during life. In the first of these (Case xxx) the pericardium was universally adherent by means of *recent* lymph, and this state of adhesion existed at the period of the patient's admission. In the total absence of *symptoms* to direct attention to the heart, the detection of this condition of the pericardium was utterly impossible; or rather it would have been impossible to affirm, admitting that the adhesion had been detected, to have pronounced this to have been of *recent* rather than of more or less *distant* origin. In Case xxxi there were ten ounces of bloody serum in the pericardial sac, and nearly the entire of both surfaces, cardiac and parietal, were covered with scabrous lymph. The patient in this case was admitted with the effusion, therefore no friction-sound was detected, though sought for. Dullness more extensive than natural existed in the cardiac region, but its characters were not carefully inquired into. The man had hypertrophy of the heart also; but no symptoms directing attention to that organ. Here, then, the observer's faculties were especially wakened to the possibility of the ex-

istence of pericarditis, yet in its most intense form it eluded discovery. In Case xxxii there were twelve ounces of serum in the sac, the walls of which were besides coated with soft lymph. Here no examination of the cardiac region had been made during life. In Case xxxv, the cause of failure was the same as in Case xxx; the laminæ of the pericardium were universally adherent at the time of the man's admission. In Case xxxvi the pericardium was greatly thickened; there were old adhesions in front, and at the back of the left ventricle, and at the apex; also recent adhesions on the right ventricle, while the remaining cavity held serum and a solid loose fibrinous coagulum. The subject of this case was an old man dying of vesical disease, and it seems possible that the pericarditis may have occurred during the several weeks preceding death, and during which (attention being engrossed by the urinary disease) the cardiac region was not examined. Or, on the other hand, it may be conjectured that the heart having really been examined, no friction-sound was detected,—or (it may be) produced, on account of the peculiar anatomical condition of the diseased sac. In Case xxxix friction-sound was mistaken for valvular murmur, the examination being carelessly conducted. In Case xl the pericardium contained about a pint and a half of flocculent serum. In this instance the patient had been transferred from the surgical wards for an attack of pneumonia occurring while he was under treatment for an injury to the knee. The diagnosis had been as it were settled, when the boy (for such he was) reached the medical wards, and was quietly accepted; the more readily as he was so excessively ill-tempered that force was required to make him submit to auscultation.

Looking then to these cases, we may draw the general inference that failures in the diagnosis of pericarditis are to be referred either to careless examination, or to the fact that either adhesion or abundant effusion has already taken place when the case falls first under observation,—each of these causes of failure being as it were collaterally supported by the total absence of cardiac *symptoms*. When adhesion has occurred at the period of the patient's admission, we believe it impossible to determine the fact of its *recent* origin (admitting that its actual existence has been satisfactorily made out at all); especially as there will in all probability be quite enough in the lungs, pleura, or elsewhere, to account for any acuteness in the character of the disease. As respects the detection of pericardial effusion developed fully at the period a patient, whose commemorative history is imperfect, and who labours under other disease also, first comes before us, we confess that (while we agree with Dr. Taylor as to the great significance of the *form* of the percussion-dullness, when the peculiarity of that form is well developed) positive affirmation does not appear to us, no matter how careful the examination, to be *always* possible or justifiable.

There are a certain number of cases in which all physical signs of pericarditis are deficient, though the anatomical state, apparently of necessity productive of some of them, is found after death. Three such cases appear in Dr. Taylor's collection, and each is referable to a different category. In one the deficiency of signs arose from the interfering influence of old adhesions of the pericardium. Upon this source of difficulty we have had occasion to comment in a former number (Vol. XXIII, p. 172, January 1847) when engaged in the examination of Dr. Latham's antithetic phrases on the subject. We then took occasion to observe that

though persuaded (as matter of common sense as well as of experience) that when old adhesions are close and universal, a new inflammation will give no physical evidence of its existence, we were equally persuaded from *actual observation* that a new pericarditis may, in spite of the past existence of an old one, give rise to sound referable to friction of the serous laminæ. And this for the simple reason that *complete* adhesion is not the invariable condition of a pericardium which has gone favorably through an attack of inflammation; *incomplete* adhesion (such adhesion as shall not utterly prevent gliding motion) is the rule. The murmur generated in such a *spoiled* pericardium may be deficient in true *friction* character it is true; but, as we have before said, "the position in which the murmur is heard, its instability in character as in precise locality, its non-transmission beyond the precordial region, its evidently superficial seat,—all these conditions will point to the pericardium as the seat of the new-made sound." And further (supported by experience acquired since the above sentence was first written) we would add (what does not appear to have been noticed by any of the describers of pericarditis), that although the quality of *friction* or of *creaking* may be utterly wanting in a given murmur audible in the cardiac region, still certain peculiarities in its *special character* may (quite independently of considerations of localization, propagation, &c.) suffice to fix the pericardium as its seat. In Dr. Taylor's second case of deficiency of friction-signs, their absence appears to have in all probability depended on the limitation of *recent* exudation to the posterior surface of the heart; the existence of old adhesions in front of the organ, may, however, have also played its part in the same direction. In the third case, lymph was effused on one only of the two surfaces of the pericardium; possibly the unaltered smoothness of the other surface may have prevented friction-sound; but the heart's action was here very feeble likewise.

Symptoms. We may now turn to the review of certain functional results of pericarditis. Orthopnœa is one of those concerning which writers have dwelt most complacently; and its existence, in connexion especially with tendency to syncope, immobility of the trunk, &c., has been set down as distinguishing cases attended with liquid effusion from others in which plastic lymph had alone been thrown out. Now we know from our own observation that this doctrine will not hold good at the bed-side; and Dr. Taylor supplies (in one direction at least) very precise and gratifying destruction of its provisions. In the first place, he gives seven cases in which there was liquid effusion without orthopnœa; in one of the number there were coexistent enlargement of the heart and pleural effusion. In the second place three cases are recorded in which there was orthopnœa, though no serous effusion had occurred in the pericardium. In the third place appear two singular cases in which orthopnœa set in after, and not until after, the absorption of serum in the sac. In these latter cases Dr. Taylor suggests that the orthopnœa (which was paroxysmal and accompanied in one of them by spasmodic cough) was referable to the "irritative influence of the inflammation in the heart or lungs propagated to the spinal cord, and reflected to the lungs again." The orthopnœa and cough, under such circumstances, appear, he continues, "to belong to the same order of symptoms with the chorea-like motions of the limbs in other cases."

In connexion with this view of the occasional nature of the orthopnœa of pericarditis, it may be well to refer to Dr. Taylor's notions on the subject of spasmodic movements in pericarditis generally. As is well-known, numerous cases are on record in which individuals, having presented distinct apoplectic or epileptic symptoms, maniacal symptoms, tetanus or trismus, chorea or aggravated hysterical attacks, have been examined after death, and found in possession of healthy brains and medullæ. In all of these cases the pericardium was actually inflamed; in half at least of them with rheumatism, and in the rest without rheumatism. Now it is matter of accredited doctrine that in such cases a certain influence is transmitted from the inflamed pericardium "to the nervous centres, and reflected upon the motor roots of various parts." In commenting upon a case of pericarditis attended with such phenomena, Dr. Taylor speaks as follows:

"The case before us shows that a different explanation of some of these cases may be suggested and supported, I think, by at least an equal amount of evidence. 1. There had been cramps in the legs, and occasional loss of memory and double vision, before the pericarditis appeared; this disease could not, therefore, be the cause of the symptoms in question. 2. The coma and other recent nervous symptoms would all have been referred, without hesitation, to the renal disease" [Bright's disease existed in the case], "if no pericarditis had been found. 3. I have elsewhere (Med.-Chir. Trans. vol. xxviii) attempted to show that the mass of cases of non-rheumatic pericarditis is due to renal disease. 4. I believe it will be found that a large proportion of cases of non-rheumatic pericarditis is quite latent, in the ordinary acceptation of the term, and also that the mortality is much greater than among the cases of rheumatic pericarditis. I therefore regard the coma and the pericarditis, in this case, as the common effects of the renal disease, and I believe them to be altogether independent of each other. I further believe it to be probable that the pericarditis and the nervous symptoms stand in the same relation to each other in a certain proportion of the published cases referred to. One or two particulars may be pointed out, either confirmatory of this view, or consistent with it. (1). In no one of the cases collected by Dr. G. Burrows (Med. Gaz. May, 1843) has the state of the kidneys been expressly mentioned after death, or the presence or absence of albumen been noted during life. In several of these, indeed, it is stated that there was no morbid appearance in any other organ than the pericardium, but all who are practically acquainted with the way in which bodies are inspected, and the appearances recorded, will know that such a statement affords hardly the least proof that the kidneys were even seen, much less that they were so examined that they might be pronounced healthy. (2). The mortality was twice as great in the non-rheumatic as in the rheumatic cases" [collected by Dr. Burrows]; "in which respect the two classes resemble, respectively, cases of rheumatic and renal pericarditis. (3). All the cases with apoplectic symptoms occurred among those without rheumatism; and one of the patients is stated to have had general dropsy. One of the rheumatic cases having ended in coma. Two of the three tetanic cases belonged to the non-rheumatic class. From this comparison, therefore, of my own cases of renal pericarditis, and those of non-rheumatic pericarditis, I think there is much probability in the supposition that the kidneys were diseased in some, at least, of the cases of the latter class."

We have transferred this passage in full to our pages, because it not only refers to a most important point, and one concerning which we perfectly and fully coincide with the learned writer, but because it exhibits a fair specimen of Dr. Taylor's lucid and simple, unpretending style, and of his close, logical, and conscientious mode of solving clinical problems.

To the class of spasmodic symptoms Dr. Taylor refers great irregularity of the pulse (at one time 70, at another 120-130, and subsequently not to be counted, from its indistinctness and irregularity); presuming it to arise from "spasm of the heart, analogous to spasm of other muscles." To this notion it may be objected that pain is a concomitant of spasm in muscle generally, that there is no reason why it should not be present in spasm of the heart (and, indeed, some excellent observers hold that the essential and sufficient cause of the paroxysm of angina pectoris is none other than this same cardiac spasm), while in the instance referred to there was none present. The heart in this case was excessively soft and dilated,—a state of things which has been found consistent with frequent and irregular pulse under various circumstances; but it of course remains to be determined in what manner such softness generates the peculiar condition of the heart's action.

Taking Dr. Taylor's cases together, the pulse is found to have been very often regular, sometimes rather full and jerking, sometimes irregular or intermittent for a time, and sometimes thready. Thread-like character of the pulse, observes Dr. Taylor, "I suppose to be due to the mechanical compression of the left ventricle (by effused serum), and the consequent obstacle offered to the free entrance of blood into its cavity, as well as perhaps to the free contraction of the muscular fibres. . . . . In respect to its diagnostic value, however, I may remark, that I have observed cases of pericarditis with serous effusion, in which this smallness of pulse was not present; and also, that the same kind of pulse is of frequent occurrence in various organic diseases of the heart without any pericarditis, as well as in other diseases not implicating the heart at all, except functionally." Then why, it may be asked, not consider it a functional, rather than a mechanical effect of pericarditis, where it accompanies this disease?

Remarking upon the occurrence of sudamina in the last stage of a case of fatal pericarditis, and the alleged connexion of sudamina generally with profuse perspiration, Dr. Taylor observes that, "in the present case no eruption was noticed when the sweat was profuse; and when the former did appear, the perspirations had ceased to be remarked. It was most abundant near to the blistered surface, as if the local irritation had something to do with the production of it." He is disposed, with the majority of persons, to refer the occurrence of sudamina to some peculiar condition of the system.

The formation of adhesions of the pericardium (or of agglutination of the two laminæ) is illustrated by several of Dr. Taylor's cases—five at least. From the remarks on these cases we learn that the speaker calculates on fixed position of the heart's apex and undulatory movement (especially if this be very well marked and constant) as diagnostic of the anatomical condition in question. But such diagnoses are generally to be made on the dead subject only. It has been our fortune to meet with two individuals possessed in their own opinions of a peculiar faculty for the diagnosis of "adherent pericardium;" and they were certainly both remarkably successful. Their success, however (it may be as well to add), uniformly consisted in detecting adhesions when the post-mortem examination proved them to be absent, and in failing to discover them where they really existed. There is one point ingeniously and neatly made out by Dr. Taylor in connexion with this matter, which we may not pass over

without notice. It would seem that where hypertrophy (or rather enlargement generally) of the heart co-exists with pericardial agglutination, the existence of the latter modifies (or may modify) the position of the heart in such manner as to enlighten its own diagnosis. In commenting on a case of the kind, the Professor observes: "Notwithstanding the great volume of the heart, the apex was not lower than usual, nor further towards the left side, and the base was something higher. This is certainly not what generally happens in enlargement of the heart without adhesion of the pericardium, in most of which cases the apex is either depressed or displaced laterally, or both." Now this seems a fitting place to put a problem in diagnosis to Dr. Taylor, to which we should be glad to have his answer. Given, a middle-aged subject, stupid and unable to supply a trustworthy account of himself, even as to the precise time he has been suffering, evidently labouring under some affection of the heart or its appendages, which affection is unattended with local pain or palpitation (or, in a word, with any symptom connected by the patient himself with the cardiac region); the subject is flabbily and abundantly fat in person; the impulse of the heart is very feeble; as well as can be distinguished (for this is very obscure), the apex-beat corresponds to the upper edge or posterior surface of the sixth rib on the vertical level of the nipple; the precordial dullness is considerably more extensive than natural, and at the base rises above the left third cartilage, reaching the second intercostal space certainly, and possibly [with two ? ?] in a minor degree the second left cartilage; the heart's sounds are feeble and toneless; there is neither endocardial nor pericardial murmur, and undulatory movement cannot be traced. Now, here is a picture (which we know Dr. Taylor's practised mind will at once recognise as clinical, real, and unexaggerated in its difficulties), and we ask him how he would, under such circumstances, determine whether the patient was the subject, on the one hand, of soft, flabby (it may be fatty) enlargement of the heart, combined with agglutinated pericardium, or, on the other, of more or less recent hydro-pericarditis? Dr. Taylor may say, the form of the dullness will settle the point; if so, we demur: for we deliberately affirm that, when an enlarged heart is adherent, not unfrequently will its dullness be found to extend upwards quasi-pyramidically in most puzzling propinquity to the second left cartilage. In rare cases the same difficulty will arise (probably through a loaded state of the right auricle especially) independently of the existence of adhesion. Again, it may be said, that were the case one of hydro-pericarditis, the obscure apex-beat would be discovered on a higher level than that supposed. Now we are quite aware (for we have more than once seen the thing) that it is amongst the tendencies of accumulation of liquid in the pericardium to twist the apex somewhat upwards, and so *raise,* instead of depress, the point of the apex-beat: but this is a physical sign, to the absence or contradiction of which we are positive Dr. Taylor would agree with us in not attaching any serious importance, so uncertain is its signification in (at all events) the present state of knowledge.

Course of the disease. The fact that pericarditis occasionally undergoes temporary suspension in its course receives full illustration from the records of Dr. Taylor. One case is referred to where circumstances arose

lending some plausibility to the notion that a suspension of twenty-three days' duration occurred; but these circumstances, we would observe, do not altogether exclude another and more simple explanation.

The morbid changes which pericarditis, cured symptomatically, inflicts upon the heart itself, are by no means to be despised. The chief are (as enumerated by Dr. Taylor) hypertrophy of the left ventricle, dilatation with hypertrophy of the same ventricle, and atrophy of the heart generally. In commenting upon a case where great dilatation of the left ventricle co-existed with comparatively very slight hypertrophy, he observes: "In the present case I see no cause for the great dilatation, compared with the thickness of the ventricle; but the close adhesion of the pericardium to the heart and to the ribs would tend to prevent the walls of the ventricle from approximating towards the centre of the cavity. This supposition, however, would lead us to expect some, if not an equal, degree of dilatation, of the right ventricle, which yet was not observed to exist." Not having ourselves distinctly seen atrophy of the heart as a consequence of pericarditis (under circumstances when the pericarditis alone could have been its cause) we turned with considerable interest to Dr. Taylor's evidence and commentary thereon. But his evidence is deficient beyond all question. Other persons have published statements of the same character as Dr. Taylor's; but it has, on the other hand, been affirmed by certain pathologists (crotchety ones, it may be, but withal experienced) that cardiac atrophy is never produced by the embrace of pericardial false membranes.

Among the morbid appearances produced or left in the pericardium itself there are some which Dr. Taylor illustrates in a manner requiring notice.

Thus, in a case of pericarditis, apparently very rapidly fatal, "the pericardium externally was quite dry and was transparent, like oiled-silk;" and the writer observes, that he has repeatedly met with this condition without being able satisfactorily to explain its mode of production. Dryness, however, as an early effect of inflammation of membranes is a well-known condition in those of the serous class; it is the cause of the earliest physical sign (grazing friction-sound) of pleurisy, and is frequently observed in the arachnoid. In membranes of the mucous class its occurrence is exemplified by coryza. It is certainly curious, however, that in the case of dry pericardium particularly referred to now (No. vii), Dr. Taylor is disposed to believe that there had been tolerably abundant effusion present during life. No such effusion was nevertheless found (two drachms constituted the whole amount); Dr. Taylor supposing that it had been absorbed during the twenty-four hours preceding death.

The author's Case xxvii gives him an opportunity of discussing the question whether pericarditis is curable without adhesions. In this instance the signs of that inflammation existed during life; the post-mortem inspection revealed "not more than three or four drachms of serum in the pericardium, and that not turbid; great redness of the serous or subserous tissue of the visceral pericardium, a little over the anterior, but chiefly over the posterior, surface of the heart; the redness was composed of fine blood-vessels, and was not uniform as in mere staining by imbibition; no loss of smoothness or transparency; a few spots of rather firm

false membrane appeared in the course of the anterior coronary artery, easily scraped off with a scalpel." Now upon these conditions the lecturer thus comments:

"Looking at the morbid appearances apart from the symptoms, it might be disputed whether inflammation of the pericardium had existed. The redness, however, had the characters of that produced by inflammation, as far as such characters can be considered to be distinctive. The unequivocal double friction-sound, with nothing else to explain it, appears a decisive argument in favour of the same view. The friction-sound, then, may be produced by inflammation accompanied by mere redness and without effusion of lymph, or otherwise the inflammation must have been in process of cure, and the effused lymph had been absorbed. Seeing that the sound had disappeared for several days before death, it is probable that the latter is the true view. Upon either supposition the case proves that pericarditis, attended with a friction-sound, may be cured without any adhesions, and probably without any other traces of the disease existing."

The modes of death dwelt upon by Dr. Taylor as distinctly connected with the nature of the disease are by sudden suspension of the heart's action and by gradual failure in the power of the left ventricle. Of the former mode of death, not uncommon in heart diseases generally, Case xix furnishes a very distinct and instructive example. In this instance, as in others which have fallen under our own notice, the sudden cessation of the heart's contractions was caused by abrupt change of position on the part of the patient.

The post-mortem examinations of Dr. Taylor are recorded with such care and minute accuracy, that their analysis could not fail to throw great light upon the concomitant alterations of textures and organs (whether these precede as its causes, accompanying as its coincidences, or as co-effects of some primary cause, or follow as its results) found in the victims of pericarditis. In this particular branch of the subject Dr. Taylor's inquiries stand absolutely alone; and we must express (this is not a mere conventional trick to get rid of the matter, but the announcement of a fact and a verity) our sincere regret that the bulk of our Number prevents us from accompanying the learned author *seriatim* through this division of his subject. Let us content ourselves with an enumeration of the various morbid states observed, and some commentary upon a few of the number. These states, then, were: acute endocarditis, acute carditis, acute pleurisy, chronic pleurisy, pneumonia, acute bronchitis, meningitis, softening of the brain, hemiplegia, acute peritonitis, erysipelas, phlebitis, inflammation and ulceration of the colon, enlargement of the heart, temporary sanguineous engorgement of the heart, old valvular disease, disease of coats of aorta, tubercles in the lungs, emphysema, asthma, œdema of the lungs, serous effusion in pleura, anemia, cirrhosis of the liver, atrophy of the liver, enlargement of the spleen, ascites, anasarca, Bright's disease of the kidneys, chronic ulceration of the stomach, encephaloid cancer of the bladder, &c. Every one of these morbid states receive very full illustration in respect of its diagnosis and symptomatology (more especially in regard to connexion with pericarditis) at the hands of Dr. Taylor.

First, of endocarditis. We have in a previous number (No. XLV, p. 173, January 1847) given in full a passage from Dr. Taylor's paper in the 'Medico-Chirurgical Transactions,' wherein he discusses the question of the possible distinction of the date of an endocardial murmur in a patient seen for the first time, and without cardiac history which can be come at.

We have there shown how Dr. Taylor's experience fails to meet the most difficult part of the subject (and since we wrote the page in question, new difficulties, under a combination of circumstances hitherto unrecorded, have arisen to ourselves, giving yet a further dash of insolubility to the problem), while at the same time we gave him due praise for the excellence of his inquiries, so far as they go. We beg the reader interested in Dr. Taylor, and in men of his stamp of mind, will look back to the Number referred to. But this same endocarditis serves as Dr. Taylor's thesis for more than one interesting discussion. Here, for example, in Case xxx: a woman was cut off with various acute and some chronic ailments; of the former (with which only we have to do) were lobular pneumonia, endocarditis, and pericarditis. The lecturer observes:

"This form of pneumonia, and perhaps this exclusively, is found in cases of purulent infection of the blood, consequent on phlebitis, or occurring in other circumstances. The pus-globules are perhaps arrested mechanically in the capillary vessels of the lungs, and there excite inflammation. Hence the disseminated character of the inflammation in the first instance.....These considerations led me to look for some such cause of the pneumonia in this case, and a probable one may be found, I think, in the acute endocarditis seated in the left auricle. Granules of lymph were there found, pus also may have been secreted, and the particles of one or other of these morbid products may have been carried with the current of the blood to the lungs, and there have been arrested."

As far as we are aware, Dr. Taylor is the first propounder of this doctrine, and it is one of no mean importance. He has himself raised a difficulty in its admission in respect of this particular case, namely, that no pus was discovered, though it *may* have been present. But we can get over this difficulty for him. In truth, it is not necessary that *pus* should actually be present as a result of a local inflammation capable of contaminating the blood, and therethrough giving rise to secondary abscesses; the dynamic changes in the fluid, coupled with the circulation of solidified lymph-particles, seem in the existing state of knowledge to be (in all human probability at least) capable, under favouring conditions, of working out the whole series of phenomena ending in peripheral pus-formations. As regards the further evidence, deducible from Dr. Taylor's cases, there are two other examples of fatal endocarditis; in one of these (Case xix) there was lobular pneumonia; in the other (ii) the lungs were not examined. Of seven cases of endocarditis, not fatal, four were attended with pneumonia; in the remaining three (the endocardial inflammation was slight) there were no signs of pneumonia.

In 17 of 38 cases there was acute pleurisy. It is notable that it occurred most frequently in *non*-rheumatic pericarditis; there were 10 examples of pleurisy in 16 cases of this species of pericarditis (62·5 per cent.); 7 in 24 cases of the rheumatic species (29·1 per cent.) The same peculiarity is to be noted in respect of pneumonia: 12 examples of this inflammation occurred in 24 cases of non-rheumatic pericarditis (50 per cent.); 4 examples in 16 cases of the rheumatic species(25 per cent.)

Most valuable remarks appear in the commentary on Case xxiv, on the subject of the causes of hypertrophy with dilatation of the left ventricle. These remarks convey negation of knowledge rather than add to what is presumed to be known; they show that much which is admitted to be certain is problematical, and much which is admitted to be probable

is absolutely groundless. But commend to us the man who, like Dr. Taylor, rejoices in such negatives;—can it for a moment be doubted that *no* knowledge is better than *false* knowledge? Thus Dr. Taylor knocks down from its etiological pedestal *diminished elasticity of the coats of the aorta;* certain "physiologists" would make a hard push we know for the restitution of the said diminished elasticity, but if clinical observations say "No," who will be daring enough to say "Yes?" Never has Dr. Taylor known enlargement of the heart "which could be clearly ascribed to *mere nervous excitement* of the organ, however long continued." He most justly affirms that the notion of such enlargement being caused by *unusual muscular efforts* requires to be supported by very different evidence from that which has hitherto been adduced in its favour, before it becomes entitled to the least confidence. Dr. Taylor has no evidence that *habits of intemperance* cause the affection. Again, it is believed that *Bright's disease* acts as a cause of hypertrophy; but the lecturer, wisely calling to mind the numerous instances of advanced disease of the kidney of the kind referred to, in which the substance of the heart is not increased, holds that the influence in question is as yet unestablished. Dr. Taylor thinks that a certain amount of *obstruction in the lungs* may be a cause of enlargement of the left ventricle, as it certainly is of the right. "When the blood," he observes, "for a long time or habitually, cannot pass freely through the lungs—as in emphysema for example—it accumulates in the right cavities of the heart, and causes them to become dilated and thickened. But this obstruction must also be reflected on the capillary circulation; the right cavities of the heart being full, the blood cannot flow into these from the veins, and the veins thus in a state of engorgement cannot receive the blood from the capillaries; hence the necessity for increased efforts on the part of the left ventricle." These ideas are admitted to be speculative, and only to be estimated by an appeal to a large body of facts; it is certain, meanwhile, as Dr. Taylor himself shows, that the left ventricle is sometimes atrophous in cases of emphysema. Hence to revert to our starting-point, the influences really producing hypertrophy of the left ventricle are as yet enveloped by a sufficiently dense cloud of mystery.

In Case xxxvi there was some reason to think that the heart had at one period been in a state of hypertrophy, which had disappeared at the time of death. The case would go then to prove that hypertrophy may be cured by general reduction of the body—for in this instance, the individual had been the subject of cancer of the bladder, and had gradually wasted in mass to a very notable degree. "Could the same result be produced by artificial means? Probably it could (though I have never seen it)." Such are the query and response of Dr. Taylor on the question of the curability by art of hypertrophy of the heart: theory whispers the soft delusion, that the thing is possible; experience cries aloud the hard verity, that the thing is utterly impossible. Which shall we believe? We have shown in a previous Number (January, 1847, p. 180) what Dr. Hope, who represents theory, and Dr. Latham, who represents experience, say on this question: Dr. Hope never failed almost in curing the disease; Dr. Latham never succeeded or knew another to succeed! Verily Dr. Hope was an exquisite specimen of stolid egotism *quoad therapeia;*—and we could name shoulders whereon his mantle hath descended, and sitteth in folds which the wearer judgeth to be full of grace

and dignity,—but it is more to the purpose that we record our own experience as being accurately in accordance with that of Drs. Latham and Taylor.

Passing over a mass of materials well worthy of study, we come to the consideration of the treatment of pericarditis; and the first general result of Dr. Taylor's experience on this head is, that the pericardial inflammation attendant on rheumatism is much less fatal, than that which occurs without rheumatism. If, then, one of the class of *ex professo* curers of disease—men privileged above their fellows—raise his modest boast of the success with which *he* treats pericarditis, just ask him (in all probability he will be unable to answer the query, for your true therapeutist soars in a region where such petty matters are unthought of), nevertheless, ask him, whether his cases were of rheumatic or non-rheumatic pericarditis. And then quietly inform him, that pericarditis of all kinds indiscriminately, and pericarditis left utterly to itself—*untreated and uncared for*—does not prove fatal in more than one in six cases.* And how will your therapeutist reply, and how will he act? Why he will utter curses not loud but deep on all statistics and numerical methods that ever were created; he will continue to believe (or to feign to believe) in his providence-sent prowess, and hold forth yet more energetically than ever to gaping youths concerning the all-potency of art in warding off, staying the progress of, or curing completely all manner of evils that flesh is heir to. But no matter for all this; let sober men continue to register experience, albeit that experience tell sad tales of the powerlessness of art; let them be assured that it is only when the amount of that existing powerlessness is generally felt and understood, that improvement in therapeutics may rationally be looked for.

The next general inference deducible from Dr. Taylor's recorded cases, is illustrative and confirmatory of the tenor of the remarks we have just made. In four cases pericarditis got well without treatment, being in one of these instances complicated with dysentery, and in another with endocarditis. In a fifth case the pericarditis, which got well under treatment, would probably have got well without any, seeing that the patient was not submitted to any until the twelfth day of the disease. Dr. Taylor comments, as follows, on the case (xxx) of pericarditis associated with dysentery above referred to:

"It deserves to be remarked that the pericarditis certainly, and the inflammation and ulceration of the colon very probably, had escaped detection, and consequently had not been treated. Both diseases had been very severe; yet both, I think, had been cured by the powers of Nature. The patient died, but, as it appears to me, of another disease; and I think there is every reason to believe that, notwithstanding the absence of treatment for the inflammations in question, life would have been spared, although perfect health could not have been restored. Such cases are very common. I have repeatedly seen pneumonia (one of the most fatal of all internal inflammations) and pleurisy terminate favorably without any active treatment, their existence having been overlooked until their course was nearly run. Surely these and similar facts should teach us a little more caution in assigning so large a part to ourselves, and so small a part to Nature, of the credit that is attached to the favorable termination of cases of even severe inflammatory diseases."

The more important plans of treatment illustrated by Dr. Taylor's cases

* Louis, Mém. Anat. Pathol., p. 289. Paris, 1826.

are those by general and local bleeding, and by mercurials. We do not find anything very plainly novel brought out in connexion with the two former; but the inferences drawn by the learned author concerning the power and influence of mercury, are most singularly valuable. On the general subject of mercurial treatment in membranous inflammation of the heart, we have spoken in the Number of this Journal already more than once referred to. We there submitted the statements and facts published by Dr. Latham on this question to examination;—and it proved an easy task, not only to show that that physician's *general* high estimate of the powers of the mineral was not borne out by his *particular* facts, but that he himself, in the course of his examination of those facts, was in common honesty compelled to soften somewhat the tone of almost worship with which the influences ("antiphlogistic and reparatory") of mercury were in their general bearings set forth. In the present place, then, we shall simply lay before the reader a series of inferences, supplied by Dr. Taylor's cases, illustrating the utility or non-utility of mercury in pericarditis, and some other inflammations.

1. Ptyalism was not followed by any abatement of the pericarditis in *twelve* cases (i, ii, iii, v, vi, xi, xiii, xiv, xv, xvii, xxii, xxiii).

2. In *one* case ptyalism was followed by speedy relief (vii).

3. In *two* cases (ix, xxvii) ptyalism was followed by a diminution, and then gradual cessation of pericardial murmur.

4. In *one* case pericardial murmur had been diminishing for some days before, and it ceased soon after ptyalism was produced (xvi).

5. In *one* case pericarditis and pneumonia both increased in extent and intensity after ptyalism (xxi).

6. In *four* cases pneumonia supervened after the establishment of, and therefore was not prevented by, ptyalism (iv, v, xii, xv). [Was it CAUSED by it?]

7. In *three* cases endocarditis supervened after ptyalism (v, x, xiv).

8. In *six* cases ptyalism was followed by pericarditis (xi, here the mouth was very sore at the time, and the pericarditis continued sixteen days; xii, xxi, xxiii, here the two things were nearly of simultaneous origin; xxiv, xxvii).

9. In *one* case ptyalism could not be produced, and yet the pericarditis went on favorably (xx, mercury was given for nineteen days).

10. In *two* cases ptyalism was followed by extensive pleuritis (xxiii, xxiv).

11. In *one* case ptyalism was followed by erysipelas and inflammation of the larynx (xxvii).

12. In *two* cases rheumatism continued long after ptyalism was produced (xvi, xvii).

The importance of these propositions need not be insisted on,—they very clearly show that current opinions on the powers of mercury require revision. The question of the issue of pericarditis treated with mercury, and of pericarditis not treated with mercury, cannot be solved by these cases for several obvious reasons; but these cases may be used as elements for its decision by future inquirers—Dr. Taylor's subscription in the cause. The same may be said of their applicability in reference to the question of the comparative influence of mercury in rheumatic and in non-rheumatic cases of the disease.

So far we have been unassisted, in our examination of Dr. Taylor's cases, by any published analysis of them by the author himself. It had been his intention to undertake this labour (and unquestionably upon a more minute and extended plan than that followed in the preceding pages by ourselves), but circumstances appear to have occurred preventing its fulfilment. The author has, however, published an analysis of his experience in respect of a special question, the etiology of pericarditis,* and of this we proceed to lay a brief notice before our readers,—premising that modern medical literature scarcely to our knowledge (and we have full cognisance of the labours of Louis and his followers) contains a single Essay which may reasonably be said to vie with this in closeness of argumentation and precision of induction.

The author does not profess in this paper to investigate *all* the causes of pericarditis. He has first inquired what were the causes actually observed in all the instances of the disease which had fallen under his notice; and, in the next place, investigated more in detail the frequency, both absolutely and relatively to each other, of each of these observed causes, as well as some other of the circumstances connected with them. And, finally, he has examined, incidentally, the influence of the same causes in producing inflammation of other internal organs, both in connexion with and independently of pericarditis.

The cases of acute and severe pericarditis, examined in the paper and added in a note, are 38 in number. Of these

20 occurred in the progress of acute rheumatism;

1 was probably rheumatic;

10 were complicated with Bright's disease;

1 was complicated with some renal disease and empyema;

1 was complicated with some renal disease, but probably not Bright's disease;

1 was complicated with double chronic nephritis and encephaloid cancer of the bladder;

2 *may have been* complicated with Bright's disease;

1 was complicated with malformation of the heart and cyanosis;

1 proceeded from extension of inflammation from contiguous textures.

These cases of severe pericarditis may be conveniently divided into two groups: first, of those occurring in persons previously in good health or actually labouring under some acute disease; and secondly, of those occurring in persons in bad health or labouring under some chronic disease. A remarkable and important difference is discoverable in these two groups in relation to the causes of the disease. For it appears that of 16 cases belonging to the first group, all were complicated with acute rheumatism, and none (so far as is known) with Bright's disease; while of 13 cases, belonging to the second group, only one was complicated with acute rheumatism, whereas fully two thirds *were known* to be associated with Bright's disease, and all of them may have been so associated. The chief causes of pericarditis are therefore inferribly two in number, namely, acute rheumatism and Bright's disease.

Dr. Taylor next enters into some considerations intended to show that these two diseases own their power of inducing pericarditis to the same kind of ultimate cause, namely, an alteration in the composition of the

* Med.-Chir. Transactions, vol. xxviii.

blood; but he does not attempt to determine whether that alteration be essentially the same in the two diseases. And further, if (he argues) it be assumed that in the single case of pericarditis, associated with cyanosis, the acute disease depended upon the state of blood existing in the chronic affection, it will follow that only two causes of the membranous inflammation of the heart were observed in the 38 cases under consideration,—namely, a morbid condition of the blood, and extension of the inflammatory process from a neighbouring texture.

The author next examines his cases of adhesion of the pericardium and of "white spots" on its surface, with reference to the causes of the inflammation producing them. This examination leads to the inference that, in every case in which any information was obtained on the point, there had previously been either acute rheumatism or pleurisy, or there was found, actually existent, either Bright's disease or some other disease of the kidney.

Dr. Taylor proceeds in continuation to examine with more detail the two affections mentioned, in regard of their agency in producing pericarditis,—and (first) *acute rheumatism.*

I. The frequency of acute rheumatism as an observed cause of pericarditis was, as already stated, 75 per cent. The complementary inquiry into the frequency of the cardiac inflammation in the course of acute rheumatism remains to be made. Now it appears that among 75 cases of acute rheumatism, treated by the author in University College Hospital, there were 37 (or about half) in which there was disease of the heart of some kind or degree; in the others there was probably none. Among these 75 cases of rheumatism there occurred 6 instances of acute pericarditis of considerable severity, besides 2 of very slight character. The proportion of the former therefore was 1 in $12\frac{1}{2}$ cases. In the same 75 cases of rheumatism there were 32 cases of valvular disease of the heart, either old or recent, besides 2 known to be recent. There was therefore 1 case of valvular disease in about every 2 cases of rheumatism.

Comparing these results with the statements of various writers upon the same subject,* Dr. Taylor draws the two following conclusions: 1st. That acute inflammation of the heart has occurred less frequently as a complication of acute rheumatism in his experience, than it has been *believed* to occur in the experience of those writers whose opinions seem to have been most generally adopted by the profession. 2d. That the frequency of cardiac inflammation, even in his cases, has been such as abundantly to show the great influence of acute rheumatism in its production. An attempt is then made by our laborious author to ascertain the real amount of the difference (with its causes) between his own observations and those of the writers referred to. The result, as respects most of those writers, may be briefly stated as follows:

* Some of these statements being totally unsupported by numerical estimates, are obviously not worth the paper they cover, and might have been omitted by Dr. Taylor. But we thank him for placing once again before the world (many a time and oft have we pondered in bewildered wonderment over the original phrases) the notorious dicta of Dr. Hope on this subject. Dr. Hope, having recorded his opinion, that in *about one half* of all cases of rheumatism either neglected or "inefficiently treated" (which, being interpreted, means treated on any other plan than the particular one he fancied and patronized), complacently adds: "I think about *one case in twelve* would be the maximum in MY practice."! This absurdity we thank Dr. Taylor for recording in an additional place, because it may serve to prevent some youths, possessed of the loose reasoning faculty of Dr. Hope, from entering upon a therapeutical career such as his. Even Dr. Hope did not *begin* with such feats as the above: let them look in time *to what they might come.*

First (in connexion with *pericarditis*). In those instances, in which such data have been given as enable us to compare *similar* cases, the results are very nearly the same. In various instances, however, no comparison can be fairly made, either from the want of figures, from the fact that cases of endocarditis and of pericarditis are mixed up together, or from a great difference in the age of the patients. Secondly (in connexion with *endocarditis*). The discrepancy is much greater than in cases of pericarditis; and one of the chief causes of the difference appears to be, that most writers have given the proportion of cases of valvular disease, in acute rheumatism, in such a manner as to imply (when this is not actually and directly stated) that they were all cases of acute disease,—omitting, in other words, to distinguish the proportion of these valvular affections which were of older date. (Vide on this point our Number for January, 1847, pp. 172 et seq.)

The proportion of cases of valvular disease of all dates observed by the author is nearly the same as that observed by the chief writers referred to; but he attempts to show (and succeeds completely), in the first place, that the greater number of these are examples of *old* valvular disease; in the second place, that at all events in most cases it is very difficult to distinguish when the disease is recent and when old; and, in the third place, that acute endocarditis is less frequent than acute pericarditis in rheumatism.

The frequency of disease of the heart in chronic rheumatism is now inquired into and compared with that in acute rheumatism; and from this inquiry it appears: (*a*) that the total number of cases of morbus cordis (old and recent) is nearly the same in the two kinds of rheumatism; (*b*) that acute inflammation of the pericardium and of the endocardium is much more common in cases of acute than of chronic rheumatism.

Dr. Taylor next turns his attention to the circumstances favoring the supervention of cardiac inflammation during the course of acute rheumatism. (*a*) The doctrine of *metastasis* has probably no supporters now in any quarter of the medical globe; and it is satisfactory to find that the cases analysed by their author give warranty to the prevalent views. Metastasis did not occur in any one of the cases observed. Dr. Taylor hence infers that metastasis is not of the ordinary—nor even a frequent—mode in which rheumatism produces the heart-affection. It does not, however, follow from these facts (he urges) that metastasis never takes place; and an attempt is made to show that its occasional occurrence is both consistent with theory and established by observation,—an attempt in which the author appears to us less felicitous than usual. In connexion with this question Dr. Taylor refers (1) to some cases of rheumatism in which the inflammation of the heart appeared before that of the joints; (2) to certain cases of what has been termed "rheumatic fever without arthritis;" (3) to one of his own cases, in which he thinks it probable [and we think so too] that there may have been acute rheumatism, and in which there was pericarditis, but no affection of the joints throughout. (*b*) As respects the particular influence of *particular forms* of rheumatism; all Dr. Taylor's cases of rheumatic pericarditis occurred in connexion with the *fibrous* as distinguished from the *capsular* (Macleod) form of the disease. It must not be forgotten, however, that the former variety is much more common than the latter. (*c*) *Intensity of the rheumatism.* From the cases examined it appears to result that the *violence and fatality* of rheu-

matic pericarditis are generally greater in the cases in which the accompanying rheumatism is very acute than in those in which it is subacute. Whether pericarditis be more *frequent* in the more severe than in the less severe form of rheumatism, the author's cases, he confesses, do not enable him with confidence to determine; as far as they go, however, they are opposed to the notion of the frequency of the one being in the direct ratio of the severity of the other,—for three fourths of the examples of rheumatic pericarditis occurred in subacute rheumatism. (*d*) *Stage of the rheumatism.* In more than one half the cases of rheumatic pericarditis the affection of the heart appeared on or before the fourth day of the disease. With one exception, the pericarditis did not appear sooner in those cases in which it was very severe than in those in which it was much less severe. (*e*) *Influence of repeated attacks of rheumatism.* In the cases examined pericarditis was found to be both more frequent and more severe in the first than in subsequent attacks of rheumatism. (*f*) *Previous disease of the heart.* Ten out of fifteen patients had no previous disease of the heart; and among these were found all the most severe cases of pericarditis. (*g*) *Age.* Of 15 patients 9 were aged 20 or under, 5 from 20 to 26, and 1 above 40. (*h*) *Sex.* Of 15 patients 9 were males and 6 females. Rheumatism is, however, more common among men than women. (*i*) *Influence of venesection.* 12 of these 15 patients had not been bled before the pericarditis appeared; the remaining 3 were bled, 1 eleven days, 1 five days, and 1 three days before the pericarditis supervened.

Even Dr. Taylor must have his hypotheses. Here is an outline of his theory of the manner in which rheumatism produces pericarditis. The cause of acute rheumatism is probably the presence of some morbid matter in the blood, which has an especial affinity for the fibrous and fibro-serous tissues of the body, and which, by fixing itself in one or more of these, induces various local inflammations. The similarity of the structures implicated is probably the reason why rheumatic pericarditis or endocarditis often occurs at the same time with, or succeeds to, rheumatic inflammation in the joints (Bouillaud), just as rheumatic inflammation in one joint occurs with or succeeds to that in another. And the heart is more frequently [??] and more severely affected in severe cases of acute rheumatism for the same reason that more joints are affected, and these more severely affected, and more fever present, in such cases. And the efficient cause of such greater severity may not improbably be a greater abundance of the materies morbi in the blood. Now we cannot, we confess, concede to this theory the quality of close inference; and in some of its elements it is deliberately defective. Observe Dr. Taylor arguing that the heart is more frequently affected in severe than in mild cases; though his facts, as far as they go (see last page), positively demonstrate the reverse. But if he fail, he may console himself with the reflection that this is a matter in which *le plus fort perdrait son Latin.*

II. Next the author enters upon a detailed examination of Bright's disease as a cause of cardiac membranous inflammations.* It has already

* Since the production of Dr. Taylor's paper, exhibiting, as it does, clearly and for the first time, the *powerful influence* of Bright's disease in generating pericarditis, many individuals have undergone abrupt opening of the eyes as to the extensive and accurate information they themselves already possessed on the point. As though the fact, vaguely known even by the most ignorant, that among the

been seen that in 13 (or more than one third) out of 35 cases of pericarditis Bright's disease was the only assignable cause of the inflammation. In respect of the frequency of pericarditis and endocarditis in Bright's disease hear Dr. Taylor's facts.

1. In the bodies of 50 patients who had either died of Bright's disease, or who were ascertained to have this disease in an advanced stage—

Acute pericarditis was found in 5, or in 1 out of 10.

Acute endocarditis was found in 4, or in 1 out of 12.

2. On the other hand, in 142 bodies in which the kidneys were not affected with any appreciable disease—

Acute pericarditis was found in 4, or in 1 out of 35.

Acute endocarditis was found in 2, or in 1 out of 71.

Pericarditis and endocarditis therefore, being four times more frequent in fatal cases of Bright's disease than in fatal cases without renal disease, it seems clearly to follow that the influence of Bright's disease in producing these inflammations is unquestionable and great.

3. From a comparison of the frequency of other internal inflammations in fatal cases of renal disease, and in fatal cases without renal disease, it appears: (*a*) that the proportional number of acute internal inflammations (exclusive of those of the heart) is twice as great in the series of cases with renal disease as in that without such disease,—the numbers being respectively 96 and 42 per cent. (*b*) That the proportion of patients likewise, among whom these inflammations were distributed, is greater in the former than in the latter series of cases, the numbers being respectively 60 and 36 per cent. Hence the safe inference is drawn that Bright's disease has a great tendency to produce other internal inflammations besides those of the heart.

4. A further examination of the same facts shows that the relative frequency of various internal inflammations is different in fatal cases of Bright's disease and of other diseases taken indiscriminately. The following inflammations (those inquired into by the author in connexion with the present point) are arranged in the order of their frequency, as they were calculated to be due to renal disease, or to the causes operating in other fatal diseases. (*a*) *Inflammations due to renal disease.* Cerebritis, pneumonia, pleuritis, pericarditis, endocarditis, meningitis, peritonitis. (*b*) *Inflammations independent of renal disease.* Pleuritis, pneumonia, peritonitis, meningitis, cerebritis, pericarditis, endocarditis.

5. The numbers given in the author's paper allows us to calculate the comparative tendency to produce internal inflammations exhibited, on the one hand, by the sum of causes operating in fatal cases of Bright's disease,—and, on the other hand, by the sum of causes present in fatal cases without renal disease. The result is displayed in the following tabular arrangement.

secondary serous inflammations of the renal disease, sparse examples of pericarditis might be found, could (except by the most addled of brains) be considered anticipatory of the discovery that Bright's disease (in its advanced, as distinguished from its early, stages) is as active a cause of pericarditis as acute rheumatism! Hear the remarkable profession of faith put forth by Dr. Latham on this point: "My own experience of pericarditis is mainly derived from what it is, as an accompaniment of acute rheumatism. I have seen the disease, indeed, under other circumstances; but it has been very seldom; so seldom, indeed, that I have little acquaintance with other conditions, external or internal, conducing to it. *I can neither tell whence to look for it, nor when to expect it,* except when it occurs as a part of acute rheumatism.....Separate from acute rheumatism, even the practice of a large hospital does not present me *with more than an instance or two of it in several years.*" (Lectures on the Heart, vol. i, p. 136.)

Bright's disease produces

a. Endocarditis *almost* 5 *times as often as* all other causes put together.
b. Cerebritis *fully* $3\frac{1}{2}$ *times as often as* ,,
c. Pericarditis ,, $2\frac{1}{2}$ *times as often as* ,,
d. Pneumonia ,, *just as often as* ,,
e. Pleuritis ,, $\frac{3}{4}$ *times as often as* ,,
f. Meningitis ,, 3 *times less frequently than* ,,
g. Peritonitis ,, 100 *times less frequently than* ,,

In instituting his next inquiry, namely, into the comparative power of acute rheumatism, and of Bright's disease in producing pericarditis and other internal inflammations, the author meets at the threshold with a difficulty, in the fact that one of these diseases is acute and seldom fatal, the other chronic and generally fatal. Dr. Taylor considers that the best way of steering clear of this difficulty is by comparing *fatal* cases of Bright's disease with ordinary cases of acute rheumatism. If the object were to ascertain the proportion of cases, in which traces of *previously existing* inflammation were found, this method would be decidedly objectionable; because the one disease, having run a much longer course than the other, would have had much more time than the latter to produce any inflammation, which it had the power to produce. But if cases of *actually existing* inflammation alone be counted, the same objection (or certainly not by any means to the same extent) does not exist; and the result should not (we agree with the author) be far from the truth. It appears, then, that of

75 cases of *acute rheumatism*, 8 were complicated with pericarditis (1 in $9\frac{1}{2}$).

50 cases of *fatal Bright's disease*, 5 were complicated with pericarditis (1 in 10).

Hence Bright's disease, *in the advanced stage*, and acute rheumatism appear to have caused acute pericarditis in a very closely equal proportion of cases. An examination of 20 cases of *old* adhesion of the pericardium, however, shows (what the considerations already put forward might have led us to anticipate) that *old* adhesions of the membrane were produced twice as often by Bright's disease as by previous attacks of acute rheumatism.

From considerations remarkable for their sustained closeness and sagacity, but for which we must refer our readers to the original, Dr. Taylor infers that acute rheumatism has a greater tendency to produce pericarditis than has Bright's disease in its earlier stages; and consequently that the tendency of Bright's disease to induce pericarditis, and probably also other internal inflammations, increases in proportion as the affection of the kidney is more advanced. This inference (which had on imperfect and theoretical grounds been admitted by others) it is pleasurable to find established, as it is established, in these pages.

In conclusion, Dr. Taylor makes some remarks upon the probable occurrence of pericarditis in other blood-diseases besides those observed by himself; and likewise on the importance of the constitutional or predisposing causes of inflammation, as distinguished from the exciting causes. To these we invite the reader's attention.

The esteem in which we hold the contributions of Dr. Taylor to the history of pericarditis, appears not only from the closeness with which

we have analysed them, but from the simple fact of our having noticed them at all in the pages of this Journal. As our readers are aware, it is not our habit to cull papers from periodical literature, and make for ourselves from the sum of these the basis of an analytical article. The absolute value of the original productions, and the hope that Dr. Taylor's example would lead others to engage in pure clinical observation (apart from chemistry and micrology gone mad) led us to swerve from our usual habits. We trust Dr. Taylor will give us more of his "Contributions;" but whether he do or not, he has established for himself the highest character the physician can aspire to—that of a sagacious and an *honest observer*.

Art. XIX.

The Human Brain: its Structure, Physiology, and Diseases. With a Description of the Typical Forms of Brain in the Animal Kingdom. By Samuel Solly, f.r.s., Senior Assistant-Surgeon to St. Thomas's Hospital, and Lecturer on Clinical Surgery, &c. &c. With numerous Wood-Engravings. Second Edition.—*London*, 1847. 8vo, pp. 688.

This very beautiful volume might almost be considered as a new work, rather than as a new edition of the treatise published by Mr. Solly under the same title eleven years since, and noticed by us with much commendation in our Eighth Number, Its size has been greatly augmented; the whole of its matter has been remodelled; and the lithographic plates of the original have been superseded by no fewer than 118 wood-engravings, many of them of the most elaborate character, and all executed in the first style of art. We regret that our limits compel us to present our readers with a much more cursory notice of its contents than their important character might fairly claim; and in justice to Mr. Solly we shall chiefly direct attention to those features of his work which give to it its most distinctive character.

The first chapter gives a sufficiently ample description, derived from the most recent and accurate sources, of the elementary structures of which the nervous system is composed. Mr. Solly may very reasonably lay claim to the merit of having been among the first to entertain that view of the functional relations between the white and gray, or the tubular and vesicular forms of neurine, which is now almost universally received amongst physiologists; having expressed himself quite clearly and decidedly on this point in his first edition. Evidence in favour of this doctrine has since been furnished from so many different sources, and it seems altogether so firmly based, that we *now* are tempted to overlook its very recent origin, and to wonder that it should have been ever doubted.

In the second chapter, we find such a general *resumé* of the comparative anatomy of the nervous system, as is calculated to prepare the student for the due comprehension of its structure in man. Commencing with the star-fish, he shows that we find in it the elements of the most highly developed nervous system; namely, ganglionic centres, commissures, and nerves. The brain of man is nothing else than a collection of distinct ganglionic centres, packed together in one mass; its complexity of structure depend-

ing upon the number of these which the different functions require, and upon the intimacy of relation which is established between them by the various commissures. Mr. Solly proposed, in his former edition, the term *hemispherical ganglia* to distinguish the cortical substance of the cerebral hemispheres from the other masses of gray matter which are included under the collective designation Brain, or even within the more limited term Cerebrum; and although it is rather too lengthy for ready use, we do not know what other could be advantageously substituted for it. His early perception of the fact, that the various insulated patches of gray matter at the base of the cerebrum and in the medulla oblongata must be regarded as so many distinct and independent ganglia, each of which has its own proper function, was, in our apprehension, the great merit of his original descriptions; and it has been his guide in various subsequent anatomical inquiries, the results of which we shall presently notice. After a brief general exposition of the nature and offices of the principal divisions of the nervous system, Mr. Solly traces these through the higher invertebrata; quoting the anatomical descriptions of Garner and Owen for the mollusca, and of Newport for the articulata; and adopting the physiological explanations given by Dr. Carpenter in his Prize Thesis and subsequent writings. It is to the anatomy of the nervous system in the vertebrated classes that Mr. Solly has more particularly devoted his own attention; and here we find the descriptions drawn almost entirely from his own observations, although he has occasionally profited by the writings of Serres, Leuret, Owen, and others. In attributing to Professor Owen and to Mr. Newport, in a former article, the first indication of the true homology of the cephalic ganglia of the mollusca and articulata respectively, we had overlooked the fact that Mr. Solly had previously stated, in general but distinct terms, his opinion that the hemispheric ganglia are organs confined to the vertebrata, and that the cephalic ganglia of the invertebrata are collective representatives of the ganglia in immediate connexion with the organs of sense, which make up the principal part of the encephalon of the lower fishes. This is another instance of Mr. Solly's clear-sightedness; and we gladly accord him the credit of having been, as we believe, the first to form a stable and permanent bridge over the chasm that was previously supposed to divide the encephalic centres of the vertebrated from those of the invertebrated animals. If, as we believe, our future advances in the physiology of the brain depend more upon a philosophical use of the facts supplied by comparative anatomy, than upon any other single source of information, we shall owe much to Mr. Solly for having thus indicated the right path of inquiry.

At the time when Mr. Solly's first edition was published, anatomists and physiologists were far from being agreed respecting the homology of the fish's brain; and many mistakes had been made, in consequence of insufficient scrutiny into the connexions of the elements of which it is composed. Where *four* ganglia were present in approximation with each other in the antero-posterior direction, as in the encephalon of the eel or pike, the most anterior, from its evident connexion with the olfactory nerves, had been recognised as the olfactory ganglion, whilst the hindmost had been universally and correctly designated as the cerebellum. But of the two intermediate ganglia, the anterior were regarded by some, and the

posterior by others, as the homologues of the cerebral hemispheres of higher animals. And the discrepancy was still further increased by the misinterpretation of the structure of the encephalon of the cod, whiting, &c., in which the olfactory ganglia are removed to a great distance from the posterior masses, and are connected with them by long peduncles. The existence of these olfactory ganglia was overlooked, and the commissural peduncles were treated as the olfactory nerves; so that the total number of ganglionic masses in such cases was only reckoned at three, and what are really the second pair of ganglia in the fish's brain (reckoning from before backwards) were frequently taken for the first, or were supposed to combine the functions of the first and second. To Mr. Solly is due the chief merit of having corrected this important error, into which an anatomist no less distinguished than Serres had fallen, and from which not even Leuret (who does not seem to have been acquainted with Mr. Solly's work) has escaped. It was shown by Mr. Solly that in no instance are there fewer than four distinct ganglionic masses in the fish's encephalon; and that there always exists a pair of olfactive ganglia anterior to those which constitute the rudiment of the cerebral hemispheres, although the distance between them is frequently very considerable. Thus in a carp, whose entire length was but seven inches, the length of the commissural peduncle was somewhat above an inch. The encephalon of the carp is one of the most interesting of all the brains of this class of animals; from the circumstance that certain ganglionic centres, which have elsewhere coalesced or are buried in the medulla oblongata, are here distinct. This is the case with the auditory ganglia, whose homologues in man, as we shall presently see, are the gray nuclei of the posterior pyramidal bodies; and also with the pneumogastric ganglia, which are represented in the human subject by the gray matter of the restiform bodies.

Having so recently expressed our convictions as to the impossibility of obtaining a correct idea of the component parts of the human encephalon, without a careful study of the ascending series of brains in which successive stages of complication are exhibited, we need not now follow Mr. Solly through his detailed description of the comparative anatomy of this organ in reptiles, birds, and mammals; but shall only say that it exhibits throughout the clearness and completeness which mark a thorough practical acquaintance with his subject, and the conciseness and simplicity which are indicative of ample experience in anatomical tuition; whilst the details are never obtrusively presented, as they are in most works treating *ex professo* on comparative anatomy, but are all kept in subordination to the one great object, the illustration of the structure of the human brain. But we shall direct the attention of our readers to his account of the structure of the medulla oblongata, which he has made the subject of careful and repeated investigation, both in man and the lower animals. It is, in fact, first brought under consideration when the brain of the sheep is being described. No mistake is more common, than to confuse the *strands* or bundles of fibres which go under the names of corpora pyramidalia (anteriora and posteriora), olivaria, and restiformia, with the *nuclei* of gray matter which some of these contain, but with which functionally they have nothing to do. The strands are cords of communication between the encephalon above and the spinal cord below; whilst the nuclei are the ganglionic centres of the nerves issuing from the medulla oblongata

itself. The anterior pyramids are *merely* fibrous tracts; but the olivary, restiform, and posterior pyramidal bodies possess gray nuclei imbedded in these. The size of these projections on the surface of the medulla oblongata bears no relation whatever to that of the nuclei; and the absence of the latter must not be inferred from the want of the former. Thus the olivary bodies have been affirmed to have no existence in many mammalia, their highest development being attained in man. Mr. Solly has found the ganglionic nuclei, however, in the sheep, horse, calf, and cat; and he entertains no doubt that they are universally present. Their position in the horse is curiously changed, as they approximate much more closely to the median line, so as almost to occupy the place of the pyramids in man; so that, as Mr. Solly justly remarks, they must be regarded as ganglia simply imbedded in the motor tract, and as not forming any line of physiological demarcation. In his former edition he had expressed the idea that they might be regarded as the ganglionic centres of the pneumogastric nerve; but his later researches have shown him that this was an erroneous determination, and that the real pneumogastric ganglia are those imbedded in the restiform bodies, whilst the olivary ganglia are the centres of the motor nerves of the tongue, commonly termed the ninth pair. The great bulk of the vesicular nervous substance which they contain in man would thus seem to correspond with the multiplied movements of his tongue as an organ of speech.

The gray nucleus of the restiform body, or the proper restiform ganglion, had been recognised by Stilling as the true centre of the pneumogastric and glossopharyngeal nerves; but he had not sufficiently distinguished it from the auditory ganglion or nucleus of the posterior pyramidal body, with which it is in close contiguity. The clear determination of this point, which may be more distinctly made out in some of the lower mammalia than in man, is due to Mr. Solly; who has thus had a large share in the discovery of another very important fact in physiological anatomy. It may be interesting to remark that this close connexion of the auditory ganglion with the pneumogastric harmonizes well with the direct influence which the sensations derived through the former have upon the movements of vocalization, in which the latter has so considerable a share. In the locus niger, a collection of gray matter in the substance of the crus cerebri, to which no one, we believe, had previously assigned a definite function, Mr. Solly finds the ganglionic centre of the third or oculo-motor nerve, one root of which passes into it.

Our readers are probably aware that to Mr. Solly is due the discovery of the connexion between the anterior columns and the cerebellum by three sets of fibres, of which two are superficial, and are derived from the pyramidal columns, whilst another is deep, and is derived from the olivary columns. The former had been previously noticed by Santorini and Rolando, under the name of the "arciform" fibres; but these anatomists do not seem to have been aware that these fibres passed directly to the cerebellum by the restiform column with which they become incorporated, but described them as passing down from the pons varolii. The merit of Mr. Solly's researches on this head consists in his having traced out the superior connexions of these superficial fibres, which Rolando had left obscure; and also in having discovered an entirely new and deeper series connecting the cerebellum and the olivary columns. Another very

interesting point in the anatomy of the medulla oblongata seems to have been settled by his researches; namely, the decussation of the sensory columns. The existence of this might be predicated upon physiological grounds, from phenomena of the same class with those which led the decussation of the motor columns to be anticipated; but though Sir C. Bell thought that he had made it out, it has not been generally admitted as an anatomical fact. According to Mr. Solly, the decussation does not take place at a point so low as that described by Sir C. Bell; and the last-named anatomist was probably misled by the appearance presented by the posterior part of the motor column, where the anterior pyramids are interlacing. The decussation seen by Mr Solly takes place whilst the sensory tract is passing through the pons varolii, and is consequently above the origin of the auditory nerve. It appears from Foville's delineations, that he had previously recognised it; but he does not describe it. The same decussation has been recently figured by Dr. Radclyffe Hall in the plates illustrating the last of his papers on the 'Sympathetic Nerve' (Edinb. Med. and Surg. Journal, July, 1847, Plate VII); but we do not find any detailed description of it in his text.

On one very important anatomical question,—namely, the connexion of the roots of the spinal nerves with the cord,—Mr. Solly gives us valuable testimony from his own investigations. Our readers will be aware that a few years ago the prevalent idea was, that all the roots of the spinal nerves are continuous with the white or fibrous strands of the cord; so that when Mr. Grainger first ventured to assert that a portion of each root terminates in the gray matter of the cord, his statement was received with considerable hesitation. Of late, however, the current has set in the opposite direction; for Stilling and Messrs. Todd and Bowman have been disposed to deny that the continuity of any of the fibres entering into the roots of the nerves with the fibrous columns of the cord can be distinctly traced; and they have thus considered the gray matter of the spinal cord as the ganglionic centre of *all* the roots of the spinal nerves; its white columns being commissural in their character, conveying influence from one ganglionic centre to another, instead of establishing a direct connexion between the encephalon and certain portions of each spinal nerve. We have never been able to regard this view as physiologically probable; and it has always appeared to us inconsistent with the analogy of the articulata, in which the direct connexion of the nerves of the cord with the cephalic ganglia, through the medium of the fibrous tract, is sufficiently clear. Dr. Budge of Bonn had contested the accuracy of Stilling's assertions on this point; and we are happy to find that Mr. Solly's observations fully coincide with his,—fully bearing out the truth of Mr. Grainger's original statement.

We have thought it due to Mr. Solly, and likely to interest our readers, that we should thus briefly advert to the chief points on which his anatomical labours have contributed to the positive advance of neurology. The value of this portion of his work, however, does not depend upon the number of new facts it contains, but upon the aspect in which it has placed the old. We have not space to follow him through his account of the anatomy of the human brain, towards which all the previous details of comparative anatomy are made skilfully to converge; but we shall quote the concluding paragraphs, which will show the general plan on which his descriptions are arranged.

"The above descriptions demonstrate that the encephalon or brain in the human subject is not a large solid mass of matter, in the interior of which are cavities scooped, as it were, out of its substance to be appropriately denominated ventricles; but that it really consists of ganglia or collections of cineritious neurine, placed on each side of the mesial line:—some of them being the appropriate ganglia of the nerves of sensation, as, for instance, the olfactory ganglia, the optic ganglia or tubercula quadrigemina, the auditory ganglia or posterior pyramidal bodies, the pneumogastric ganglia or restiform ganglia, the olivary bodies or lingual ganglia; the others being the motory and sensory ganglia, as the corpora striata and thalami nervorum opticorum. The hemispherical ganglia, again, that they might present the greatest possible extent of surface, are folded up into innumerable plaits, and thus cover or surround every other ganglion within the cranium, so that, on first removing the skull-cap, nothing can be seen but the convoluted surface of these extensive ganglia.

"And here let me insist upon this important principle in the study of the brain, which is also one of the first ideas that the student should acquire regarding its composition, namely, that it consists of corresponding or symmetrical parts on each side of the mesial plane; and that, instead of regarding the fissures of separation between its different portions as forming ventricles or cavities, he must direct his attention to the ganglia which bound the fissure, and to the structures called commissures, which, connecting them together, cross the fissure, and necessarily alter its character in different points, masking it, it is true, but not at any place changing the fissure into a true bag or circumscribed cavity. The third, the iter a tertio ad quartum ventriculum, the fourth, and fifth ventricles, as we have already seen, are in truth no more than the successive dilatations from below upwards of the posterior fissure of the cord; difficult enough to be understood when these are viewed in different situations and unconnected with each other, as in the ordinary mode of dissecting the brain, but which seem necessary and obvious where its parts are traced in connexion with one another.

"In conclusion, let me express the hope that these views or analyses, if I may be allowed so to call them, of the component parts of the encephalon, will really simplify the whole of its anatomy, and materially assist the student in acquiring a knowledge of its true character; I wish that custom did not require the student to burthen his memory with fanciful and unmeaning names, and that instead of learning a long catalogue of the contents of the lateral ventricles, as they are erroneously designated, and puzzling himself with the absurd titles of hippocampus major and minor, pes hippocampi, tænia hippocampi, cornu ammonis, &c., he should be required simply to observe how the spinal columns appear to terminate superiorly in two large tubercles, the *corpora striata* and *thalami*, from the sides and under parts of which the hemispheres spring out, being afterwards reflected so as completely to envelop this bulbous extremity of the spinal cord. In the same way, the third ventricle should be described as a fissure separating the two halves of the brain, his particular attention being directed to the commissures which pass across it to connect the different cerebral ganglia with one another. The description of the relative position of these ganglia, the commissures connecting them, and their relation to the ganglia and columns of the spinal cord, comprehend all the information which is either interesting or useful to the student." (p. 283.)

We regret extremely that our vanished space compels us to omit all notice of the pathological part of this volume. We can, however, assure our readers that they will find in it much valuable practical matter. The work altogether reflects the highest credit on the author, and ought to find a place in the library of every practitioner.

Art. XX.

Observations on the Treatment of Lateral Curvature of the Spine, &c.; with Woodcuts. By Edward F. Lonsdale, Fellow of the Royal College of Surgeons, &c. &c.—*London*, 1847. 8vo, pp. 116.

Mr. Lonsdale has added another work to the number already treating of deformities of the vertebral column. Like his predecessors, he has his theories, his mechanical supports, his couch, and his rules for muscular exercise.

His theories are of the mechanical order. The causes of lateral curvature, and of curvature to the right side in especial, are pointed out to be, first, employments requiring constrained positions, and in which the right arm is principally in use, and which leads to imperfect expansion of the left side of the thorax and a pendulous position of the left arm, so that it acts as a dead weight on the vertebral column. The original normal difference in the size of the two lungs may, he thinks, be also stated as a predisposing cause of lateral curvature to the right side.

The constriction of the thorax, by stays, is also less resisted by the ribs of the left side than of the right, in consequence of this inferiority in the expansive capacity of the left lung. The more frequent use of the right side than of the left is another cause, inasmuch as this is followed by a superior development of the muscles of that side, and a freer expansion of the lungs. Then Mr. Lonsdale refers to the greater development of the liver on the right side; this prevents the ribs of that side giving way. The liver, in fact, acting as a mechanical support, a sort of pad to the ribs, and even predisposing to the right-handedness of the majority of people. Mr. Lonsdale boldly denies, however, that there is any evidence or reason to suppose that the muscles on the one side are weaker than on the other.

From these and other remarks, and from certain faults of omission too, we infer that Mr. Lonsdale's physiological knowledge is not profound, and that he has little if any acquaintance with French and German writers on the subject of spinal deformities. In the first place, the inequality in the vital powers of the two symmetrical halves of the body is a demonstrated general fact in physiology. Mr. Lonsdale will find numerous instances illustrative of this fact in physiological works, some of which are stated in Dr. Laycock's 'Treatise on the Nervous Diseases of Women.' In the embryo the liver is a symmetrical organ; it assumes its usual form from shrinking of the left half. In air-breathing gasteropoda and in serpents with one lung only, it is the left side that is defective; the pulmonary apparatus is on the right side, so in other organs, as the testes, mammæ, &c. In females the thoracic development is less energetic than in men, and hence a proclivity to those deformities of the thorax, and displacement of the thoracic vertebræ which are dependent on irregular muscular action. All this seems to be unknown to Mr Lonsdale. We had thought that this connexion of lateral curvature with debility or quasi-paralysis of groups of respiratory muscles must be universally known by this time, and that the primary seat of the affection ought to be sought in the nervous centres, and espe-

cially in that portion from which the external respiratory nerves arise. Mr. Lonsdale will do well to read our review of Stromeyer's work on Paralysis of the Muscles of Inspiration (Vol. V, p. 133), or, better still, the original publication.

The mechanical support Mr. Lonsdale recommends is a modification of one invented by Mr. Tamplin. The pelvis (as in all these instruments) is the base of support; a crutch, arising from an apparatus attached to the pelvis (a "pelvis hook"), supports the left shoulder, and a pad attached to a lever taking its point of support from the same apparatus, is compressed by means of a screw against the bulging ribs of the right side. No numerical details inform us as to the success with which this machine has been used, but it is probably neither greater nor less than that of others of the same kind. It looks ingenious on paper.

This machine is the main agent in the treatment; two other proceedings are adjuvant to it. First, a couch is devised, by which the patient is recumbent on her right side, the "bulging ribs" being compressed by means of a strap. This seems to be little more than a modification of the hammock recommended by Pravatz, of Paris, with indications precisely similar to those stated by Mr. Lonsdale. Secondly, a plan is recommended whereby the muscles of the back are brought into free action. Mr. Lonsdale observes, that in the confirmed cases of scoliosis exercise is injurious.

There is not a word in the book as to the constitutional treatment of the affection. We are inclined to think that Mr. Lonsdale can produce a better book than this; but he must eschew mere mechanics, and study the living machinery of respiration in all its relations. Of the mode he has partly an example in Stromeyer's works; without this, no work on deformities can rise above the barest mediocrity.

Art. XXI.

On Dyspepsia; with Remarks, submitted in support of the Opinion that the Proximate Cause of this and of all other Diseases affecting the General System is Vitiation of the Blood. By John Burdett Steward, M.D., Fellow of the Royal College of Physicians.—*London*, 1847. 8vo, pp. 106.

Dr. Steward "admits his conviction to be, that the proximate cause of all diseases consists in some alteration in the force, quantity, or quality of the circulating fluid; and that of those affecting the general system, vitiation of the blood is an invariable accompaniment." This principle is that which runs through his 'Remarks.'

On looking to see how Dr. Steward explains the origin of *indigestion*, and shows its proximate cause to be some change in the blood, we find him turning the tables on himself, and showing how indigestion itself is the cause of the change.

"A person, at the time healthy, either from quantity or quality of the food or fluid taken, or from grief, or any other cause equal to produce it, has an attack of what is called indigestion, that is to say, the food does not undergo the required changes; weight, uneasiness, and nausea are present; the concoction is imperfect,

and the chyme necessarily suffers in character from this imperfect concoction, being less fit to form the composition which it is destined to become by the action of the bile. In other words, the chyme . . . . thus altered, when acted upon in the duodenum by the bile, must produce a chyle also less than perfect. . . . . The blood, therefore, with which this chyle, so changed, becomes mixed, must suffer deterioration in an equal degree." (p. 32.)

Here we have functional derangement, or disease of the digestive organs, followed by imperfect "concoction," and the blood is allowed to be healthy: what then becomes of Dr. Steward's conviction? How can he explain the action of grief upon the digestive organs, unless, indeed, he allows that the proximate cause of indigestion may consist in some change in the nervous system? It does not suit Dr. Steward's views to inquire into changes of this kind, for the more he inquires into them, the more he is bewildered—the deeper he gets into "mystery." It might have occurred to Dr. Steward that some of these difficulties, and the discovery of these unfathomable "mysteries," may be as fairly attributable to the mind of the individual inquiring, as to the subject itself. So appalled is Dr. Steward by the initiatory studies requisite for a comprehension of the physiology of the nervous system, and of the relations of that system to morbific changes in structure and function, and even to changes in the blood itself, that he will not enter upon them at all; and, in short, rejects all consideration of the nervous system whatever.

"It is not because I think lightly of its influence, that I reject reference to the nervous system, but because, in referring to it, every step we take leads us further and further into mystery. Not only is the power by which it works its effects hidden from us, but a knowledge of its changes during life being impossible, and after death doubtful, our reasoning must be hypothetical, and our treatment therefore uncertain." (p. 16.)

We need scarcely subjoin to this extract that Dr. Steward adds nothing to the pathology of the blood; gives no new researches showing its changes during life or after death, and, of course, tells his readers nothing about the changes it experiences in dyspepsia, except those hypothetical inferences which any one of his readers, whether medical or non-medical (for the book is addressed to both classes), might not as readily make as Dr. Steward himself.

With regard to the treatment of dyspepsia advocated by Dr. Steward, we need only add that it is the same as adopted by nine tenths of the practitioners of the United Kingdom. The book is, in short, a good manual for Dr. Steward's friends and patients; to them he can properly address Abernethy's counsel—"read my book," and to them it will be useful, and for them, we are inclined to think, it was written, and not for us, or for the literary and philosophical practitioner.

THE LATE DR. ANDREW COMBE.

[We are indebted to the columns of that very superior newspaper, THE SCOTSMAN, for the following excellent account of an excellent man—if ever such there was. We have reason to believe that it is from the pen of a celebrated writer as well as a kindred spirit, who knew the deceased long and well—Mr. Robert Chambers. In all that is therein said in commendation of the character of Dr. Combe, we so entirely concur—and we speak from long personal intercourse—that if we could wish any of the expressions altered, it would be only that they might be made still stronger and more emphatic. Never, we will venture to say, did the ranks of Physic lose a more estimable member; and rarely—very rarely—has the grave closed over a gentler, truer, wiser, or better man. His loss to his friends is a loss that can never be supplied; his loss to the community is one of the greatest it could sustain in losing an individual. But he had fulfilled his mission, and done his work as far as was permitted: may they who are left to lament him, strive, as far as in them lies, to emulate his bright example.]

Dr. Combe's death took place at Gorgie Mill, near Edinburgh, on Monday, the 9th of August, when he had nearly attained the age of fifty years. Since 1820 he had laboured under severe pulmonary consumption,* which frequently interrupted his practice, compelled him to spend many winters in France, Italy, or Madeira, and at length, by wholly unfitting him for the active duties of his profession, gave him that leisure which he turned to so excellent account in the preparation of his well-known works on Health and Education. In April last, hoping to receive benefit from a voyage, he paid a short visit to the United States; and although this hope was disappointed, his health did not appear to have suffered from the exertion, and it was not till within a few days of his death that his condition became alarming. The immediate cause of that event was chronic disease of the bowels, which suddenly came to a crisis, and was such as to defy every effort of medical skill. His sufferings were extremely slight, and he displayed to the end that cheerfulness and serenity which was a prominent feature of his character during life.

Dr. Combe was born at Livingston's Yards, a suburb under the south-west angle of the rock of Edinburgh Castle, on the 27th of October, 1797. His father—who carried on the business of a brewer, and was remarkable for his worth and unassuming manners—had married in 1775 a daughter of Abram Newton, Esq., of Curriehill, noted among her acquaintances for her skilful domestic management and general activity and good sense. From this union sprang a family of seventeen children, among whom Andrew held the place of fifteenth child and seventh son. In those days, the district we have named abounded still more than it does at present with offensive pools and ditches, the noxious influence of which, in conjunction with defective ventilation of small and crowded sleeping-apartments at home, appears to have been a chief cause of the disease and early mortality

* The examination after death showed great disease of one lung, but the affection was not, strictly speaking, *consumption*, as there were no tubercles there or elsewhere.—ED.

which prevailed in the family. Certain it is that his youthful experience and observation supplied Dr. Combe with not a few of the examples by which he afterwards impressed on his readers the evil effects of ignorance of the laws of the human constitution.

Having gone through the usual course of instruction at the High School, he was bound apprentice to the late Henry Johnston, Esq., surgeon in Edinburgh; and in 1817 had acquired an amount of skill and knowledge which enabled him then to take a surgeon's degree. With the view of farther qualifying himself for medical practice, he next repaired to Paris, where two years were laboriously spent under the tuition of such men as Dupuytren, Esquirol, and Spurzheim. In 1823 he began to practise in Edinburgh, and about two years later took there his medical degree. The conscientiousness, kindliness, and sagacity which he displayed as a physician, and the extensive knowledge he had acquired of his profession, speedily brought him a flourishing practice, which became every year more extensive, till a return of pulmonary symptoms obliged him in 1831 to proceed once more to Italy. He was, however, able to pass the winter 1832-3 in Scotland, and in the latter year to resume his practice. In 1836 he was honoured with the appointment of Physician in Ordinary to the King and Queen of the Belgians, and for several months attended the royal family in Brussels; but the climate proving unfavorable to him, an alarming return of the pulmonary symptoms abruptly sent him back to recruit his health in his native land. Subsequently he continued to act as Consulting Physician to their Majesties, and occasionally paid them a visit. About six or seven years ago he was appointed one of the Physicians Extraordinary to the Queen in Scotland, and afterwards one of her Majesty's Physicians in Ordinary in this part of the United Kingdom. He was also a Fellow of the Royal College of Physicians of Edinburgh, and a Corresponding Member of the Imperial and Royal Society of Physicians of Vienna.

The works by which the name of Dr. Combe is best known to the public are—'The Principles of Physiology applied to the Preservation of Health, and to the Improvement of Physical and Mental Education,' of which twelve editions have been called for since its first appearance in 1834; 'The Physiology of Digestion considered with Relation to the Principles of Dietetics,' originally published in 1836, and now in the seventh edition; and 'A Treatise on the Physiological and Moral Management of Infancy, for the use of Parents,' of which the first edition came out in 1840, and the fifth in the present year. In preparing these works, his constant aim was to exhibit the relation subsisting between the rules of conduct recommended and the particular laws of the organization according to which their influence is exerted, so that the recommendation might rest, as far as possible, on the foundation of Nature, and not on his mere personal authority. He wished to make his readers understand *why* certain courses are beneficial and others hurtful—to exhibit *principles* in a clear and intelligible manner, so that every individual might be enabled to adapt his conduct rationally to his own peculiar circumstances. He urges, that as every organ of the body has a specific constitution, and is regulated in its action by fixed laws appointed by Divine Wisdom, success in avoiding causes of disease, and in removing them when they come into play, will greatly depend on the extent of our knowledge of the nature and laws of

the various organs, and their relations to each other and to external objects. "In teaching dietetic rules and hygienic observances, therefore," says he, "the precepts delivered should be connected with and supported by constant reference to the physiological laws from which they are deduced. *Thus viewed, they come before the mind of the reader as the mandates of the Creator;* and experience will soon prove that by his appointment, health and enjoyment flow from obedience, and sickness and suffering from neglect and infringement of them." The words we have put in italics express an idea on which he frequently dwells with earnestness in his works, and which he delighted in private conversation to enforce. The extent to which these valuable treatises have been circulated, and not merely read but *studied*, shows that he did not mistake the manner in which such instruction can be successfully imparted.

In 1820 was instituted the Phrenological Society, of which he and his elder brother, Mr. George Combe, were among the leading members. He contributed two essays to the volume of Transactions published by that body in 1824, and subsequently wrote many valuable papers in the *Phrenological Journal*, which was commenced in 1823, and now extends to twenty volumes. In 1831 he published 'Observations on Mental Derangement; being an Application of the Principles of Phrenology to the Elucidation of the Causes, Symptoms, Nature, and Treatment of Insanity.' This work has long been out of print, and is thus referred to in the preface to the eleventh edition of his 'Physiology applied to Health and Education:' "As many inquiries continue to be made for a new edition of my 'Observations on Mental Derangement,' I avail myself of this opportunity to state that infirm health having prevented me from devoting much attention to the treatment of insanity for some years past, and consequently disqualified me from doing that justice to the subject which its later progress and inherent importance imperatively demand, I have, although with great reluctance, abandoned all present intention of reprinting the work." In the beginning of 1846, his strong conviction of the importance of phrenology to medical men induced him to write, at the expense of considerable fatigue, an 'Address to the Students of Anderson's University, Glasgow, at the opening of Dr. Weir's first course of Lectures on Phrenology.' It was delivered to a crowded audience by his brother, and subsequently appeared as a pamphlet. He also contributed several articles to the *British and Foreign Medical Review*, and was an occasional writer on medical and sanitary subjects in our own columns.

The decease of Dr. Combe will have taken no one who knew him by surprise, for he was for many years in that condition which makes life a greater miracle than death; but it will not on this account be the less deplored, either as causing a blank in the circle of private friendship, or as the signification of a public loss. Dr. Combe belonged to that rare class of physicians who present professional knowledge in connexion with the powers of a philosophical intellect, and yet in practical matters appear constantly under the guidance of a rich natural sagacity. All of his works are marked by a peculiar earnestness, lucidity, and simplicity, characteristic of their author; they present hygienic principles with a clearness for which we know no parallel in medical literature. To this must be ascribed much of the extraordinary success they have met with, and on this quality undoubtedly rests no small portion of their universally

acknowledged utility. Those, however, who look below the surface will not fail to trace a deep philosophical spirit as pervading these works, something arising from a perfect apprehension of, and a perfect allegiance to, the natural rule of God in our being. It has been a guidance—we would almost say an inspiration, to the author, without ever carrying him for a moment where ordinary readers could not follow him. Here we think is the true though latent strength of Dr. Combe's popular writings, and that which will probably give them a long-enduring pre-eminence in their particular department. We always feel, in reading them, that we are listening to one of those whom Nature has appointed to expound and declare her mysteries for the edification of her multitudinous family. In his own section of her priesthood, certainly few have stood in his grade, fewer still become his superiors.

The personal character and private life of Dr. Combe formed a beautiful and harmonious commentary upon his writings. In the bosom of his family and the limited social circle to which his weakly health confined him, he was the same benignant and gentle being whom the world finds addressing it in these compositions. The same clear sagacious intelligence, the same entire right-mindedness, shone in his conversation. An answer to any query put to him, whether respecting professional or miscellaneous matters, was precisely like a passage of one of his books, earnest, direct, and conclusive. Whatever, moreover, he called upon others to do or to avoid, that he did and that he avoided, in his own course of life; for doctrine with him was not something to be treated as external to himself, but as the expression of a system of divine appointment of which he was a part. To his rigid, though unostentatious adherence to the natural laws which he explained, it was owing that he sustained himself for many years in a certain measure of health and exemption from suffering, while labouring under the consumptive tendency which finally has cut short his career. On this point there is the more reason to speak emphatically, when we reflect that the years thus redeemed from the grave were employed in that which will yet save many from premature death, as if it had been his aim to show the value of even the smallest remains of life and strength, and thus advance one of the principles dearest to humanity. It was not, however, in any of these respects that the character of Dr. Combe made its best impression, but in his perfect geniality and simplicity, and the untiring energy of his practical benevolence. Here resided the true charm of his nature, and that which made him the beloved of all who knew him. No irritability attended his infirm health; no jealousy did he feel regarding those whom superior strength enabled to outstrip him in the professional race. Kindly and cordial to all, he did not seem to feel as if he could have an enemy—and therefore, we believe, he never had one. It might almost have been said that he was *too* gentle and unobtrusive—and so his friends, perhaps, would have thought him, had it not on the other hand appeared as the most befitting character of one who, they all knew, was not long to be spared to them, and on whom the hues of a brighter and more angelic being seemed already to be shed.

POSTSCRIPT

TO

No. XLVIII OF THE BRITISH AND FOREIGN MEDICAL REVIEW.

BY THE EDITOR AND PROPRIETOR.

With the present Number, the 'British and Foreign Medical Review,' after an uninterrupted course of quarterly publication for twelve years, comes to a conclusion;—at least, under its original form and designation, and as *my* Journal. On such an occasion, it seems but courteous to my readers, and to the numerous friends who have so warmly supported me, to say something by way of leave-taking; and it may be both interesting and useful to some of them, as well as to others, now or hereafter, if I also give a few particulars respecting the history and fortunes of a work which the profession has been pleased to regard as valuable and influential. Having been the exclusive originator, planner, proprietor, and conductor of the Journal, I possess means of making an exposition of this kind, which have seldom existed in similar cases.*

For many years before the commencement of the Review,—in common I believe, with all who had any pretensions to be judges in such matters, and who had paid even moderate attention to the state of medical literature in this country,—I had been deeply impressed with the deficiencies of the medical press generally, more especially in the department of criticism, as exhibited in the pages of the more popular journals. In some of these, not only was the general style of the writing of the coarsest and most slovenly character, but, what was still more lamentable, the judgments and opinions passed on the books reviewed were, not merely crude and unsifted, but often most incorrect and unfair. Generally speaking, the articles mainly consisted of extracts, connected by little scraps from the pen of the reviewer, made up of the merest truisms and dullest common-places, usually of a more or less laudatory character. The principle of indiscriminate laudation would, indeed, almost seem a necessary part of such scissors-work and patch-work reviewing; as a critic while passing a severe condemnation on a book, could hardly have the face to treat his readers to voluminous extracts from its pages. This general inferiority of medical criticism and false appreciation of literary merit, and these defective judgments had another and facile source in the unfortunate system of *gratuitous contribution* generally prevalent. An editor whose professed system was not to pay for contributions, was naturally enough ready to receive help in his laborious task, and was not likely to be over-fastidious in questioning the capabilities of his volunteer writers, or in canvassing the motives which induced them to write in particular cases. It may thus easily be understood how articles might find their way into journals so conducted, written by very incompetent or improper persons; by raw, unpractised youths anxious to see their lucubrations in print; by the friends of the authors whose works were

* My excellent friend, Dr. Conolly, co-operated with me as Editor during the publication of the first few numbers; but, although his name remained afterwards on the title-page, he took no share whatever in the editing. The readers of the Review, however, had often the benefit of his admirable talents as a contributor.

reviewed; or even by the authors themselves.* *Foreign* works were, of course, almost virtually excluded from publications of the kind referred to, except occasionally when an easy French volume afforded an opportunity to some youthful literary aspirant of exercising a laudable industry in mastering the heads of a practical treatise, and giving an epitome thereof: criticism in such a case was not to be expected. Other reasons might be given for the low standard of medical criticism at the period referred to, as well as for the very limited scope of the journals exhibiting it; but what I have stated is sufficient to show that at the time when this Journal was set on foot, there was great need for a Review of a superior stamp; and there seemed ample justification, both professionally and commercially, for the attempt to establish the 'British and Foreign Medical Review' on a broader basis and with higher aims.

The plan and objects of the new Journal were long under consideration, and were completely matured previously to the appearance of the first number. They were intended to save it from the various evil influences which were believed to interfere so seriously with the utility and value of some other journals, as well as from evils of a different kind, which, without precaution, might spring up in its own path.

The leading principles on which it was determined to conduct the Journal were—in the first place, a thorough impartiality and independence of criticism, and a determination to speak the truth both as to books and men, without regard to person or place; secondly, the establishing, as far as this was practicable, of a tribunal, competent by the character, learning, and experience of its members, not merely to judge truly of the merits of the different works that came under notice, but to correct their errors and supply their deficiencies; thirdly, the regarding works under review as literary as well as technical productions, and thus holding the authors responsible for the form as well as the matter of their productions—for the language as well as the science; lastly, the considering the medical literature of all countries as equally entitled to notice, that is, reviewing books according to their actual merit and importance, without reference to nation or language.

It was proposed to carry these principles into effect by means of certain well-considered arrangements. As these were adopted from the very first, and adhered to as closely as possible through the whole course of the Journal, there will be little impropriety, while noticing them, to speak of them either in the past tense, or prospectively, as may be most convenient. The following are the more material of these arrangements:

1. No gratuitous articles to be admitted; and the remuneration to be such as to justify the Editor in expecting the co-operation of competent writers.†

2. All books thought deserving of notice, whether British or foreign (including Journals), to be purchased, if not presented to the editor.

3. The different writers to be enjoined the strictest impartiality. As promotive of this, the Editor was careful to avoid intrusting particular books to writers who, from circumstances, might be presumed to be more or less biassed against or in favour of the authors, from being known to have been or to be at variance, or particularly intimate with the author, from the

* I think it right to state that I do not intend these strictures to apply to the 'Edinburgh Medical and Surgical Journal.' Its reviews were, on the whole, good; but its limited plan entirely prevented it from giving anything like a comprehensive account of books.

† The precise remuneration was seven shillings and sixpence per printed page, or six pounds per printed sheet of sixteen pages, extracts being reckoned as original matter.

parties living in the same town, from being rivals in practice, or from being the authors of rival works, &c. &c. On the same grounds it was also an invariable rule never to allow the name of the publisher of any book to appear at the head of an article. Still further aid towards insuring impartiality (as well as other important ends, which need not be specified) was derived from the regulation, seldom neglected, of the books reviewed being returned along with the MS. articles. The Editor had thus the opportunity of again examining the work criticised, if at any time it should appear to him, while reading the articles in manuscript, that there were any doubts of the writer's perfect fairness. The contributors will bear witness that this was a duty which the Editor never shrank from, and rigidly exercised when he thought he had reason on his side.

4. It was felt to be the most important duty of the Editor to endeavour on all occasions to obtain the best articles, by securing, if possible, not merely the best writers, but writers who, from circumstances, were specially qualified as judges on the subjects of the respective works. This end it was impossible always to attain in a perfect manner, but assuredly no pains were spared to attain it. It was on this principle that, although some of the Editor's literary friends were more or less constantly contributing to the Review, there was nothing like a fixed band of contributors, to whom a certain quantity of work was habitually to be intrusted, and who were alike ready and qualified for all sorts of books and all sorts of subjects. In proof of this rule being constantly attended to, it may be mentioned that the lists of the writers in any two consecutive numbers differed greatly, that the average number of contributors to a single number varied from twelve to twenty, while the total number of gentlemen engaged in writing for the Review at one time, say within the period of a year, was forty or fifty, and sometimes more than this.

5. It was an invariable rule with the Editor to read over carefully the MSS. of all the articles,* and it was always an understanding formally come to between him and the contributors, that he should reserve to himself the power of altering articles, by omission, addition, or otherwise, and even to reject them altogether. It is but justice to his literary friends here to state, that there were rarely any difficulties interposed in the way of alterations: the total rejection of an article was a very rare occurrence.

6. Partly from an indisposition to engage in compositions requiring much labour of thought or much continuous study, and partly from want of time, the Editor rarely supplied any elaborate articles from his own pen. By thus saving his time at the expense of his money (as indolent men are wont), he was not only enabled to perform his common editorial duties more effectually, but he was left much more in the condition of an impartial judge of the articles that came under his examination, than he possibly could have been had many of these been his own. He can sincerely and without any affectation add his conviction, that, by this abstinence, he consulted not merely the greater impartiality, but the greater excellence in every way, of the writings in the Journal. This disclaimer respecting contributions from his own pen, the Editor intends to apply to the longer or more elaborate articles only; he certainly wrote many brief notices of books, and added or re-wrote passages of more or less length in very many of the longer articles contributed by others.

* To be scrupulously accurate, I should say that articles written by experienced and careful contributors were occasionally not read until they were in proof; but I think I can safely say that the total number of articles so privileged was under twelve during the twelve years.

7. Partly from the fear of being behind the proper day of publication, but principally to allow ample time for the examination of MSS. previously to going to press, it was a general rule with the Editor in making his arrangements, not to consider whether any particular article was wanted for a particular number, but simply whether such an article was desirable for the Review. If it was deemed desirable for the Review, and was found when written to be a good article, it was sure to find a place, some time or other. Besides the advantages mentioned, adherence to this rule had some other excellent consequences, which tended greatly to the comfort of the Editor and his literary friends. Thanks to it, he was seldom under the necessity of hurrying his contributors, and rarely under the necessity of hurrying either himself or the printer; his MS. boxes were so well stored that he was enabled to give out for particular numbers the articles that seemed to him most suitable; and the printer's proofs coming in, as they did, singly and during every week of the year, were read at leisure and without much inconvenience or trouble.*

8. The fortunate state of universal peace that prevailed, and the unlimited intercourse with foreign nations, left no difficulty in the way of procuring publications from abroad; and it is greatly to the credit of the profession that I readily found, in our own country, men capable of conveying to their professional brethren, not merely a full account of the contents of foreign works, but also a sound estimate of their merits as scientific productions. Every number of the Review will attest the truth of this as regards French and German literature, while most of the volumes bear similar witness in respect of the Italian, Danish, and Swedish, as well as of the Greek and Latin. I believe I may safely add that the Review itself was no inconsiderable cause of the greatly-increased knowledge of foreign languages, now so honorably characteristic of the better-educated members of the profession in this country. Many young men, to my knowledge, were stimulated to study the continental languages by the treasures they found extracted from them in the Review; and some of my literary friends, in their zeal to serve me and the Journal, actually set themselves to master some of the stranger tongues, in order that they might supply analyses of the more valuable works written in them.

The first number of the Review was published on the 1st of January, 1836, and the succeeding numbers appeared on the first day of each subsequent quarter, respectively, without one single exception. Indeed, the precision of the arrangements was such that the Number was almost always ready a week or a fortnight before the day of publication. By this means the Journal was enabled to appear on quarter-day in Edinburgh and Dublin, as well as in London. At the suggestion of the publisher, no less than 2250 copies were printed of the first number, 1000 copies being intended for distribution to societies, clubs, editors, &c.; 2000 were printed of the three following numbers; and 1750 of the numbers of the second year: by this time the probable standing sale of the work having been ascertained, the number was, in 1838, reduced to 1500, and has not varied since.

The Review was considered by "the trade," at the close of the first

* During the twelve years the press never stopped—the new number being generally begun to be *printed* before the previous one was *published*. It would be unjust to my excellent printers, Messrs. Adlard, and their intelligent manager, Mr. Tucker, if I did not here express my thanks to them for the uniform readiness and attention they showed in meeting all my wishes respecting the printing of the Review, as well as for their excellent and accurate typography.

year, to be eminently successful, as it very speedily reached an amount of steady sale which had heretofore been regarded as very satisfactory for a periodical work devoted to medical science. It was so considered by myself; and I should be very ungrateful to my numerous and kind friends if I did not take this opportunity of thanking them most cordially for the liberal support extended to the Review during its whole course, but more especially at its commencement. All things considered, the patronage it received was quite as general and extensive as could reasonably be expected. Assuredly, I cannot but regret that the taste for the higher departments of medical science has not been and is not more general in this country; and I take my share of shame, as a member of the profession, that our better literature is not more substantially patronised by those who can afford this patronage; but I am far too sensible of my own deficiencies in this respect, to be in a position to blame others for not buying books—however good—on principle and for the pure love of literature and science. When the taste and the love for such things shall grow stronger and spread wider—and I believe they are now doing so—the gratification of the desire thence arising will follow as a matter of course, and then we shall have patronage of authors and books and journals even to the contentment of the commercial heart itself. In the meanwhile we must not quarrel with what is equally in the natural course of things.

In the table given below, the actual sale of the Review is specified for each year, from the commencement to the close of the work, together with the cost of production, and the total money-produce. From this table it will appear that the Review, regarded as a commercial speculation, was by no means a successful undertaking; as it left the Proprietor very considerably a loser, even while making a present to the concern (as he has done in drawing up the table), not only of the interest of the money expended in the first instance, but of the whole of his editorial labour, and all his own literary contributions. Had I added these latter items to the other items of expenditure, as properly speaking I ought to have done, the balance, on the wrong side, would have been not a little increased. At the very lowest rate of payment that could be thought of for such services, say £50 per number, or £200 per annum, the total balance against the work would have been increased by £2400, and thus raised to £2945 10*s.* instead of £545 10*s.*, as it there stands. It is, however, but fair to deduct from the adverse balance the sum of £500 received by the Proprietor for the copyright of the Review. The stock of books remaining on hand is also worth something; but the amount reckoned as likely to accrue from this will be required to cover the loss expected to be incurred by the publication of the General Index now in hand, and which has always been regarded as an essential portion of the work.

Although in originating the 'British and Foreign Medical Review', I certainly calculated on its eventual success as a commercial undertaking, feeling confident, on general principles, that the superiority of the article intended to be produced, would, in the long run, ensure a decidedly remunerative sale,* I had so many other strong motives for establishing it, and for carrying it on, that the discovery, a few years after

* Some of my original schemes respecting the Review were very grand. Had its commercial success justified the outlay, it was one of my plans to have a German translation of it made in Hamburgh, for circulation among the German states, and also a reprint of it in America. This last has been done lately by an American publisher at his own cost.

its first publication, that it entailed a loss instead of a gain, interfered in no degree with my determination to persevere in the undertaking, or with the zeal or satisfaction—I might indeed say the delight—with which I continued my labours. It was only on attaining that great milestone of man's life, THREESCORE, that I began to think it would be as well to relinquish a pursuit which had for twelve years absorbed much of my time and my best faculties, and to look round me and see whether some "pastures new" could not be found which might advantageously take the place of the old. I felt the remonstrance of Lucretius's NATURA to be almost as applicable to myself as to his Man in general on a more solemn occasion:

> —— "Si grata fuit tibi vita anteacta priorque
> Et non omnia pertusum congesta quasi in vas
> Commoda perfluere atque ingrata interiere,
> Cur non ut plenus vitæ conviva recedis?"

And when I felt, in addition, that the task I was engaged in was in no respect compulsory, and that all considerations of a pecuniary kind were decidedly against its continuance, my mind was soon made up as to the expediency of a change. Accordingly, the discontinuance of the Journal was no sooner thought of than decided on. Up to the time in which this decision was come to (in January last), I can honestly say that my exertions in the cause of the Journal, such as they were, had undergone no diminution, and that I had lost none of the feelings of satisfaction which usually accompanied them; and it is with real comfort that I am able to add, in now closing my editorial labours, that the same feelings have continued to attend me up to this, the last scene of the little history which I am now, rather too egotistically I fear, obtruding on my reader's notice.

But although commercially a failure, in a literary and scientific point of view the 'British and Foreign Medical Review' must be regarded as a very successful publication. Its home circulation, compared with that of other quarterly and monthly medical publications, was large, and it had a much greater circulation abroad than any of them. It failed as a commercial undertaking, not so much because the extent of its sale was actually small for a medical publication, but because its cost of production, so much beyond that of any other medical journal, was disproportioned to this sale. In every other respect, *except this of sale*, the success of the Review was great, and entirely fulfilled every sober hope and expectation entertained at its establishment.

And here, while speaking of the literary and scientific merits of the Review, I beg to remind the reader of a statement made above as to the very small share I myself had in the actual composition of its contents: so small, indeed, was my share, that I consider myself entitled to pass a judgment on the merits of the whole work, with almost the same freedom as if I had had no connexion with it. And the delicacy I might feel on this head is yet further removed by the two following considerations—first, that I should not otherwise be able to do justice to the excellent friends who did write the Journal; and, secondly, that opinions of fully as favorable a character as those I entertain, have been expressed to me, hundreds of times, in speech and by letter, by most competent judges in this and other countries. I therefore do not hesitate to repeat that the success of the Review was in every respect great—I might indeed say triumphant—except as a mere money-making speculation. It succeeded in establishing, for the first time, in medical literature, a high critical tri-

bunal analogous to those which had previously conferred so great benefits on general literature. It investigated the literary as well as the scientific merits of books, and boldly pronounced its opinions of them, whether favorable or unfavorable, without regard to the name or station of the authors. It made the medical profession of this country as familiar with the productions of foreign countries as with their own. It allowed no improvement, or discovery, or new fact of interest or curiosity to be promulgated in any part of the world, without conveying it speedily to its readers. In giving its judgments, it was altogether beyond the influence of authors or publishers, or of any individuals whatever; and if its judgments were not always right, they were never prompted by unworthy motives. A few respectable individuals intolerant of fair criticism from overweening vanity or self-conceit, several weak men whom nature never intended to meddle with the pen, and sundry intra-professional quacks of various rank and complexion, have occasionally attempted to raise the cry of partiality and injustice against its decisions; but I here boldly appeal to all disinterested men and good judges, whether, taken as a whole, the opinions promulgated, and the critical awards made by the writers in the Review, from its beginning to its termination, have not been remarkable for their honesty, accuracy, and justice. That they were *always right*, no man of common sense could for a moment pretend; but that they were as nearly right as the means at the Editor's command could make them, is confidently asserted. As consequences of this character, it is still further maintained—*firstly* (in reference to the past), that the Review has of late years been regarded by the best-informed members of the profession, in this and other countries, as the highest authority on medical subjects; and, *secondly* (in reference to the future), that, by showing what a learned, independent, and liberal review is and can effect, it has not only prevented, in time to come, works of inferior character and merit from finding acceptance with the profession in this country, but has assured the perpetuity of a work or works of the same high stamp as itself.

After thus distinctly and deliberately committing myself to maintain for the Review a character of such comparative excellence, my readers may naturally expect from me some explanation of the fact that the circulation of the work did not become more general. It appears from the table, given below, that the highest quarterly sales (including *back* numbers) were for the years 1840 and 1841, amounting, on the average, to 1415 and 1410 respectively; but the sale of the current numbers—in other words, *the circulation* of the Review—was considerably below this, never reaching to 1400. Premising that the amount of *sale*, and the amount of *circulation* of a journal, are not always or generally the same thing, I think a good many reasons may be given, some weaker, some stronger, why it happened, that the sale of the 'British and Foreign Medical Review' was not greater.

1. Since the introduction of the lighter medical publications—the weekly journals—the larger periodical publications have generally declined in circulation, and perhaps the quarterly, as more expensive, more than the others. Many persons are naturally tempted by the small cost of the weekly journals, by the attraction of a quickly-recurring publication, by the facility of conveyance by post, by the gossip and news, &c. constituting an essential character of such publications, to become subscribers to one or other of them; and either from economy, or other motives,

cease to take in any other journal. *Retrospects* and *Abstracts,* lately published in a compendious form half-yearly, have still farther encroached on the circulation of the larger journals.

2. The establishment of medical book clubs or reading societies throughout the kingdom, decidedly more numerous of late years, has very considerably affected the sale of the larger journals. In these clubs, established in almost every town, generally speaking only a single copy of a journal is usually taken, and this is made to supply the whole society, including, it may be, ten, twenty, or more members. No doubt these clubs create a sure demand for a very considerable number of copies of the various journals published; but as in most of them *all* the journals are generally ordered, as a matter of course, no room is left for the operation of special preference founded on the individual merit of the publications. The number of persons thus enabled to *see* the medical journals is no doubt large; and the casual, temporary, or late sight of them thus afforded, is sufficient to satisfy the curiosity of a majority of readers.

3. Zealous young literary men, especially young editors or projectors of Journals, entertain a very exaggerated opinion of the actual number of readers of medical journals or other medical books in this country. Feeling how interested they themselves are about improvements and novelties in medicine, or about medical progress generally, they are apt to imagine that most other men think as they do; altogether overlooking the large proportion of practitioners who never cared about such things, and the equally large proportion who cared about them when young and comparatively idle, but whose attention the progress of years and active professional occupation have turned entirely into other channels. If the inquiry were made, I have little doubt that it would be found to be a fact, that a considerable proportion of the most eminent and best employed physicians and surgeons in the kingdom rarely buy any medical books, and still more rarely any journals. If they see the latter at all, they are contented with an occasional glance of them in their public or private clubs and societies.

It forms no part of my present business to explain or account for this singular disregard of medical literature by those best qualified to appreciate it and to promote it; but it is a circumstance, as already hinted at, not a little discreditable to the profession in this country. Yet who shall throw the first stone?

4. To the class of persons just mentioned, the expense of books is a matter of no moment, and is probably seldom considered by them; but it is certain that of the total number of practitioners in this country, there is a large proportion to whom the annual cost of a journal is a matter of consideration, sufficient, certainly, to preclude all idea of purchasing a second, if the expense of one has been already incurred.

The preceding explanations are applicable to journals generally: the following have more especial reference to the 'British and Foreign Medical Review.'

5. At the time the 'British and Foreign Medical Review' was established, in England the field was occupied by a Quarterly Journal having a large circulation. The great majority of the subscribers to this journal would naturally continue to take it; only a small proportion would take both journals, or would replace the old by the new. Even if the new journal were considered superior to the other, the substitution of the one for the other would not be a quick process. It is, nevertheless, well known that the establishment

of the 'British and Foreign Medical Review' had a very unfavorable influence on the sale of its rival; as this sale certainly decreased after the appearance of the new journal, and fell at length to an amount considerably below that of its opponent. The existence of the two journals was thus mutually injurious in point of circulation: the competition, however, was doubtless valuable in other respects.

6. The very excellencies of the 'British and Foreign Medical Review,'—those high qualities which obtained for it the suffrages of the best judges,—were direct obstacles to its success with a large portion of the medical profession. It was pitched in too high a strain for the less educated practitioners. Its physiological and philosophical discussions, its investigation of principles, its generalizations, its correct but abstract and philosophical style, were, in a great measure, thrown away on this class of readers; they desiderated something lower, humbler, simpler; something they could at once grasp and understand. The mere "practical men" also,—men who look upon medical literature as a sort of manufactory, storehouse, shop, or waggon, for investigating, detecting, promulgating, and conveying "improvements in practice," by which they mean the announcement of a new remedy, or a modification of an old one, or its new or improved application in some unusual case, &c. &c.; in a word, some fresh addition to the already boundless stock of empirical conventionalities,—were sadly disappointed at finding in its pages general principles taking the place of individual cases, and therapeutical doctrines giving the go-by to bran-new specifics and charming formulæ without a chemical or pharmaceutical defect. I was early warned by many kind friends of the necessity of consulting the tastes of the classes just referred to; but I preferred to adhere to the fundamental principles of the Journal, knowing that these were in reality the *only true practical* principles, and hoping that the progress of years might bring greater intellectual capacity and more general enlightenment; or even that the Review might itself cure the evil from which it was meanwhile a sufferer. It has always been held expedient by the best judges to pitch the general tone of instruction rather above than below the capacity of the learners in any department of science, and I felt it my duty to adopt this rule. At the present moment I am far from regretting that such was my determination: I am sure the general result on the professional mind has been most beneficial: many individual instances of such benefit have come under my own observation.

7. Those who are accustomed to observe in ordinary life the wonderfully conciliating effect of praise and flattery, will be prepared to find these still more active in the world of letters, where, as is well known, vanity and the desire of literary and professional reputation are moving powers of the first order. It is accordingly familiar to the initiated how much all authors, but more especially those of the mediocre sort, are influenced by criticism of their writings. By weak men of this class, every commendatory or eulogistic notice of their works is set down as evidence of the critic's judgment, while every unfavorable report is attributed to ignorance, prejudice, or to some malignant motive. Now, when it is considered how very considerable a proportion of the medical profession in this country come forward in the present day, either as the writers of distinct works, or as the authors of communications in the numerous journals, it will be seen that the generally favorable or unfavorable *animus* of this class towards a journal, might thus have a consider-

able influence on its circulation. A man who is just hesitating whether he shall take-in a certain journal or not, may very likely be influenced *pro* or *con* by a favorable or unfavorable notice of his book or paper at the time; and a certain portion of the more sensitive will even be led by a severe though just criticism, to throw up the journal previously subscribed for. This is a fact well known to publishers. So well was this principle of action known to an editor whom I once knew, and so fully was its importance appreciated by him, that he adopted the system of general laudation as a commercial principle, and with great success. Now, in looking for the causes of the comparatively small sale of the 'British and Foreign Medical Review,' it is probably fair to reckon what has been called its *severity*, but which I call its impartiality and justice, as one of them. It is undoubtedly true that in its pages, quacks, cheats, pretenders of all sorts, and ignorant and illiterate writers of all ranks found little or no mercy; while even men of eminence were often severely criticised, without reference to their position in letters or station in society, but merely in relation to the actual merits or demerits of their books. It is equally true that never were there lavished in any journal more cordial and sincere commendations than were bestowed on works of real merit; but even in these cases, fair criticism, or even partial blame, was never withheld when it was thought due. Some instances of the curious effect of such mixed or neutralizing criticism on the minds of authors have come to my knowledge. And more than once, in the course of the publication of the Review, I have had an amusing illustration of the different way in which the same mind will view a criticism when directed at the production of another and at its own. I refer to the case in which a contributor has himself afterwards published a book. When this book has been reviewed by one of his colleagues, and undergone gentle criticism—it might possibly be somewhat more gentle on account of the previous relationship of the author to the Review—it has been amusing to see the quondam contributor exhibit the same sensitiveness to reproof as if he had never bestowed any on others; entirely forgetting how he himself had formerly laughed at (in others) the very feelings he now indulged as legitimate!

8. It appears from the Table that there has been a slight falling off in the sale during the last and present year. The greater part of this is easily explained by the fact that last year the Review began to be reprinted in America; consequently there has been a considerable decrease in the number of copies sent out to the United States. I think it likely, also, that a portion of the defalcation may be caused by the cry raised by some of the weekly Journals—pretty loud for a month or two—against certain articles in the Review on the subject of Homœopathy and Hydropathy.* These articles, while especially suited to the taste and judgment of the more philosophical members of the profession,—by whom, indeed, they were generally commended,—were likely enough to be misunderstood by the ill-informed generally, and especially by the numerous class of routineers whose knowledge of their profession is mainly empirical, and whose ideas of practice are almost entirely limited to the conventional administration of drugs. In no instance since the publication of the Review have I had more cause to be satisfied with the effect produced by any articles, than by these. In no case have I received such numerous, strong,

* The same cry will probably be raised against the excellent article on *Teetotalism* in the present number.

and cordial acknowledgments of the services rendered to medicine by the Review, as I have received and continue to receive on account of those articles, from all countries, and from the best men in them. I believe, indeed, that they have been the means of exciting a spirit of philosophical inquiry into the present state of therapeutics, which cannot fail to end in consequences most beneficial to medical science. It was amusing to observe, that while the weekly journals of general medicine were attacking these papers as *favoring* homœopathy (of course the writers of the attacks had never redd them), the Journals of Homœopathy were universally denouncing them as opposing it, and the great champions of the system of infinitesimals honoured them with numerous replies, and so-called refutations in the shape of pamphlets, letters, &c.

The various causes just enumerated seem to me satisfactorily to explain the comparatively limited sale of the Review, and its failure as a commercial speculation. Of course, there still remains *another* view of the matter, which those who choose may adopt for explaining the fact I have attempted to explain in my own way. It may be said that the Review failed because it *deserved* to fail—because, instead of being, as pretended, a very good commercial article, it was a very bad commercial article, and consequently could not and did not succeed commercially. As I have already committed myself on this question, I cannot here re-argue it. I leave it with confidence in the hands of honest men and competent judges.

As one of my principal objects in making the present exposition, is to aid future journalists in their attempts to serve the profession, I think it right to make one more observation on the financial part of the subject, before concluding. It must not be supposed from anything I have said, or from the inspection of the abstract of my accounts given below, that the financial arrangements of the Journal were heedlessly made or carelessly superintended. This was not the case. I believe that the sums paid to the printer, the paper-maker, and the publisher were really as moderate as they well could be, in fairness between man and man, regard being had to the quality of the materials and the work. The only thing in which a saving in these items might have been occasionally made, was *the size* of the different numbers. Eighteen sheets, or 288 pages, was the covenanted size of each number. It frequently happened, however, that when there was a pressure of what seemed interesting or important matter, the fixed limits were transgressed by the addition of one, one and a half, or even two sheets.* There was certainly no absolute necessity for this increase of bulk; and I dare say a rigid adherence to the original commercial covenant might have saved the proprietor about £40 or £50 annually. This is a point worth consideration for future proprietors and editors.

On the remuneration to contributors also, no doubt a saving *might* have been made. Small as my rate of payment was, compared with that received by contributors to respectable journals of general literature, still it was much beyond what had been customary in medical literature; and I dare say that if, on finding the Journal unprofitable, I had made the proposition to my literary friends to accept less for their labours, they would have readily agreed to it, as I am proud to say I always found them, to a man, influenced in their relations to the Journal by much higher considerations than those

* The average excess on the whole work was about one sheet and a half per volume, including the tables of contents, title-pages, and indexes. The present number contains twenty sheets.

of a pecuniary kind. By reducing the *honorarium* from six to five pounds per sheet, I might thus have saved from £60 to £70 per annum, which added to the saving just noticed, as practicable in paper and print, would have benefited the exchequer of the proprietary a good deal. I confess, however, that it never occurred to me to make this reduction; and I should be grieved to think that the future state of the medical press should render such a reduction necessary. Assuredly, the honorarium allowed by me was quite as small as accomplished scholars and experienced writers, like my literary friends, ought to accept; and was very inadequate remuneration for the time, and pains, and thought bestowed by many of them on their compositions. Still it is clear from the financial exposition now made, that one of two things must happen in future: either the profession in this country must be contented with an article produced at less cost,—probably, therefore, an inferior article,—or they must determine to become more extensively *purchasers* as well as *readers* of journals, if they will have them *good*, that is, produced at a certain expenditure.*

Although, in the preceding pages, I have had occasion more than once to

* A simple and ready way of augmenting the sale of journals, with little individual sacrifice, would be for each medical society or book-club to take two copies instead of one, of *good* journals. I know, from experience, how very *convenient* such an arrangement would be to the members of the societies generally; and its advantages in the way just hinted at are so obvious, that I cannot but hope that the hint may be taken. I would not have the rule made universal in regard to journals, but only applicable as to those which the members might deem deserving the distinction. Surely many large societies and large book-clubs might take, with great advantage to their members, not merely two, but three or four copies of some journals. The difference thus produced in the sale of a journal (probably 400 or 500 copies) might make all the difference between a journal struggling for existence and a journal highly prosperous as a commercial undertaking.

Account of the EXPENDITURE *and* RECEIPTS *of the*

EXPENDITURE.

	1836	1837	1838	1839	1840
	£ s. d.	£ s. d.	£ s. d.	£ s. d.	£ s. d.
Paper, Printing, Engraving, Stitching	1166 1 1	745 10 1	659 0 10	666 4 11	742 9 3
Advertising	49 17 6½	18 18 0	63 16 3	95 17 6	39 15 6
Books for Review (purchased)	71 11 4	18 7 8	48 16 0	44 0 2	49 7 10
Contributors	460 13 6	316 18 6	362 10 0	379 7 6	389 2 6
Miscellaneous, including Carriage, Postage, &c.	125 12 0	108 0 5	48 10 6½	70 17 6	79 4 7
Total Expenditure	1873 15 5½	1207 14 8	1182 13 7½	1256 7 7	1299 19 8

RECEIPTS.

For Books sold	812 9 4	817 17 6	984 0 4	1161 12 4	1187 6 10
Advertisements and Bills	121 6 0	104 8 6	163 18 6	130 15 0	139 12 0
Total Receipts	933 15 4	922 6 0	1147 18 10	1292 7 4	1326 18 10
*Total number of copies sold each year, *including back numbers*	3900	3985	4598	5573	5663
*Average number of copies sold quarterly each year, *including back numbers*	975	996	1149	1393	1415

* The numbers in these two lines show the total actual sale of the Journal for each year, and the total average sale fo each quarter; but they do not, by any means, give a true representation of the actual and relative sale of the *curren numbers*, or what is usually termed the *circulation* of the Journal. Generally speaking, the numbers there given ar

refer to the pleasing relations existing between the Editor and the Contributors to the Journal, I should ill satisfy my feelings if I took leave of them without some more distinct acknowledgment of the claims they possess to my gratitude and sincere regard. It was no common fortune to occupy, for a series of years, a position which necessarily placed me in constant and close literary and personal intercourse with many of the most learned and intellectual members of the profession; which made me, for the time being, the prompter and, to a certain extent, the director of their studies, —the witness and, in a small degree, the sharer of their labours; and in which it was my especial function to communicate the fruits of these studies and labours to the medical world at large. It would be strange if the privileges and advantages of such a position could be even partially resigned without some pain and some regret. The parting with my late associates in their capacity of contributors, however, would be very different from what it now is, if I thought I was at the same time taking leave of them as friends and friendly correspondents. But I am thankful to say this is not so. It is most gratifying to me to believe that the breaking of the bond which constituted our formal literary union, leaves still untouched those nearer and dearer ties which gave to our official intercourse so cordial a character, and made the years devoted to it among the happiest as well as the most useful of my life:

"nec me meminisse pigebit——
Dum memor ipse mei, dum spiritus hos reget artus."

JOHN FORBES.

Old Burlington Street;
30*th September*, 1847.

British and Foreign Medical Review *from the Commencement.*

EXPENDITURE.

1841			1842			1843			1844			1845			1846			1847			Total.		
£	s.	d.	£	s.	d.	£	s.	d.	£	s.	d.	£	s.	d.	£	s.	d.	£	s.	d.	£	s.	d.
604	1	7	560	5	6	622	7	3	516	8	6	594	17	2	580	2	0	567	18	9	8025	6	11
37	1	1	25	7	0	23	14	0	19	6	6	75	2	6	28	9	0	29	0	6	506	5	4½
33	4	5½	42	1	3	38	14	11	27	16	6	14	10	0	25	1	5	10	6	10	423	18	4½
367	7	6	387	10	6	380	18	6	424	14	0	427	10	6	369	19	0	401	12	0	4668	4	0
9	4	9	2	7	0	2	17	3	1	5	0	1	4	0	1	3	0	1	10	0	451	16	0½
1050	19	4½	1017	11	3	1068	11	11	989	10	6	1113	4	2	1004	14	5	1010	8	1	14075	10	8½

RECEIPTS.

1184	11	3	1057	0	7	1039	3	2	1067	8	1	1014	6	8	943	10	10	866	3	9	12135	10	8
134	2	0	100	0	0	100	0	0	100	0	0	100	0	0	100	0	0	100	0	0	1394	2	0
1318	13	3	1157	0	7	1139	3	2	1167	8	1	1114	6	8	1043	10	10	966	3	9	13529	12	8
5643			5451			5285			5481			5107			4919			4451					
1410			1363			1321			1370			1276			1229			1112					

greater than the current sale; and they are irregularly so, the amount of *back* numbers sold being much greater in some years than others. The sale of back numbers has considerably decreased during the last few years, which makes the diminution of the *circulation* during these years, appear much greater than it really has been.

P.S. Although the 'British and Foreign Medical Review,' as above stated, literally and absolutely closes its career with the present Number, I feel great satisfaction in being able to announce that it will be immediately succeeded by another Journal, which, I am given to understand, will be conducted on nearly the same plan and principles. The new Journal will have one great commercial advantage over the old, in having no English rival for the public favour, as it is arranged that there shall be an amalgamation of the two London Quarterly Journals into one, under the title of the 'BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.' And I am informed by my late excellent publisher,* the joint proprietor of the new Journal, that the same independent and liberal course, both as to criticism and literary remuneration, will be pursued as in the case of the Journal he so long published on my account. I have also reason to believe that many of the best writers in my Journal will be contributors to that which is to take its place. Although I shall not cease to feel great interest in the character and position of the new Journal, I think it right to state here, most explicitly, that I have and shall have no connexion whatever with it, as regards its proprietorship, composition, editing, or publishing, or in any other way, directly or indirectly. I sincerely hope for the honour and welfare of the profession, that it will be at least a GOOD JOURNAL; and I can conscientiously add, that if it shall prove to be a BETTER JOURNAL than that which I was able to supply (and with the experience of its predecessors to guide it, showing it as well what to imitate and what to eschew, it may reasonably be expected to be better), there will not be one among its readers who will more cordially rejoice than myself.

I hope my successor in the editorial chair, whoever he may be, will pardon me if I venture to offer him, as a parting legacy, a little advice for his guidance in his new and "Great Place." Although not so intended originally, these precepts are, like all that fell from the pen of the wise man who wrote them, of such universal application as to seem to be addressed specially to an Editor assuming office. At any rate, I am sure the Editor of the new Journal cannot but profit by giving heed to them.

"In the discharge of THE PLACE, set before thee the best examples; for Imitation is a globe of precepts. And after a time set before thee thine own example, and examine thyself strictly whether thou didst not best at first. Neglect not also the examples of those that have carried themselves ill in the same place; not to set off thyself by taxing their memory, but to direct thyself what to avoid. Reform, therefore, without bravery or scandal of former times and persons; but yet set it down to thyself, as well to create good precedents as to follow them. Reduce things to the first institution, and observe wherein and how they have degenerated; but yet ask counsel of both Times, of the ancienter Time what is best, and of the latter Time what is fittest. Seek to make thy course regular, that men may know beforehand what they may expect: but be not too positive and peremptory; and express thyself well when thou digressest from thy rule. Preserve the Right of thy Place, but stir not questions of jurisdiction; and rather assume thy right in silence and *de facto*, than voice it with claims and challenges. Preserve likewise the rights of Inferior Places; and think it more honour to direct in chief than to be busie in all. Embrace and invite helps and advices touching the execution of thy Place; and do not drive away such as bring thee information, as meddlers, but accept of them in good part. . . . . Use the memory of thy Predecessor fairly and tenderly; for if thou dost not, it is a debt will sure be paid when thou art gone. If thou have Colleagues respect them, and rather call them when they look not for it than exclude them when they have reason to be looked to be called."
—BACON, *Of Great Place.*

J. F.

* I cannot let this last opportunity pass without expressing my sincere thanks to Mr. Churchill for his uniform liberality and kindness, and his great attention to the affairs of the Journal.

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